FIEC

Raising Florida's Minimum Wage

18-01

2019

Financial Impact Estimating Conference

Raising Florida's Minimum Wage
Serial Number 18-01

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- Minimum Wage and Real Wage Inequality: Evidence from Pass-Through to Retail Prices
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Tab 1

Authorization

FLORIDA DEPARTMENT *of* STATE

RECEIVED 3-8-19 Carolyn Coleman

RON DESANTIS
Governor

LAUREL LEE
Secretary of State

March 8, 2019

Financial Impact Estimating Conference
c/o Amy Baker, Coordinator
Office of Economic and Demographic Research
111 West Madison Street, Ste. 574
Tallahassee, Florida 32399-6588

Dear Ms. Baker:

Section 15.21, Florida Statutes, provides that the Secretary of State shall submit an initiative petition to the Financial Impact Estimating Conference when a sponsoring political committee has met the registration, petition form submission and signature criteria set forth in that section.

The criteria in section 15.21, Florida Statutes, has now been met for the initiative petition titled **Raising Florida's Minimum Wage**, Serial Number **18-01**. Therefore, I am submitting the proposed constitutional amendment petition form, along with a status update for the initiative petition, and a chart that provides a statewide signature count and count by congressional districts.

Sincerely,

Laurel M Lee

Laurel Lee
Secretary of State

LL/am/ljr

pc: John Morgan, Chairperson, Florida For A Fair Wage

Enclosures

R.A. Gray Building • 500 South Bronough Street • Tallahassee, Florida 32399
850.245.6500 • 850.245.6125 (Fax) • DOS.MyFlorida.com

CONSTITUTIONAL AMENDMENT PETITION FORM

Note:
- All information on this form, including your signature, becomes a public record upon receipt by the Supervisor of Elections.
- Under Florida law, it is a first degree misdemeanor, punishable as provided in s. 775.082 or s. 775.08, Florida Statutes, to knowingly sign more than one petition for an issue. [Section 104.185, Florida Statutes].
- If all requested information on this form is not completed, the form will not be valid.

Your name____
Please print name as it appears on your Voter Information Card

Your address______________________________________

City __________________ Zip____________ County________________

Voter Registration Number____________________ **OR** Date of Birth __________________

☐ Please change my <u>legal residence</u> address on my voter registration record to the above <u>residence</u> address (check box, if applicable).

I am a registered voter of Florida and hereby petition the Secretary of State to place the following proposed amendment to the Florida Constitution on the ballot in the general election:

BALLOT TITLE: Raising Florida's Minimum Wage

BALLOT SUMMARY: Raises minimum wage to $10.00 per hour effective September 30th, 2021. Each September 30th thereafter, minimum wage shall increase by $1.00 per hour until the minimum wage reaches $15.00 per hour on September 30th, 2026. From that point forward, future minimum wage increases shall revert to being adjusted annually for inflation starting September 30th, 2027.

ARTICLE AND SECTION BEING AMENDED OR CREATED: Article X, Section 24

Full text of proposed constitutional amendment is as follows:

ARTICLE X, SECTION 24. Florida minimum wage.—

(c) MINIMUM WAGE. Employers shall pay Employees Wages no less than the Minimum Wage for all hours worked in Florida. Six months after enactment, the Minimum Wage shall be established at an hourly rate of $6.15. <u>Effective September 30th, 2021, the existing state Minimum Wage shall increase to $10.00 per hour, and then increase each September 30th thereafter by $1.00 per hour, until the Minimum Wage reaches $15.00 per hour on September 30th, 2026.</u> On September 30th of <u>2027</u> ~~that year~~ and on each following September 30th, the state Agency for Workforce Innovation shall calculate an adjusted Minimum Wage rate by increasing the current Minimum Wage rate by the rate of inflation during the twelve months prior to each September 1st using the consumer price index for urban wage earners and clerical workers, CPI-W, or a successor index as calculated by the United States Department of Labor. Each adjusted Minimum Wage rate calculated shall be published and take effect on the following January 1st. For tipped Employees meeting eligibility requirements for the tip credit under the FLSA, Employers may credit towards satisfaction of the Minimum Wage tips up to the amount of the allowable FLSA tip credit in 2003.

________________ X____________________________

DATE OF SIGNATURE **SIGNATURE OF REGISTERED VOTER**

Initiative petition sponsored by Florida for a Fair Wage, 6619 S. Dixie Highway, #148, Miami, FL 33143

If paid petition circulator is used:

____

Circulator's name

____

Circulator's address

RETURN TO:

Florida for a Fair Wage
6619 S. Dixie Highway, #148
Miami, FL 33143

For official use only: *Serial number* 18-01
Date approved 1/10/2018
Revised 4/17/2018

Attachment for Initiative Petition

Raising Florida's Minimum Wage
Serial Number 18-01

1. **Name and address of the sponsor of the initiative petition:**
   John Morgan, Chairperson
   Florida For A Fair Wage
   20 North Orange Avenue
   Suite 1600
   Orlando, FL 32801

2. **Name and address of the sponsor's attorney, if the sponsor is represented:** Unknown

3. **A statement as to whether the sponsor has obtained the requisite number of signatures on the initiative petition to have the proposed amendment put on the ballot:** As of March 8, 2019, the sponsor has not obtained the requisite number of signatures to have the proposed amendment placed on the ballot. A total of 766,200 valid signatures are required for placement on the 2020 general election ballot.

4. **If the sponsor has not obtained the requisite number of signatures on the initiative petition to have the proposed amendment put on the ballot, the current status of the signature-collection process:** As of March 8, 2019, Supervisors of Elections have certified a total of 87,528 valid petition signatures to the Division of Elections for this initiative petition. This number represents more than 10% of the total number of valid signatures needed from electors statewide and in at least one-fourth of the congressional districts in order to have the initiative placed on the 2020 general election ballot.

5. **The date of the election during which the sponsor is planning to submit the proposed amendment to the voters:** Unknown. The earliest date of election that this proposed amendment can be placed on the ballot is November 3, 2020, provided the sponsor successfully obtains the requisite number of valid signatures by February 1, 2020.

6. **The last possible date that the ballot for the target election can be printed in order to be ready for the election:** Unknown

7. **A statement identifying the date by which the Financial Impact Statement will be filed, if the Financial Impact Statement is not filed concurrently with the request:** The Secretary of State forwarded a letter to the Financial Impact Estimating Conference in the care of the coordinator on March 8, 2019.

8. **The names and complete mailing addresses of all of the parties who are to be served:** This information is unknown at this time.

FLORIDA DEPARTMENT OF STATE
DIVISION OF ELECTIONS

SUMMARY OF PETITION SIGNATURES

Political Committee: **Florida For A Fair Wage**

Amendment Title: **Raising Florida's Minimum Wage**

Congressional District	Voting Electors in 2016 Presidential Election	**For Review** 10% of 8% Required By Section 15.21 Florida Statutes	**For Ballot** 8% Required By Article XI, Section 3 Florida Constitution	Signatures Certified
FIRST	386,504	3,093	30,921	81
SECOND	360,098	2,881	28,808	1,406
THIRD	356,715	2,854	28,538	1,836
FOURTH	428,190	3,426	34,256	1,598
FIFTH	316,115	2,529	25,290	6,044
SIXTH	385,918	3,088	30,874	7,073
SEVENTH	370,466	2,964	29,638	6,049
EIGHTH	409,569	3,277	32,766	3,969
NINTH	362,593	2,901	29,008	4,191
TENTH	320,548	2,565	25,644	6,790
ELEVENTH	417,253	3,339	33,381	2,836
TWELFTH	386,775	3,095	30,942	3,902
THIRTEENTH	367,818	2,943	29,426	4,546
FOURTEENTH	336,289	2,691	26,904	7,884
FIFTEENTH	340,331	2,723	27,227	3,243
SIXTEENTH	403,805	3,231	32,305	4,915
SEVENTEENTH	360,061	2,881	28,805	798
EIGHTEENTH	388,772	3,111	31,102	643
NINETEENTH	389,415	3,116	31,154	446
TWENTIETH	291,984	2,336	23,359	5,457
TWENTY-FIRST	355,842	2,847	28,468	961
TWENTY-SECOND	361,305	2,891	28,905	3,872
TWENTY-THIRD	342,784	2,743	27,423	4,238
TWENTY-FOURTH	269,446	2,156	21,556	2,969
TWENTY-FIFTH	269,983	2,160	21,599	421
TWENTY-SIXTH	294,742	2,358	23,580	689
TWENTY-SEVENTH	304,012	2,433	24,321	671
TOTAL:	**9,577,333**	**76,632**	**766,200**	**87,528**

Date: 3/8/2019 11:50:49 AM

CONSTITUTIONAL AMENDMENT PETITION FORM

Note:
• All information on this form, including your signature, becomes a public record upon receipt by the Supervisor of Elections.
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Your name____
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Your address____

City ______________________Zip_____________County_____________________

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☐ Please change my legal residence address on my voter registration record to the above residence address (check box, if applicable).

I am a registered voter of Florida and hereby petition the Secretary of State to place the following proposed amendment to the Florida Constitution on the ballot in the general election:

BALLOT TITLE: Raising Florida's Minimum Wage

BALLOT SUMMARY: Raises minimum wage to $10.00 per hour effective September 30th, 2021. Each September 30th thereafter, minimum wage shall increase by $1.00 per hour until the minimum wage reaches $15.00 per hour on September 30th, 2026. From that point forward, future minimum wage increases shall revert to being adjusted annually for inflation starting September 30th, 2027.

ARTICLE AND SECTION BEING AMENDED OR CREATED: Article X, Section 24

Full text of proposed constitutional amendment is as follows:

ARTICLE X, SECTION 24. Florida minimum wage.—

(c) MINIMUM WAGE. Employers shall pay Employees Wages no less than the Minimum Wage for all hours worked in Florida. Six months after enactment, the Minimum Wage shall be established at an hourly rate of $6.15. <u>Effective September 30th, 2021, the existing state Minimum Wage shall increase to $10.00 per hour, and then increase each September 30th thereafter by $1.00 per hour, until the Minimum Wage reaches $15.00 per hour on September 30th, 2026.</u> On September 30th of <u>2027</u> ~~that year~~ and on each following September 30th, the state Agency for Workforce Innovation shall calculate an adjusted Minimum Wage rate by increasing the current Minimum Wage rate by the rate of inflation during the twelve months prior to each September 1st using the consumer price index for urban wage earners and clerical workers, CPI-W, or a successor index as calculated by the United States Department of Labor. Each adjusted Minimum Wage rate calculated shall be published and take effect on the following January 1st. For tipped Employees meeting eligibility requirements for the tip credit under the FLSA, Employers may credit towards satisfaction of the Minimum Wage tips up to the amount of the allowable FLSA tip credit in 2003.

________________________ X____

DATE OF SIGNATURE **SIGNATURE OF REGISTERED VOTER**

Initiative petition sponsored by Florida for a Fair Wage, 6619 S. Dixie Highway, #148, Miami, FL 33143

If paid petition circulator is used:

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Circulator's name

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Circulator's address

RETURN TO:

Florida for a Fair Wage
6619 S. Dixie Highway, #148
Miami, FL 33143

For official use only: *Serial number* 18-01
Date approved 1/10/2018
Revised 4/17/2018

Tab 2

Current Law

SECTION 24. Florida minimum wage.—

(a) PUBLIC POLICY. All working Floridians are entitled to be paid a minimum wage that is sufficient to provide a decent and healthy life for them and their families, that protects their employers from unfair low-wage competition, and that does not force them to rely on taxpayer-funded public services in order to avoid economic hardship.

(b) DEFINITIONS. As used in this amendment, the terms "Employer," "Employee" and "Wage" shall have the meanings established under the federal Fair Labor Standards Act (FLSA) and its implementing regulations.

(c) MINIMUM WAGE. Employers shall pay Employees Wages no less than the Minimum Wage for all hours worked in Florida. Six months after enactment, the Minimum Wage shall be established at an hourly rate of $6.15. On September 30th of that year and on each following September 30th, the state Agency for Workforce Innovation shall calculate an adjusted Minimum Wage rate by increasing the current Minimum Wage rate by the rate of inflation during the twelve months prior to each September 1st using the consumer price index for urban wage earners and clerical workers, CPI-W, or a successor index as calculated by the United States Department of Labor. Each adjusted Minimum Wage rate calculated shall be published and take effect on the following January 1st. For tipped Employees meeting eligibility requirements for the tip credit under the FLSA, Employers may credit towards satisfaction of the Minimum Wage tips up to the amount of the allowable FLSA tip credit in 2003.

(d) RETALIATION PROHIBITED. It shall be unlawful for an Employer or any other party to discriminate in any manner or take adverse action against any person in retaliation for exercising rights protected under this amendment. Rights protected under this amendment include, but are not limited to, the right to file a complaint or inform any person about any party's alleged noncompliance with this amendment, and the right to inform any person of his or her potential rights under this amendment and to assist him or her in asserting such rights.

(e) ENFORCEMENT. Persons aggrieved by a violation of this amendment may bring a civil action in a court of competent jurisdiction against an Employer or person violating this amendment and, upon prevailing, shall recover the full amount of any back wages unlawfully withheld plus the same amount as liquidated damages, and shall be awarded reasonable attorney's fees and costs. In addition, they shall be entitled to such legal or equitable relief as may be appropriate to remedy the violation including, without limitation, reinstatement in employment and/or injunctive relief. Any Employer or other person found liable for willfully violating this amendment shall also be subject to a fine payable to the state in the amount of $1000.00 for each violation. The state attorney general or other official designated by the state legislature may also bring a civil action to enforce this amendment. Actions to enforce this amendment shall be subject to a statute of limitations of four years or, in the case of willful violations, five years. Such actions may be brought as a class action pursuant to Rule 1.220 of the Florida Rules of Civil Procedure.

(f) ADDITIONAL LEGISLATION, IMPLEMENTATION AND CONSTRUCTION. Implementing legislation is not required in order to enforce this amendment. The state legislature may by statute establish additional remedies or fines for violations of this amendment, raise the applicable Minimum Wage rate, reduce the tip credit, or extend coverage of the Minimum Wage to employers or employees not covered by this amendment. The state legislature may by statute or the state Agency for Workforce Innovation may by regulation adopt any measures appropriate for the implementation of this amendment. This amendment provides for payment of a minimum wage and shall not be construed to preempt or otherwise limit the authority of the state legislature or any other public body to adopt or enforce any other law, regulation, requirement, policy or standard that provides for payment of higher or supplemental wages or benefits, or that extends such protections to employers or employees not covered by this amendment. It is intended that case law, administrative interpretations, and other guiding standards developed under the federal FLSA shall guide the construction of this amendment and any implementing statutes or regulations.

(g) SEVERABILITY. If any part of this amendment, or the application of this amendment to any person or circumstance, is held invalid, the remainder of this amendment, including the application of such part to other persons or circumstances, shall not be affected by such a holding and shall continue in full force and effect. To this end, the parts of this amendment are severable.

History.—Proposed by Initiative Petition filed with the Secretary of State August 7, 2003; adopted 2004.

448.110 State minimum wage; annual wage adjustment; enforcement.—

(1) This section may be cited as the "Florida Minimum Wage Act."

(2) The purpose of this section is to provide measures appropriate for the implementation of s. 24, Art. X of the State Constitution, in accordance with authority granted to the Legislature pursuant to s. 24(f), Art. X of the State Constitution. To implement s. 24, Art. X of the State Constitution, the Department of Economic Opportunity is designated as the state Agency for Workforce Innovation.

(3) Effective May 2, 2005, employers shall pay employees a minimum wage at an hourly rate of $6.15 for all hours worked in Florida. Only those individuals entitled to receive the federal minimum wage under the federal Fair Labor Standards Act and its implementing regulations shall be eligible to receive the state minimum wage pursuant to s. 24, Art. X of the State Constitution and this section. The provisions of ss. 213 and 214 of the federal Fair Labor Standards Act, as interpreted by applicable federal regulations and implemented by the Secretary of Labor, are incorporated herein.

(4)(a) Beginning September 30, 2005, and annually on September 30 thereafter, the Department of Economic Opportunity shall calculate an adjusted state minimum wage rate by increasing the state minimum wage by the rate of inflation for the 12 months prior to September 1. In calculating the adjusted state minimum wage, the Department of Economic Opportunity shall use the Consumer Price Index for Urban Wage Earners and Clerical Workers, not seasonally adjusted, for the South Region or a successor index as calculated by the United States Department of Labor. Each adjusted state minimum wage rate shall take effect on the following January 1, with the initial adjusted minimum wage rate to take effect on January 1, 2006.

(b) The Department of Revenue and the Department of Economic Opportunity shall annually publish the amount of the adjusted state minimum wage and the effective date. Publication shall occur by posting the adjusted state minimum wage rate and the effective date on the Internet home pages of the Department of Economic Opportunity and the Department of Revenue by October 15 of each year. In addition, to the extent funded in the General Appropriations Act, the Department of Economic Opportunity shall provide written notice of the adjusted rate and the effective date of the adjusted state minimum wage to all employers registered in the most current reemployment assistance database. Such notice shall be mailed by November 15 of each year using the addresses included in the database. Employers are responsible for maintaining current address information in the reemployment assistance database. The Department of Economic Opportunity is not responsible for failure to provide notice due to incorrect or incomplete address information in the database. The Department of Economic Opportunity shall provide the Department of Revenue with the adjusted state minimum wage rate information and effective date in a timely manner.

(5) It shall be unlawful for an employer or any other party to discriminate in any manner or take adverse action against any person in retaliation for exercising rights protected pursuant to s. 24, Art. X of the State Constitution. Rights protected include, but are not limited to, the right to file a complaint or inform any person of his or her potential rights pursuant to s. 24, Art. X of the State Constitution and to assist him or her in asserting such rights.

(6)(a) Any person aggrieved by a violation of this section may bring a civil action in a court of competent jurisdiction against an employer violating this section or a party violating subsection (5). However, prior to bringing any claim for unpaid minimum wages pursuant to this section, the person aggrieved shall notify the employer alleged to have violated this section, in writing, of an intent to initiate such an action. The notice must identify the minimum wage to which the person aggrieved claims entitlement, the actual or estimated work dates and hours for which payment is sought, and the total amount of alleged unpaid wages through the date of the notice.

(b) The employer shall have 15 calendar days after receipt of the notice to pay the total amount of unpaid wages or otherwise resolve the claim to the satisfaction of the person aggrieved. The statute of limitations for bringing an action pursuant to this section shall be tolled during this 15-day period. If the employer fails to pay the total amount of unpaid wages or otherwise resolve the claim to the satisfaction of the person aggrieved, then the person aggrieved may bring a claim for unpaid minimum wages, the terms of which must be consistent with the contents of the notice.

(c)1. Upon prevailing in an action brought pursuant to this section, aggrieved persons shall recover the full amount of any unpaid back wages unlawfully withheld plus the same amount as liquidated damages and shall be awarded reasonable attorney's fees and costs. As provided under the federal Fair Labor Standards Act, pursuant to s. 11 of the

Portal-to-Portal Act of 1947, 29 U.S.C. s. 260, if the employer proves by a preponderance of the evidence that the act or omission giving rise to such action was in good faith and that the employer had reasonable grounds for believing that his or her act or omission was not a violation of s. 24, Art. X of the State Constitution, the court may, in its sound discretion, award no liquidated damages or award any amount thereof not to exceed an amount equal to the amount of unpaid minimum wages. The court shall not award any economic damages on a claim for unpaid minimum wages not expressly authorized in this section.

2. Upon prevailing in an action brought pursuant to this section, aggrieved persons shall also be entitled to such legal or equitable relief as may be appropriate to remedy the violation, including, without limitation, reinstatement in employment and injunctive relief. However, any entitlement to legal or equitable relief in an action brought under s. 24, Art. X of the State Constitution shall not include punitive damages.

(d) Any civil action brought under s. 24, Art. X of the State Constitution and this section shall be subject to s. 768.79.

(7) The Attorney General may bring a civil action to enforce this section. The Attorney General may seek injunctive relief. In addition to injunctive relief, or in lieu thereof, for any employer or other person found to have willfully violated this section, the Attorney General may seek to impose a fine of $1,000 per violation, payable to the state.

(8) The statute of limitations for an action brought pursuant to this section shall be for the period of time specified in s. 95.11 beginning on the date the alleged violation occurred.

(9) Actions brought pursuant to this section may be brought as a class action pursuant to Rule 1.220, Florida Rules of Civil Procedure. In any class action brought pursuant to this section, the plaintiffs shall prove, by a preponderance of the evidence, the individual identity of each class member and the individual damages of each class member.

(10) This section shall constitute the exclusive remedy under state law for violations of s. 24, Art. X of the State Constitution.

(11) Except for calculating the adjusted state minimum wage and publishing the initial state minimum wage and any annual adjustments thereto, the authority of the Department of Economic Opportunity in implementing s. 24, Art. X of the State Constitution, pursuant to this section, shall be limited to that authority expressly granted by the Legislature.

History.—s. 2, ch. 2005-353; s. 399, ch. 2011-142; s. 73, ch. 2012-30.

CHAPTER 2005-353

Senate Bill No. 18-B

An act relating to the state minimum wage; amending s. 95.11, F.S.; providing periods of limitations on actions for violations of the Florida Minimum Wage Act; creating s. 448.110, F.S., the Florida Minimum Wage Act; providing legislative intent to implement s. 24, Art. X of the State Constitution in accordance with authority granted to the Legislature therein; requiring employers to pay certain employees a minimum wage for all hours worked in Florida; incorporating provisions of the federal Fair Labor Standards Act; requiring the minimum wage to be adjusted annually; providing a formula for calculating such adjustment; requiring the Agency for Workforce Innovation and the Department of Revenue to annually publish the amount of the adjusted minimum wage; providing criteria for posting; requiring the agency to provide written notice to certain employers; providing a deadline for the notice to be mailed; providing that employers are responsible for maintaining their current addresses with the agency; requiring the agency to provide the department with certain information; prohibiting discrimination or adverse action against persons exercising constitutional rights under s. 24, Art. X of the State Constitution; providing for civil action by aggrieved persons; requiring aggrieved persons bringing civil actions to provide written notice to their employers alleged to have violated the act; providing information that must be included in the notice; providing a deadline by which an employer alleged to have violated the act must pay the unpaid wages in question or resolve the claim to the aggrieved person's satisfaction; providing that a statute of limitations is tolled for a specified period; providing a statute of limitations period; providing that aggrieved persons who prevail in their actions may be entitled to liquidated damages and reasonable attorney's fees and costs; authorizing additional legal or equitable relief for aggrieved persons who prevail in such actions; providing that punitive damages may not be awarded; providing that actions brought under the act are subject to s. 768.79, F.S.; authorizing the Attorney General to bring a civil action and seek injunctive relief; providing a fine; providing statutes of limitations; authorizing class actions; declaring the act the exclusive remedy under state law for violations of s. 24, Art. X of the State Constitution; providing for implementation measures; designating ss. 448.01-448.110, F.S., as part I of ch. 448, F.S.; providing a part title; providing for severability; providing an effective date.

Be It Enacted by the Legislature of the State of Florida:

Section 1. Paragraph (d) is added to subsection (2) and paragraph (q) is added to subsection (3) of section 95.11, Florida Statutes, to read:

95.11 Limitations other than for the recovery of real property.—Actions other than for recovery of real property shall be commenced as follows:

(2) WITHIN FIVE YEARS.—

<u>(d) An action alleging a willful violation of s. 448.110.</u>

(3) WITHIN FOUR YEARS.—

(q) An action alleging a violation, other than a willful violation, of s. 448.110.

Section 2. Section 448.110, Florida Statutes, is created to read:

448.110 State minimum wage; annual wage adjustment; enforcement.—

(1) This section may be cited as the "Florida Minimum Wage Act."

(2) The purpose of this section is to provide measures appropriate for the implementation of s. 24, Art. X of the State Constitution, in accordance with authority granted to the Legislature pursuant to s. 24(f), Art. X of the State Constitution.

(3) Effective May 2, 2005, employers shall pay employees a minimum wage at an hourly rate of $6.15 for all hours worked in Florida. Only those individuals entitled to receive the federal minimum wage under the federal Fair Labor Standards Act and its implementing regulations shall be eligible to receive the state minimum wage pursuant to s. 24, Art. X of the State Constitution and this section. The provisions of ss. 213 and 214 of the federal Fair Labor Standards Act, as interpreted by applicable federal regulations and implemented by the Secretary of Labor, are incorporated herein.

(4)(a) Beginning September 30, 2005, and annually on September 30 thereafter, the Agency for Workforce Innovation shall calculate an adjusted state minimum wage rate by increasing the state minimum wage by the rate of inflation for the 12 months prior to September 1. In calculating the adjusted state minimum wage, the agency shall use the Consumer Price Index for Urban Wage Earners and Clerical Workers, not seasonally adjusted, for the South Region or a successor index as calculated by the United States Department of Labor. Each adjusted state minimum wage rate shall take effect on the following January 1, with the initial adjusted minimum wage rate to take effect on January 1, 2006.

(b) The Agency for Workforce Innovation and the Department of Revenue shall annually publish the amount of the adjusted state minimum wage and the effective date. Publication shall occur by posting the adjusted state minimum wage rate and the effective date on the Internet home pages of the agency and the department by October 15 of each year. In addition, to the extent funded in the General Appropriations Act, the agency shall provide written notice of the adjusted rate and the effective date of the adjusted state minimum wage to all employers registered in the most current unemployment compensation database. Such notice shall be mailed by November 15 of each year using the addresses included in the database. Employers are responsible for maintaining current address information in the unemployment compensation database. The agency shall not be responsible for failure to provide notice due to incorrect or incomplete address information in the database. The agency shall provide the Department of Revenue with the adjusted state minimum wage rate information and effective date in a timely manner.

(5) It shall be unlawful for an employer or any other party to discriminate in any manner or take adverse action against any person in retaliation for exercising rights protected pursuant to s. 24, Art. X of the State Constitution. Rights protected include, but are not limited to, the right to file a complaint or inform any person of his or her potential rights pursuant to s. 24, Art. X of the State Constitution and to assist him or her in asserting such rights.

(6)(a) Any person aggrieved by a violation of this section may bring a civil action in a court of competent jurisdiction against an employer violating this section or a party violating subsection (5). However, prior to bringing any claim for unpaid minimum wages pursuant to this section, the person aggrieved shall notify the employer alleged to have violated this section, in writing, of an intent to initiate such an action. The notice must identify the minimum wage to which the person aggrieved claims entitlement, the actual or estimated work dates and hours for which payment is sought, and the total amount of alleged unpaid wages through the date of the notice.

(b) The employer shall have 15 calendar days after receipt of the notice to pay the total amount of unpaid wages or otherwise resolve the claim to the satisfaction of the person aggrieved. The statute of limitations for bringing an action pursuant to this section shall be tolled during this 15-day period. If the employer fails to pay the total amount of unpaid wages or otherwise resolve the claim to the satisfaction of the person aggrieved, then the person aggrieved may bring a claim for unpaid minimum wages, the terms of which must be consistent with the contents of the notice.

(c)1. Upon prevailing in an action brought pursuant to this section, aggrieved persons shall recover the full amount of any unpaid back wages unlawfully withheld plus the same amount as liquidated damages and shall be awarded reasonable attorney's fees and costs. As provided under the federal Fair Labor Standards Act, pursuant to s. 11 of the Portal-to-Portal Act of 1947, 29 U.S.C. s. 260, if the employer proves by a preponderance of the evidence that the act or omission giving rise to such action was in good faith and that the employer had reasonable grounds for believing that his or her act or omission was not a violation of s. 24, Art. X of the State Constitution, the court may, in its sound discretion, award no liquidated damages or award any amount thereof not to exceed an amount equal to the amount of unpaid minimum wages. The court shall not award any economic damages on a claim for unpaid minimum wages not expressly authorized in this section.

2. Upon prevailing in an action brought pursuant to this section, aggrieved persons shall also be entitled to such legal or equitable relief as may be appropriate to remedy the violation, including, without limitation, reinstatement in employment and injunctive relief. However, any entitlement to legal or equitable relief in an action brought under s. 24, Art. X of the State Constitution shall not include punitive damages.

(d) Any civil action brought under s. 24, Art. X of the State Constitution and this section shall be subject to s. 768.79.

(7) The Attorney General may bring a civil action to enforce this section. The Attorney General may seek injunctive relief. In addition to injunctive relief, or in lieu thereof, for any employer or other person found to have willfully violated this section, the Attorney General may seek to impose a fine of $1,000 per violation, payable to the state.

(8) The statute of limitations for an action brought pursuant to this section shall be for the period of time specified in s. 95.11 beginning on the date the alleged violation occurred.

(9) Actions brought pursuant to this section may be brought as a class action pursuant to Rule 1.220, Florida Rules of Civil Procedure. In any class action brought pursuant to this

section, the plaintiffs shall prove, by a preponderance of the evidence, the individual identity of each class member and the individual damages of each class member.

(10) This section shall constitute the exclusive remedy under state law for violations of s. 24, Art. X of the State Constitution.

(11) Except for calculating the adjusted state minimum wage and publishing the initial state minimum wage and any annual adjustments thereto, the authority of the Agency for Workforce Innovation in implementing s. 24, Art. X of the State Constitution, pursuant to this section, shall be limited to that authority expressly granted by the Legislature.

Section 3. Sections 448.01-448.110, Florida Statutes, are designated as part I of chapter 448, Florida Statutes, and entitled "Terms and Conditions of Employment."

Section 4. If any provision of this act or the application thereof to any person or circumstance is held invalid, the invalidity shall not affect the other provisions or applications of the act which can be given effect without the invalid provision or application, and to this end the provisions of this act are declared severable.

Section 5. This act shall take effect upon becoming a law.

Approved by the Governor December 12, 2005.

Filed in Office Secretary of State December 12, 2005.

Chapter 2011-142
Senate Bill No. 2156

Section 399

Section 399. Subsections (2), (4), and (11) of section 448.110, Florida Statutes, are amended to read:

448.110 State minimum wage; annual wage adjustment; enforcement.—

(2) The purpose of this section is to provide measures appropriate for the implementation of s. 24, Art. X of the State Constitution, in accordance with authority granted to the Legislature pursuant to s. 24(f), Art. X of the State Constitution. To implement s. 24, Art. X of the State Constitution, the Department of Economic Opportunity is designated as the state Agency for Workforce Innovation.

(4)(a) Beginning September 30, 2005, and annually on September 30 thereafter, the Department of Economic Opportunity ~~Agency for Workforce Innovation~~ shall calculate an adjusted state minimum wage rate by increasing the state minimum wage by the rate of inflation for the 12 months prior to September 1. In calculating the adjusted state minimum wage, the Department of Economic Opportunity ~~agency~~ shall use the Consumer Price Index for Urban Wage Earners and Clerical Workers, not seasonally adjusted, for the South Region or a successor index as calculated by the United States Department of Labor. Each adjusted state minimum wage rate shall take effect on the following January 1, with the initial adjusted minimum wage rate to take effect on January 1, 2006.

(b) The ~~Agency for Workforce Innovation and the~~ Department of Revenue and the Department of Economic Opportunity shall annually publish the amount of the adjusted state minimum wage and the effective date. Publication shall occur by posting the adjusted state minimum wage rate and the effective date on the Internet home pages of the Department of Economic Opportunity ~~agency~~ and the Department of Revenue by October 15 of each year. In addition, to the extent funded in the General Appropriations Act, the Department of Economic Opportunity ~~agency~~ shall provide written notice of the adjusted rate and the effective date of the adjusted state minimum wage to all employers registered in the most current unemployment compensation database. Such notice shall be mailed by November 15 of each year using the addresses included in the database. Employers are responsible for maintaining current address information in the unemployment compensation database. The Department of Economic Opportunity is ~~agency shall~~ not ~~be~~ responsible for failure to provide notice due to incorrect or incomplete address information in the database. The Department of Economic Opportunity ~~agency~~ shall provide the Department of Revenue with the adjusted state minimum wage rate information and effective date in a timely manner.

(11) Except for calculating the adjusted state minimum wage and publishing the initial state minimum wage and any annual adjustments thereto, the authority of the Department of Economic Opportunity ~~Agency for Workforce Innovation~~ in implementing s. 24, Art. X of the State Constitution, pursuant to this section, shall be limited to that authority expressly granted by the Legislature.

CHAPTER 2012-30
Committee Substitute for House Bill No. 7027

Section 73

Section 73. Paragraph (b) of subsection (4) of section 448.110, Florida Statutes, is amended to read:

448.110 State minimum wage; annual wage adjustment; enforcement.—

(4)

(b) The Department of Revenue and the Department of Economic Opportunity shall annually publish the amount of the adjusted state minimum wage and the effective date. Publication shall occur by posting the adjusted state minimum wage rate and the effective date on the Internet home pages of the Department of Economic Opportunity and the Department of Revenue by October 15 of each year. In addition, to the extent funded in the General Appropriations Act, the Department of Economic Opportunity shall provide written notice of the adjusted rate and the effective date of the adjusted state minimum wage to all employers registered in the most current reemployment assistance ~~unemployment compensation~~ database. Such notice shall be mailed by November 15 of each year using the addresses included in the database. Employers are responsible for maintaining current address information in the reemployment assistance ~~unemployment compensation~~ database. The Department of Economic Opportunity is not responsible for failure to provide notice due to incorrect or incomplete address information in the database. The Department of Economic Opportunity shall provide the Department of Revenue with the adjusted state minimum wage rate information and effective date in a timely manner.

Florida Minimum Wage History
2000 to 2019

	Federal Minimum Wage	Florida Minimum Wage	Change in Florida Minimum Wage	Florida Effective Date	
*2000	$5.15	$5.15			
2001	$5.15	$5.15	$0.00		
2002	$5.15	$5.15	$0.00		
2003	$5.15	$5.15	$0.00		
2004	$5.15	$5.15	$0.00		
**2005	$5.15	$6.15	$1.00	5/2/2005	12/31/2005
2006	$5.15	$6.40	$0.25	1/1/2006	12/31/2006
2007	$5.85	$6.67	$0.27	1/1/2007	12/31/2007
2008	$6.55	$6.79	$0.12	1/1/2008	12/31/2008
2009	$6.55	$7.21	$0.42	1/1/2009	7/23/2009
***2009	$7.25	$7.25	$0.04	7/24/2009	12/31/2009
***2010	$7.25	$7.25	$0.00	1/1/2010	12/31/2010
***2011	$7.25	$7.25	$0.00	1/1/2011	5/31/2011
****2011	$7.25	$7.31	$0.06	6/1/2011	12/31/2011
2012	$7.25	$7.67	$0.36	1/1/2012	12/31/2012
2013	$7.25	$7.79	$0.12	1/1/2013	12/31/2013
2014	$7.25	$7.93	$0.14	1/1/2014	12/31/2014
2015	$7.25	$8.05	$0.12	1/1/2015	12/31/2015
2016	$7.25	$8.05	$0.00	1/1/2016	12/31/2016
2017	$7.25	$8.10	$0.05	1/1/2017	12/31/2017
2018	$7.25	$8.25	$0.15	1/1/2018	12/31/2018
2019	$7.25	$8.46	$0.21	1/1/2019	12/31/2019

* 2000-04, the Federal minimum wage
** 2005, Florida enacted a state minimum wage
*** Florida defaulted to the Federal minimum wage
**** Legal ruling raising the minimum wage rate to $7.31

Source: Florida Department of Economic Opportunity, October 2018

IN THE CIRCUIT COURT OF THE SECOND JUDICIAL CIRCUIT OF FLORIDA IN AND FOR LEON COUNTY

Marie Marthe CADET, Jennifer BONILLA,
Roberto RIOS, Isabel MERINO,
RESTAURANT OPPORTUNITIES CENTER
OF MIAMI, WECOUNT! INC., and
FARMWORKER ASSOCIATION OF
FLORIDA, INC.,

Plaintiffs;

CASE NO. 2011 CA 0072

-v.-

FLORIDA AGENCY FOR WORKFORCE
INNOVATION, an Agency of the State of
Florida,

Defendant.

_______________________________/

PREEMPTORY WRIT OF MANDAMUS

AND

FINAL ORDER OF SUMMARY JUDGMENT

THIS CAUSE having come before the Court, on April 26, 2011, for hearing upon the Defendant's response to the Court's Alternative Writ of Mandamus and Order to Show Cause and upon the Plaintiffs' Consolidated Motion for Summary Judgment and Reply to Defendant's Response; and the Court having considered the record and submissions of the parties and having heard the arguments of counsel and being otherwise fully advised in the premises, the Court finds that no genuine issue of material fact exists and that the Plaintiffs are entitled to judgment as a matter of law, and further finds that the Defendant has failed to show cause why a peremptory writ of mandamus should not issue. Accordingly, it is hereby

ORDERED AND ADJUDGED that the Plaintiffs' Motion for Summary Judgment be and the same is hereby GRANTED and that a Peremptory Writ of Mandamus shall issue, such that this FINAL JUDGMENT is hereby entered, as set forth herein below:

1. Pursuant to Article X, Section 24(c), of the Florida Constitution (hereinafter "subsection (c)"), it is declared that:

(a) Subsection (c) does not permit the Florida Agency for Workforce Innovation to decrease the Florida Minimum Wage rate;

(b) Where there has been deflation or no inflation during the "twelve months prior" as described in subsection (c), the Florida Minimum Wage rate shall remain unchanged for the following calendar year;

(c) Where there has been inflation during the "twelve months prior" described in subsection (c), the Florida Minimum Wage rate shall be increased in proportion to that inflation; and

(d) The Florida Minimum Wage rate for 2010 was seven dollars and twenty-one cents ($7.21) per hour; and the Florida Minimum Wage rate for 2011 is seven dollars and thirty-one cents ($7.31) per hour and, for tipped workers, four dollars and twenty-nine cents ($4.29) per hour.

2. The Florida Agency for Workforce Innovation ("Agency") shall immediately publish notice of the above-stated Florida Minimum Wage Rate for 2011, consistent with this Order; and such publication shall be by the means ordinarily used by the Agency for such publication, to wit: on its internet webpage using the notice format ordinarily used by the Agency as modified consistent with this Order, to wit: the notice attached hereto as Exhibit A; and is enjoined from

continuing to withhold a Florida Minimum Wage rate that is calculated consistent with this Order.

3. This Court retains jurisdiction of this cause so as to enter any further order necessary or appropriate in furtherance of the execution or enforcement of this Final Judgment.

DONE AND ORDERED in Chambers in Leon County, Florida, on this 3rd day of May 2011.

TERRY P. LEWIS
CIRCUIT JUDGE

Copies furnished to:

Robert A. Williams
Florida Legal Services
2425 Torreya Drive
Tallahassee, FL 32303

José Javier Rodríguez
Florida Legal Services
3000 Biscayne Blvd. #102
Miami, FL 33137

William H. Davis
Dobson, Davis & Smith
610 North Duval Street
Tallahassee, FL 32301

Faith E. Gay
Quinn, Emanuel, Urquhart & Sullivan, LLP
51 Madison Avenue
New York, NY 10010

Paul K. Sonn (admitted pro hac vice)
Tsedeye Gebreselassie
National Employment Law Project

75 Maiden Lane, Suite 601
New York, NY 10038

Lisa M. Raleigh, Special Counsel
Office of the Attorney General
PL-01, The Capitol
Tallahassee, FL 32399

James E. Landsberg, Deputy General Counsel
Agency for Workforce Innovation
The Caldwell Building, MSC 110
107 E. Madison St.
Tallahassee, FL 32399

Exhibit A

FLORIDA'S MINIMUM WAGE

(Updated May, 2011)

The Florida minimum wage is $7.31 per hour, effective June 1, 2011.

Florida law requires the Agency for Workforce Innovation to calculate an adjusted minimum wage rate each year. The annual calculation is based on the percentage change in the federal Consumer Price Index for urban wage earners and clerical workers in the South Region for the 12-month period prior to September 1, 2010.

On November 2, 2004, Florida voters approved a constitutional amendment which created Florida's minimum wage. The minimum wage applies to all employees in the state who are covered by the federal minimum wage.

Employers must pay their employees the hourly state minimum wage for all hours worked in Florida. The definitions of "employer", "employee", and "wage" for state purposes are the same as those established under the federal Fair Labor Standards Act (FLSA). Employers of "tipped employees" who meet eligibility requirements for the tip credit under the FLSA, may count tips actually received as wages under the Florida minimum wage. However, the employer must pay "tipped employees" a direct wage. The direct wage is calculated as equal to the minimum wage ($7.31) minus the 2003 tip credit ($3.02), or a direct hourly wage of $4.29 as of June 1, 2011.

Employees who are not paid the minimum wage may bring a civil action against the employer or any person violating Florida's minimum wage law. The state attorney general may also bring an enforcement action to enforce the minimum wage. FLSA information and compliance assistance can be found at: http://www.dol.gov/dol/compliance/comp-flsa.htm.

Florida Statutes require employers who must pay their employees the Florida minimum wage to post a minimum wage notice in a conspicuous and accessible place in each establishment where these employees work. This poster requirement is in addition to the federal requirement to post a notice of the federal minimum wage. Florida's minimum wage poster is available for downloading in English and Spanish from the Agency for Workforce Innovation's website at: http://www.floridajobs.org/workforce/posters.html.

The federal poster can be downloaded from the U.S. Department of Labor's website at: http://www.dol.gov/whd/regs/compliance/posters/flsa.htm .

2019 Florida Minimum Wage Calculations
Inflation Rate Calculation Using CPI-W South
Consumer Price Index - South Urban Wage Earners and Clerical Workers

Month - Year	CPI-W 1982-84 Base Year
August 2017	233.691
September 2017	235.707
October 2017	234.886
November 2017	234.667
December 2017	234.361
January 2018	235.649
February 2018	236.975
March 2018	237.318
April 2018	238.380
May 2018	239.291
June 2018	239.844
July 2018	239.787
August 2018	239.743

Point-to-Point Percent Change in CPI-W (Aug 17 to Aug 18) **0.02590 = 2.59%**

(239.743-233.691)/233.691 = 0.02590 = 2.59 %

2018 Florida Minimum Wage **$8.25**

The calculation of the 2019 Florida Minimum Wage Rate is done by applying the percentage change in the CPI-W (Aug 17 to Aug 18) to the 2018 Florida Minimum Wage Rate. The change amount is then added to the 2018 Florida Minimum Wage Rate.

(239.743-233.691)/233.691 = 0.02590

.02590 * $8.25 = $0.21

($8.25 + $0.21) = $8.46

Calculated 2019 Florida Minimum Wage **$8.46**

The Florida Minimum Wage of $8.46 exceeds the Federal Minimum Wage of $7.25 so the Florida Minimum Wage prevails.

Technical note: The change between the 2018 and 2019 Florida minimum wage rate is 21 cents or 2.59 percent.

Source: Florida Department of Economic Opportunity, Bureau of Labor Market Statistics.
Prepared: September 30, 2018

United States Department of Labor

Wage and Hour Division

Wage and Hour Division (WHD)

History of Changes to the Minimum Wage Law

Adapted from *Minimum Wage and Maximum Hours Standards Under the Fair Labor Standards Act*, 1988 Report to the Congress under Section 4(d)(1) of the FLSA.

Early in the administration of the FLSA, it became apparent that application of the statutory minimum wage was likely to produce undesirable effects upon the economies of Puerto Rico and the Virgin Islands if applied to all of their covered industries. Consequently on June 26, 1940, an amendment was enacted prescribing the establishment of special industry committees to determine, and issue through wage orders, the minimum wage levels applicable in Puerto Rico and the Virgin Islands. The rates established by industry committees could be less than the statutory rates applicable elsewhere in the United States.

On May 14, 1947, the FLSA was amended by the Portal-to-Portal Act. This legislation was significant because it resolved some issues as to what constitutes compensable hours worked under FLSA. Matters involving underground travel in coal mines and make-ready practices in factories had been decided earlier in a number of U.S. Supreme Court decisions.

Subsequent amendments to the FLSA have extended the law's coverage to additional employees and raised the level of the minimum wage. In 1949, the minimum wage was raised from 40 cents an hour to 75 cents an hour for all workers and minimum wage coverage was expanded to include workers in the air transport industry. The 1949 amendments also eliminated industry committees except in Puerto Rico and the Virgin Islands. A specific section was added granting the Wage and Hour Administrator in the U.S. Department of Labor authorization to control the incidence of exploitative industrial homework. A 1955 amendment increased the minimum wage to $1.00 an hour with no changes in coverage.

The 1961 amendments greatly expanded the FLSA's scope in the retail trade sector and increased the minimum for previously covered workers to $1.15 an hour effective September 1961 and to $1.25 an hour in September 1963. The minimum for workers newly subject to the Act was set at $1.00 an hour effective September 1961, $1.15 an hour in September 1964, and $1.25 an hour in September 1965. Retail and service establishments were allowed to employ fulltime students at wages of no more than 15 percent below the minimum with proper certification from the Department of Labor. The amendments extended coverage to employees of retail trade enterprises with sales exceeding $1 million annually, although individual establishments within those covered enterprises were exempt if their annual sales fell below $250,000. The concept of enterprise coverage was introduced by the 1961 amendments. Those amendments extended coverage in the retail trade industry from an established 250,000 workers to 2.2 million.

Congress further broadened coverage with amendments in 1966 by lowering the enterprise sales volume test to $500,000, effective February 1967, with a further cut to $250,000 effective February 1969. The 1966 amendments also extended coverage to public schools, nursing homes, laundries, and the entire construction industry. Farms were subject to coverage for the first time if their employment reached 500 or more man days of labor in the previous year's peak quarter. The minimum wage went to $1.00 an hour effective February 1967 for newly covered nonfarm workers, $1.15 in February 1968, $1.30 in February 1969, $1.45 in February 1970, and $1.60 in February 1971. Increases for newly subject farm workers stopped at $1.30. The 1966 amendments extended the fulltime student certification program to covered agricultural employers and to institutions of higher learning.

In 1974, Congress included under the FLSA all no supervisory employees of Federal, State, and local governments and many domestic workers. (Subsequently, in 1976, in *National League of Cities* v. *Usery*, the Supreme Court held that the minimum wage and overtime provisions of the FLSA could not constitutionally apply to State and local government employees engaged in traditional government functions.) The minimum wage increased to $2.00 an hour in 1974, $2.10 in 1975, and $2.30 in 1976 for all except farm workers, whose minimum initially rose to $1.60. Parity with nonfarm workers was reached at $2.30 with the 1977 amendments.

The 1977 amendments, by eliminating the separate lower minimum for large agricultural employers (although retaining the overtime exemption), set a new uniform wage schedule for all covered workers. The minimum went to $2.65 an hour in January 1978, $2.90 in January 1979, $3.10 in January 1980, and $3.35 in January 1981. The amendments eased the provisions for establishments permitted to employ students at the lower wage rate and allowed special waivers for children 10to11 years old to work in agriculture. The overtime exemption for employees in hotels, motels, and restaurants was eliminated. To allow for the effects of inflation, the $250,000 dollar volume of sales coverage test for retail trade and service enterprises was increased in stages to $362,500 after December 31, 1981.

As a result of the Supreme Court's 1985 decision in *Garcia* v. *San Antonio Metropolitan Transit Authority et.al.*, Congress passed amendments changing the application of FLSA to public sector employees. Specifically, these amendments permit State and local governments to compensate their employees for overtime hours worked with compensatory time off in lieu of overtime pay, at a rate of 1 1/2 hours for each hour of overtime worked.

The 1989 amendments established a single annual dollar volume test of $500,000 for enterprise coverage of both retail and no retail businesses. At the same time, the amendments eliminated the minimum wage and overtime pay exemption for small retail firms. Thus, employees of small retail businesses became subject to minimum wage and overtime pay in any workweek in which they engage in commerce or the production of goods for commerce. The minimum wage was raised to $3.80 an hour beginning April 1, 1990, and to $4.25 an hour beginning April 1, 1991. The amendments also established a training wage provision (at 85% of the minimum wage, but not less than $3.35 an hour) for employees under the age of twenty, a provision that expired in 1993. Finally, the amendments established an overtime exception for time spent by employees in remedial education and civil money penalties for willful or repeated violations of the minimum wage or overtime pay requirements of the law.

In 1990, Congress enacted legislation requiring regulations to be issued providing a special overtime exemption for certain highly skilled professionals in the computer field who receive not less than 6 and one-half times the applicable minimum wage.

The 1996 amendments increased the minimum wage to $4.75 an hour on October 1, 1996, and to $5.15 an hour on September 1, 1997. The amendments also established a youth sub minimum wage of $4.25 an hour for newly hired employees under age 20 during their first 90 consecutive calendar days after being hired by their employer; revised the tip credit provisions to allow employers to pay qualifying tipped employees no less than $2.13 per hour if they received the remainder of the statutory minimum wage in tips; set the hourly compensation test for qualifying computer related professional employees at $27.63

an hour; and amended the Portal-to-Portal Act to allow employers and employees to agree on the use of employer provided vehicles for commuting to and from work, at the beginning and end of the work day, without counting the commuting time as compensable working time if certain conditions are met.

The 2007 amendments increased the minimum wage to $5.85 per hour effective July 24, 2007; $6.55 per hour effective July 24, 2008; and $7.25 per hour effective July 24, 2009. A separate provision of the bill brings about phased increases to the minimum wages in the Commonwealth of Northern Mariana Islands and in American Samoa, with the goal of bringing the minimum wages in those locations up to the general federal minimum wage over a number of years.

Where to Obtain Additional Information

This publication is for general information and is not to be considered in the same light as official statements of position contained in the regulations.

For additional information, visit our Wage-Hour website: http://www.wagehour.dol.gov and/or call our Wage-Hour toll-free information and helpline, available 8am to 5pm in your time zone, 1-866-4USWAGE (1-866-487-9243).

Tab 3

State Reports

INITIATIVE FINANCIAL INFORMATION STATEMENT

Florida Minimum Wage Amendment

SUMMARY OF INITIATIVE FINANCIAL INFORMATION STATEMENT

Florida has no minimum wage law. Employers in the state are covered by the Fair Labor Standards Act, a federal law that establishes a minimum wage of $5.15 for most employers and employees. Certain employees are exempt from the minimum wage requirement, and these include farm workers employed on small farms, employees of certain seasonal amusement or recreational establishments, and casual babysitters and persons employed as companions, among others. The federal minimum wage for tipped employees is $2.13 per hour, if the employee receives at least $5.15 when the direct wages and the employee's tips are combined. The proposed amendment creates a Florida minimum wage of $6.15 per hour. This analysis assumes that the amendment applies to all employees covered by the federal minimum wage. Each year the minimum wage will be adjusted for inflation.

Based on the information provided through public workshops and staff research, the Financial Impact Estimating Conference expects that the proposed amendment will have the following financial effects:

- State and local government costs will increase, as wages paid by state and local governments to employees currently earning less than $6.15 per hour are increased to that amount. In addition, wages paid to employees earning at or slightly above $6.15 are likely to increase, as the impact of the higher minimum wage ripples upward on prevailing wage rates. Compared to the total employee compensation paid by state and local governments, the impact of this amendment is very small, approximately three-hundredths of one percent (0.03%).

- The impact of this amendment on state and local government revenues is also expected to be small. The costs of goods and services sold in Florida may rise as wages paid by private-sector employers to low-wage employees increase. Consequently, state sales tax revenues may increase slightly. However, if businesses react to the higher minimum wage by hiring fewer workers, the increased tax revenue may not materialize.

FINANCIAL IMPACT STATEMENT

The impact of this amendment on costs and revenue of state and local governments is expected to be minimal.

I. SUBSTANTIVE ANALYSIS

A. Proposed Amendment

Ballot Title:

Florida Minimum Wage Amendment

Ballot Summary:

This amendment creates a Florida minimum wage covering all employees in the state covered by the federal minimum wage. The state minimum wage will start at $6.15 per hour six months after enactment, and thereafter be indexed to inflation each year. It provides for enforcement, including double damages for unpaid wages, attorney's fees, and fines by the state. It forbids retaliation against employees for exercising this right.

A new section for Article X. is created
Florida Minimum Wage Amendment

Text of Amendment

Full Text:

(a) Public Policy. All working Floridians are entitled to be paid a minimum wage that is sufficient to provide a decent and healthy life for them and their families, that protects their employers from unfair low-wage competition, and that does not force them to rely on taxpayer-funded public services in order to avoid economic hardship.

(b) Definitions. As used in this amendment, the terms "Employer," "Employee" and "Wage" shall have the meanings established under the federal Fair Labor Standards Act (FLSA) and its implementing regulations.

(c) Minimum Wage. Employers shall pay Employees Wages no less than the Minimum Wage for all hours worked in Florida. Six months after enactment, the Minimum Wage shall be established at an hourly rate of $6.15. On September 30th of that year and on each following September 30th, the state Agency for Workforce Innovation shall calculate an adjusted Minimum Wage rate by increasing the current Minimum Wage rate by the rate of inflation during the twelve months prior to each September 1st using the consumer price index for urban wage earners and clerical workers, CPI-W, or a successor index as calculated by the United States Department of Labor. Each adjusted Minimum Wage rate calculated shall be published and take effect on the following January 1st. For tipped Employees meeting eligibility requirements for the tip credit under the FLSA, Employers may credit towards satisfaction of the Minimum Wage tips up to the amount of the allowable FLSA tip credit in 2003.

(d) Retaliation Prohibited. It shall be unlawful for an Employer or any other party to discriminate in any manner or take adverse action against any person in retaliation for exercising rights protected under this amendment. Rights protected under this amendment include, but are not limited to, the right to file a complaint or inform any person about any party's alleged noncompliance with this amendment, and the right to inform any person of his or her potential rights under this amendment and to assist him or her in asserting such rights.

(e) Enforcement. Persons aggrieved by a violation of this amendment may bring a civil action in a court of competent jurisdiction against an Employer or person violating this amendment and, upon prevailing, shall recover the full amount of any back wages unlawfully withheld plus the same amount as liquidated damages, and shall be awarded reasonable attorney's fees and costs. In addition, they shall be entitled to such legal or equitable relief as may be appropriate to remedy the violation including, without limitation, reinstatement in employment and/or injunctive relief. Any Employer or other person found liable for willfully violating this amendment shall also be subject to a fine payable to the state in the amount of $1000.00 for each violation. The state attorney general or other official designated by the state legislature may also bring a civil action to enforce this amendment. Actions to enforce this amendment shall be subject to a statute of limitations of four years or, in the case of willful violations, five years. Such actions may be brought as a class action pursuant to Rule 1.220 of the Florida Rules of Civil Procedure.

(f) Additional Legislation, Implementation & Construction. Implementing legislation is not required in order to enforce this amendment. The state legislature may by statute establish additional remedies or fines for violations of this amendment, raise the applicable Minimum Wage rate, reduce the tip credit, or extend coverage of the Minimum Wage to employers or employees not covered by this amendment. The state legislature may by statute or the state Agency for Workforce Innovation may by regulation adopt any measures appropriate for the implementation of this amendment. This amendment provides for payment of a minimum wage and shall not be construed to preempt or otherwise limit the authority of the state legislature or any other public body to adopt or enforce any other law, regulation, requirement, policy or standard that provides for payment of higher or supplemental wages or benefits, or that extends such protections to employers or employees not covered by this amendment. It is intended that case law, administrative interpretations, and other guiding standards developed under the federal FLSA shall guide the construction of this amendment and any implementing statutes or regulations.

(g) Severability. If any part of this amendment, or the application of this amendment to any person or circumstance, is held invalid, the remainder of this amendment, including the application of such part to other persons or circumstances, shall not be affected by such a holding and shall continue in full force and effect. To this end, the parts of this amendment are severable.

B. Effect of Proposed Amendment

Currently Florida has no minimum wage law. Employers in the state are covered by the Fair Labor Standards Act, a federal law that establishes a minimum wage of $5.15 per hour for most employers and employees. Certain employees are exempt from the minimum wage requirement, and these include farm workers employed on small farms, employees of certain seasonal amusement or recreational establishments, and casual babysitters and persons employed as companions, among others. The federal minimum wage for tipped employees is $2.13 per hour, if the employee receives at least $5.15 when the direct wages and the employee’s tips are combined.

If the proposed amendment is adopted by the voters, Florida will have a minimum wage of $6.15 per hour. The minimum wage paid by the employer for tipped employees would increase from $2.13 per hour to $3.13 per hour. Each year the minimum wage will be adjusted for inflation, based on the consumer price index for urban wage earners and clerical workers.

Background

Floridians for All, a coalition of labor and community groups, sponsored the petition drive to place the minimum wage amendment on the ballot. This organization argues that the current federal minimum wage has not been raised in six years, and that a person working full-time for the minimum wage cannot support a family in a decent way.[1]

[1] Jeff Chapman, “Time to Repair the Florida Wage Floor,” Economic Policy Institute, Washington, D.C.

II. FISCAL ANALYSIS & ECONOMIC IMPACT STATEMENT

Section 100.371, Florida Statutes, requires that the Financial Estimating Conference "…complete an analysis and financial impact statement to be placed on the ballot of the estimated increase or decrease in any revenue or costs to state or local governments resulting from the proposed initiative."

As part of determining the fiscal impact of this proposed amendment, the Financial Impact Estimating Conference held several public workshops over the month of June 2004. The Conference heard testimony on the fiscal effects of this amendment. Dr. Robert Pollin, a professor of economics at the University of Massachusetts and co-director of the Political Economy Research Institute, spoke as a proponent. Speaking in opposition was Mr. Stephen Birtman, representing the National Federation of Independent Business. Additionally, a questionnaire was sent to state and local governments, requesting information regarding the costs associated with the minimum wage increase. Finally, state and national data were analyzed to determine the likely impact of a minimum wage increase on state and local government labor costs and on sales tax revenues.

FISCAL IMPACT ON STATE AND LOCAL GOVERNMENTS:

The fiscal impact summary for this proposed amendment is based on independent research; oral and written statements from proponents (no opponents submitted written information); and discussions among the Financial Estimating Conference and professional staff. Three separate analyses of increasing the Florida minimum wage were considered, using different methodologies and data sources. All conclude that the impact on state and local government costs and revenue is likely to be very small. Based on this information, the Financial Impact Estimating Conference concluded that the proposed amendment to increase the minimum wage to $6.15 per hour would have a minimal impact on the budgets of state and local governments. Following is a description of data and analyses on which the conclusion is based.

Survey of State and Local Governments The Department of Management Services states that there are no full-time state employees who earn less than $6.15 per hour. There are about 351 part-time employees who earn less than $6.15. The total cost to bring their wages to $6.15 per hour is $7,639 per year.

The Department of Education surveyed the state's universities. Their results reveal that there are 27,020 employees earning less than $6.15 per hour. The cost to bring their wages to $6.15 per hour is estimated to be $1.8 million per year. However, most of the employees earning below $6.15 per hour are students and the net effect of an increase in the wage rate may be that universities, due to budget constraints, hire fewer students or reduce the hours that students work.

The Legislative Committee on Intergovernmental Relations surveyed counties and municipalities. Thirty-four out of 67 counties and 184 out of 405 municipalities responded to the survey. The counties that responded to the survey represent about 58 percent of the population and their results show that there are 224 employees that earn less than $6.15 per hour. The estimated cost to bring their wages to $6.15 per hour is roughly $88,000 per year. The municipalities that responded to the survey represent about 19 percent of the population and their results show that there are 179 employees that earn less than $6.15 per hour. The estimated cost to bring their wages to $6.15 per hour is roughly $93,000 per year.

The Florida School Board Association surveyed local school boards. Seventeen out of 67 school boards responded, representing 29 percent of the population. Their results indicate that there are 355 employees earning less than $6.15 per hour. The estimated cost to bring their wages to $6.15 per hour is approximately $120,000 per year.

Overall, the surveys suggest that the cost to state and local governments will be minimal.

Internal Analysis Based on State and National Data The data underlying the analysis come from two U.S. Bureau of Labor Statistics employment surveys: (1) "Current Employment Situation" (CES)—establishment survey of payroll data; and (2) "Current Population Survey" (CPS)—household survey of labor market participation. All data are for 2003 and come from both published and unpublished tables. The principal assumptions underlying the analysis are (1) all increased labor costs are passed on to consumers in the form of higher prices; (2) there are no adverse employment impacts from the higher labor costs; (3) there are no adverse expenditure impacts on consumers because of higher prices.

The measurement of costs associated with passage of the minimum wage proposal was limited to higher labor costs—wages, taxes, and benefits. The direct costs to state and local governments (defined to be the costs of bringing all employees earning between $5.15 and $6.14 per hour to $6.15 per hour) amount to $8 million--$3 million for state government and $5 million for local governments. This represents less than two hundredths of one percent (0.02%) of the total state and local government labor costs in 2003 (which was $40.7 billion according to the U.S. Department of Commerce, Bureau of Economic Analysis).

In addition to the direct costs, increased costs are expected as wages rise for those employees whose hourly wages are above, but near, the proposed minimum wage. This effect, known as the ripple effect, is expected to occur as a behavioral response by employers to attempt to maintain a wage scale similar to the one that existed prior to the new minimum wage. After the wage increases are fully phased in across higher earnings classes to account for the ripple effect, the total labor costs are estimated to be $13.6 million for state and local government or about 0.033% of the total state and local government labor costs in 2003.

These costs are offset by increased sales tax receipts of $6.3 million associated with the higher costs of taxable goods. When netted against the increased labor costs to state and local government, the proposal is expected to result in a net cost to state and local government of approximately $7.3 million dollars. This estimate does not include any increase in local option sales taxes received by local governments resulting from higher consumer prices for taxable goods.

Increases in state and local government employment costs and revenues from sales taxes will be reduced to the extent government and business respond to the higher minimum wage by limiting employment.

The Research of Dr. Robert Pollin[2] According to Dr. Pollin, who conducted research on behalf of the proponents of the amendment, the estimated net fiscal impact of the Florida minimum wage proposal is positive. That is, additional revenues and savings will exceed costs by $2.9 million. This estimate is derived from an estimated $13.3 million in higher wage costs, reduced by additional tax revenue and Medicaid savings of about $16.2 million.

Costs associated with an increase in the minimum wage are higher salaries ($10.8 million), higher contract costs for state contracts ($1.8 million), and one-time administrative expenses ($0.7 million).

[2] Robert Pollin: " Assessment of the Net Fiscal Impact of the Florida Minimum Wage Proposal," June 16, 2004

The increase in sales tax revenue is a result of a higher general price increase in taxable goods ($11.7 million). It is expected that the increase in wage costs to employers (mostly hotels and restaurants) will be passed on to the ultimate consumer in the form of higher prices. Dr. Pollin's analysis assumes that there will be no adverse impacts on employment.

The savings associated with Medicaid and KidCare are expected to be $4.5 million. The higher wages are expected to push some individuals above the eligibility threshold for Medicaid and KidCare coverage.

ATTACHMENT
Proposed Constitutional Amendment
Florida Minimum Wage Amendment

	All Industries	Combined State & Local Government	State Government	Local Government
Direct Effect - Impact on employees earning less than $6.15 per hour				
Direct Wages Increase	$144,787,500	$6,974,773	$2,656,134	$4,318,639
Increased Benefits/Taxes	$14,478,750	$1,046,216	$398,420	$647,796
Direct Total Cost Increase	**$159,266,250**	**$8,020,989**	**$3,054,554**	**$4,966,435**
Include Ripple Effect - Extends to $7.99 per Hour With Die-out of Effect at 33% of Prior Wage Interval				
Wages Increase	$238,417,843	$11,828,639	$4,153,050	$7,675,588
Increased Benefits/Taxes	$23,841,784	$1,774,296	$622,958	$1,151,338
Total Cost Increase	**$262,259,628**	**$13,602,934**	**$4,776,008**	**$8,826,926**
Sales Tax Impact - 40% of wage increase is spent on taxable goods and services				
Increase in Taxable Sales	$104,903,851			
Increase in Sales Taxes [1]	**$6,294,231**			

[1] The estimate excludes any local option sales taxes.

Financial Impact Estimating Conference
June 23, 2004

SENATE STAFF ANALYSIS AND ECONOMIC IMPACT STATEMENT

(This document is based on the provisions contained in the legislation as of the latest date listed below.)

Prepared By: Commerce and Consumer Services Committee

BILL: SB 18-B

INTRODUCER: Senator Alexander

SUBJECT: Minimum Wage

DATE: December 6, 2005 REVISED:

	ANALYST	STAFF DIRECTOR	REFERENCE	ACTION
1.	Gordon	Cooper	CM	**Favorable**
2.				
3.				
4.				
5.				
6.				

I. Summary:

This bill implements the provisions of s. 24, Art. X of the State Constitution, relating to the Florida minimum wage. The bill replicates the provisions of the constitution and adds additional provisions to:

- Adopt the U.S. Consumer Price Index for the south region as the applicable index for determining the annual adjustments to the state minimum wage;
- Require the Agency for Workforce Innovation and the Department of Revenue to publish the annually updated minimum wage on their respective websites;
- Require employees to first notify employers before initiating a civil action to enforce their right to receive the state minimum wage;
- Allow employers 15 calendar days to resolve any claims for the unpaid wages before a suit may be filed;
- Limit the damages awarded to employees to only unpaid wages if the court determines the employer acted in good faith and had reasonable grounds for believing that their action was not in violation of the constitution;
- Restrict the court from awarding punitive damages;
- Impose restrictions on class action suits;
- Limit eligibility for the minimum wage to workers who are currently entitled to receive the federal minimum wage under the Fair Labor Standards Act (FLSA) and its associated implementing regulations; and
- Provide that the exemptions outlined in ss. 213 and 214 of FLSA are incorporated into this act by reference.

This bill amends section 95.11 of the Florida Statutes.

This bill creates sections 448.110, F.S., of the Florida Statutes.

II. Present Situation:

Constitutional Provision

On November 2, 2004, Florida citizens passed Amendment 5 on the ballot during the general election. The amendment became s. 24, Art. X, of the State Constitution and contained seven distinct provisions. Subsection (a) outlines the purpose of the provision—to provide all working Floridians with a sufficient wage and protect employers from unfair low-wage competition. Subsection (b) provides that the terms "employer," "employee," and "wage," will be defined as they are under the federal Fair Labor Standards Act (FLSA) and its associated regulations.

Subsection (c) sets the minimum wage at $6.15 per hour beginning 6 months after the effective date of the provision. This subsection also directs AWI to calculate the wage rate annually using the consumer price index[1] for urban wage earners and clerical workers, the CPI-W or a successor index as calculated by the U.S. Department of Labor. Moreover, this subsection requires raising the rate each year by the rate of inflation in the previous 12 months. This provision also requires that the new rate be published annually by January 1st and permits employers to credit towards the satisfaction of the minimum wage, any tips received by tipped employees.

Subsection (d) expressly prohibits employers from retaliating against employees who exercise their rights under the provision. This provision outlines the rights protected by the provision including, but not limited to, "the right to file a complaint or inform any person about any party's alleged noncompliance with this amendment, and the right to inform any person of his or her potential rights under this amendment and to assist him or her in asserting such rights."

Subsection (e) authorizes employees to bring a civil action to enforce the provisions of this provision. An employee who prevails may recover unpaid wages, an equal amount in liquidated damages and attorney's fees and costs. Employers or others who violate these provisions are subject to a $1,000 per violation fine. This provision also authorizes the Attorney General or other official(s) designated by the Legislature to bring a civil action to enforce the provision. Moreover, under this provision, the statute of limitation is 4 years and 5 years for willful violations. The provision also permits class actions by employees.

Subsection (f) provides that implementing legislation is not required to enforce the constitutional provision. However, this subsection also permits the Legislature to create statutes to "establish additional remedies or fines for violation of this amendment, raise the applicable Minimum Wage rate, reduce the tip credit, or extend coverage of the Minimum Wage to employers or employees not covered by this amendment." The provision then provides that AWI or the state Legislature may adopt additional measures they deem appropriate to implement the minimum wage law. According to this provision, the subsection does not prevent the Legislature or any

[1] The Consumer Price Index (CPI) is a measure of the average change over time in the prices paid by urban consumers for goods and services.

other public body from adopting or enforcing measures that provide for the payment of higher or supplemental wages or benefits.

Subsection (g) of the provision contains a severability clause.

Minimum Wage Law

The Fair Labor Standards Act (FLSA), 29 U.S.C. 201, et. seq., governs federal minimum wage as well as overtime, recordkeeping and child labor standards. Section 206 of the act sets the minimum wage at $5.15 per hour effective September 1, 1997. Thirteen states plus the District of Columbia, excluding Florida, had minimum wage rates higher than the federal minimum wage as of January 1, 2005.[2] At that time, Florida was one of seven states that had not enacted a minimum wage law.[3]

FLSA governs or "covers" employees in one of two categories:

1. Enterprise coverage—Employees who work for enterprises, businesses or organizations doing at least $500,000 of business per year, and hospitals, businesses providing medical or nursing care for residents, schools and preschools, and government agencies; or
2. Individual coverage—Employees whose work involves the production of goods for commerce or engagement in interstate commerce and domestic workers.

Employees in the aforementioned categories are to be paid the federal minimum wage. According to the Department of Labor, where an employee is subject to both the state and federal minimum wage laws, the employee is entitled to the higher of the two minimum wages.

Like Florida's constitutional provision, current federal law also prescribes wages for tipped employees. Under FLSA, employers are required to pay $2.13 per hour as direct wages to tipped employees as long as that amount plus tips received equals at least the federal minimum wage, the employee keeps all of his of her tips and the employee meets the definition of a tipped employee (customarily and regularly receives more than $30 a month in tips).[4] Although the Florida constitutional provision does not explicitly state that the base wage for tipped employees will also be raised by one dollar, it does provide the following guidance: "For tipped Employees meeting eligibility requirements for the tip credit under the FLSA, Employers may credit towards satisfaction of the Minimum Wage tips up to the amount of the allowable FLSA tip credit in 2003."[5] The Agency for Workforce Innovation (AWI) has interpreted this language to mean that tipped employees may not be paid less than $3.13 per hour.[6] Specifically, AWI indicates this new direct wage is derived by subtracting the 2003 tip credit, $3.02, from Florida's minimum wage.

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[2] U.S. Department of Labor, *Minimum Wage Laws in the States-January 1, 2005*. 13 April 2005. <http://www.dol.gov/esa/minwage/america.htm>.
[3] Id.
[4] U.S. Department of Labor, *Questions and Answers about the Minimum Wage*. 12 April 2005. <http://www.dol.gov/esa/minwage/q-a.htm>.
[5] Section 24, Art. X, State Constitution.
[6] Workforce Innovation, *Florida's Minimum Wage*, <http://www.floridajobs.org/resources/fl_min_wage.html>. 15 April 2005.

FLSA also contains several exemptions to the minimum wage law including, but not limited to, exemptions for workers with disabilities, full time students and student learners who are employed pursuant to sub-minimum wage certificates.[7]

The Wage and Hour Division of the U.S. Department of Labor enforces the minimum wage. The division has offices throughout the country that investigate wage and labor claims, bring enforcement actions against employers, and hold education seminars related to employment laws.

Implementation of Florida's Minimum Wage

On May 5, 2005, 6 months after s. 24, Article X was enacted, Florida's minimum wage rose from $5.15 per hour to $6.15 per hour. As directed by the provision, AWI calculated a new minimum wage on September 30, 2005 and published information related to the new wage in a press release.[8] AWI also published the new wage rate on its website. According to the provision, the new wage, $6.40 per hour for those eligible to receive the FLSA minimum wage and $3.38 for "tipped employees," becomes effective January 1, 2006.

III. Effect of Proposed Changes:

This bill implements the provisions of s. 24, Art. X of the State Constitution, relating to the Florida minimum wage. The bill replicates the provisions of the constitution and adds additional provisions to:

- Adopt the U.S. Consumer Price Index for the south region as the applicable index for determining the annual adjustments to the state minimum wage;
- Require the Agency for Workforce Innovation and the Department of Revenue to publish the annually updated minimum wage on their respective websites;
- Require employees to first notify employers before initiating a civil action to enforce their right to receive the state minimum wage;
- Allow employers 15 calendar days to resolve any claims for the unpaid wages before a suit may be filed;
- Limit the damages awarded to employees to only unpaid wages if the court determines the employer acted in good faith and had reasonable grounds for believing that their action was not in violation of the constitution;
- Restrict the court from awarding punitive damages;
- Impose restrictions on class action suits;

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[7] *See*, 29 U.S.C ss. 213 and 214. On November 23, 2005, Florida's Attorney General (AG) issued an opinion, AGO 2005-64, addressing whether the federal minimum wage exemption for persons with disabilities (*see* 29 U.S.C. section 214) is incorporated into the minimum wage constitutional provision. The AG concluded that the constitutional amendment incorporates the provisions of the federal FLSA including its exceptions and exemptions. In forming this opinion, the AG relied on the Supreme Court's *Advisory Opinion to the Attorney General Re: Florida Minimum Wage*, 880 So. 2d 636, 641-642, (Fla. 2004). In that opinion the Court essentially states that, although the amendment may not have included specific references to FLSA, it does incorporate a general reference to that entire body of law.

[8] Agency for Workforce Innovation (September 30, 2005) *Florida Minimum Wage Raise in Annual Calculation Announced.* Press Release.

- Limit eligibility for the minimum wage to workers who are currently entitled to receive the federal minimum wage under Fair Labor Standards Act (FLSA) and its associated implementing regulations; and
- Provide that the exemptions outlined in ss. 213 and 214 of FLSA are incorporated into this act by reference.

Section 1 amends s. 95.11, F.S., outlining the statute of limitations for filing an action other than for the recovery of real property.

Subsection 95.11(2), F.S., provides a 5-year limitation period on the filing of a cause of action on a judgment or decree under certain conditions, a legal or equitable action based on a contract, obligation, or liability founded on a written document with certain exceptions, and an action to foreclose a mortgage. This section adds an action alleging a willful violation of the new state minimum wage law, s. 448.110, F.S., created in section 2 of this bill.

Currently, subsection 95.11(3), F.S., provides a 4-year limitation period for several actions including, for example, actions founded on negligence and actions to rescind a contract. This section of the bill adds an action generally alleging a violation of the new state minimum wage law, s. 448.110, F.S., created in section 2 of this bill.

Section 2 creates the “Florida Minimum Wage Act” in s. 448.110, F.S.

Subsection (2) expressly states that the purpose of this section is to implement s. 24, Art. X of the State Constitution pursuant to the authority granted to the Legislature under that constitutional provision.

Subsection (3) designates May 2, 2005, as the effective date of the new minimum wage of $6.15 per hour. In addition, this subsection limits eligibility for the minimum wage to workers who are currently entitled to receive the federal minimum wage under FLSA and its associated implementing regulations. This subsection also incorporates the exceptions and exemptions to the minimum wage outlined in ss. 213 and 214 of FLSA.[9]

Paragraph (4)(a) directs AWI to calculate the adjusted minimum wage rate, indexing it to the rate of inflation for the preceding 12 months. This section requires AWI to use the Consumer Price Index for Urban Wage Earners and Clerical Workers, CPI-W, for the south region or a successor index as calculated by the U.S. Department of Labor. AWI is also required to execute this calculation on September 30, 2005, and each September 30th thereafter. This section also provides that the adjusted state minimum wage will take effect each January 1st beginning on January 1, 2006.

Paragraph (4)(b) directs AWI and the Department of Revenue (DOR) to notify employers annually of the new minimum wage by posting the rate on their respective internet home pages

[9] Section 213 of FLSA exempts a long list of employees from receiving the federal minimum wage including, for example, executives, certain employees in the agricultural or fishing industries and switchboard operations. Section 214 of FLSA exempts from FLSA employees such as persons with disabilities and certain apprentices, who are hired under specific certificates obtained from the U.S. Department of Labor.

by October 15 of each year. To the extent funded in the General Appropriations Act, AWI will also send written notice of the adjusted rate to all employers registered in the most current unemployment compensation database by November 15 of each year. AWI will not be held responsible for failure to provide notice due to incorrect or incomplete address information in its database. AWI will also provide DOR the wage rate information including the effective date in a timely manner.

Subsection (5) prohibits retaliatory action by employers against employees who file a complaint, inform another person of his or her rights, or assist another individual in asserting his or her rights under s. 24, Art. X of the State Constitution.

Paragraph (6)(a) authorizes civil actions to be brought by an aggrieved person against an employer for violations of this act. This section requires potential plaintiffs to notify their employers in writing of their intent to file suit, and include the following information in the notice:

- The minimum wage to which the employee is entitled;
- The actual or estimated work dates and hours for which payment is sought; and
- The total amount of alleged unpaid wages through the date of the notice.

Paragraph (6)(b) allows the employer 15 calendar days after the receipt of the notice to pay the back wages or otherwise resolve the claim to the satisfaction of the employee. If the employer fails to pay the back wages or otherwise satisfy the claim, the employee may file a civil action.

Paragraph (6)(c) outlines the type of damages recoverable by a complainant who files civil action under this act. Subparagraph (6)(c)1. provides that a complainant who prevails in such an action shall be awarded unpaid back wages plus the same amount in liquidated damages as well as reasonable attorney's fees and costs. These permitted damages are consistent with subsection (e) of the constitutional provision. This section also permits employers to avoid a judgment of liquidated damages or any award not to exceed the unpaid minimum wage if the employer shows, to the court's satisfaction, that the act or omission that gave rise to the action was in good faith. The employer must also demonstrate reasonable grounds to believe the act or omission was not a violation of s. 24, Art. X of the State Constitution.

Subparagraph (6)(c)2. allows such legal or equitable relief that may be appropriate including, reinstatement in employment and injunctive relief. However, this subparagraph specifically prohibits granting punitive damages to the complainant.

Subsection (7) authorizes the Attorney General (AG) to bring a civil action to enforce the provisions of this act including seeking injunctive relief. This section also permits the AG to impose a $1,000 per violation fine on any employer or other person for willfully violating this section.

Subsection (8) provides a statute of limitation of 4 years from the date of the violation. That time period is extended to 5 years where there has been a willful violation of this act.

Subsection (9) allows complainants to bring class actions under this act. However, any class action must identify each class member and include proof of individual damages for each class member.

Subsection (10) states that the act provides the exclusive remedy under state law for violations of s. 24, Art. X of the State Constitution.

Subsection (11) provides that AWI's authority, aside from calculating the adjusted state minimum wage and publishing the initial state minimum wage, is limited to that authority expressly granted by the Legislature.

Section 3 provides that sections 448.01-448.110, F.S., are to be designated as Part I of chapter 448, F.S., and entitled "Terms and Conditions of Employment."

Section 4 provides a severability provision for each distinct provision in the bill.

Section 5 provides that this act will be effective upon becoming law.

IV. Constitutional Issues:

A. Municipality/County Mandates Restrictions:

None.

B. Public Records/Open Meetings Issues:

None.

C. Trust Funds Restrictions:

None.

D. Other Constitutional Issues:

While subsection 24 (f), Art. X of the State Constitution states that implementing legislation is not required to enforce this provision, it authorizes the state Legislature to "adopt any measures appropriate" for its implementation.

Access to Courts

Subsection 24(e), Art. X of the State Constitution allows persons "aggrieved by a violation of this amendment" to "bring a civil action in a court of competent jurisdiction against an Employer or person violating this amendment…." Proposed subsection 448.110(6), F.S., requires the employee to provide notification of his/her intent to initiate civil action, and restricts the filing of an action to 15 days after the employer receives notice. The 15-day period gives an employer the opportunity to resolve the dispute.

This subsection also requires that the wage, hour, and salary totals the plaintiff outlines in a complaint be consistent with the initial notice to the employer.

To the extent this implementing legislation unreasonably limits access to the court, it may be subject to constitutional challenge.

Class Action Lawsuits
Subsection 24(e), Art. X of the State Constitution permits class actions to enforce this provision "to be brought as a class action pursuant to Rule 1.220 of the Florida Rules of Civil Procedure." Proposed subsection 448.110(9), F.S., requires that "[i]n any class action brought pursuant to this section, the plaintiffs shall prove, by a preponderance of the evidence, the individual damages for each class member."

To the extent this provision conflicts with the express language of the constitution, it may be subject to constitutional challenge.

Damages
Subsection 24(e), Art. X of the State Constitution provides that, upon prevailing in an action against an employer, the employee is entitled to

> recover the full amount of any back wages unlawfully withheld plus the same amount as liquidated damages, and shall be awarded reasonable attorney's fees and costs. In addition, they shall be entitled to such legal or equitable relief as may be appropriate to remedy the violation including, without limitation, reinstatement in employment and/or injunctive relief.

Proposed subparagraph 448.110(6)(c)1., F.S., limits such damages if the court finds that the employer proves by a preponderance of the evidence that s/he acted in good faith and "had reasonable grounds for believing that his or her act or omission was not a violation" of the constitutional minimum wage provision. This limitation parallels the language of 29 U.S.C. s. 260,[10] which was enacted to minimize the uncertainty regarding the liabilities of employers under FLSA.[11]

In addition, proposed subparagraph 448.110(6)(c)2., F.S., states that the legal or equitable relief for violations of the state minimum wage "shall not include punitive damages."

Subsection (f) of section 24, Art. X of the State Constitution permits the Legislature to adopt implementing legislation related to the minimum wage to "*establish additional remedies* or fines for violations of this amendment, raise the applicable Minimum Wage rate, reduce the tip credit or extend coverage of the Minimum Wage to employers or employees not covered by this amendment." (Emphasis added).

To the extent this implementing legislation limits or reduces the damages provided in the minimum wage provision, it may be subject to constitutional challenge.

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[10] Section 11 of the Portal-to-Portal Act of 1947. (29 CFR 790).
[11] 29 CFR Ch. V, Part 790 (view at <http://www.osha.gov/pls/epub/wageindex.download?p_file=F8840/wh1056.pdf>). 15 April 2005.

V. Economic Impact and Fiscal Note:

A. Tax/Fee Issues:

None.

B. Private Sector Impact:

This bill limits the damages available to employees who have not received the wages due them, as determined by the courts.

C. Government Sector Impact:

In the 2005 regular session, AWI estimated an initial cost of posting the minimum wage rate to the internet of $90.00 and less than that figure for updates. The agency also estimated costs of $150,159 for FY 2005-2006, and $168,129 for FY 2006-2007, for the required mailing to employers.

VI. Technical Deficiencies:

None.

VII. Related Issues:

None.

VIII. Summary of Amendments:

None.

Minimum Wage

(For Discussion Purposes Only)

Date		Federal	Florida	Florida	CPI-W (South)* Index	CPI-W (South)* % Change	CPI-U (U.S.)** Index	CPI-U (U.S.)** % Change
Jan-05		$5.15	$5.15		179.400		189.367	
May-05		$5.15	$6.15					
Jan-06		$5.15	$6.40		186.600	4.01%	196.600	3.82%
Jan-07		$5.15	$6.67		194.500	4.23%	203.167	3.34%
Jul-07		$5.85	$6.67					
Jan-08		$6.55	$6.79		198.063	1.83%	207.939	2.35%
Jan-09		$6.55	$7.21		210.362	6.21%	218.861	5.25%
Jul-09		$7.25	$7.25					
Jan-10		$7.25	$7.25		205.867	-2.14%	215.344	-1.61%
Jan-11		$7.25	$7.25		208.740	1.40%	217.934	1.20%
Jun-11	^	$7.25	$7.31					
Jan-12		$7.25	$7.67		218.947	4.89%	226.033	3.72%
Jan-13		$7.25	$7.79		222.250	1.51%	229.841	1.68%
Jan-14		$7.25	$7.93		226.119	1.74%	233.300	1.50%
Jan-15		$7.25	$8.05		229.594	1.54%	237.478	1.79%
Jan-16	▪	$7.25	$8.05		228.011	-0.69%	237.862	0.16%
Jan-17		$7.25	$8.10		229.479	0.64%	240.601	1.15%
Jan-18		$7.25	$8.25		233.691	1.84%	245.368	1.98%
Jan-19		$7.25	$8.46		239.743	2.59%	251.829	2.63%

Date		Current Law/Current Administration		Based on CPI-W 30-Year Compound Growth Rate*		Based on CPI-U Growth Rate**		CPI-W (South) 30-Year Compound Growth Rate*		CPI-U Growth Rate**	
Jan-20		$7.25		$8.67		$8.63			2.43%	256.984	2.05%
Jan-21		$7.25		$8.88		$8.80			2.43%	262.003	1.95%
Jan-22	#	$7.25		$9.10	$10.00	$9.00	$10.00		2.43%	268.102	2.33%
Jan-23	#	$7.25		$9.32	$11.00	$9.21	$11.00		2.43%	274.465	2.37%
Jan-24	#	$7.25		$9.55	$12.00	$9.43	$12.00		2.43%	281.080	2.41%
Jan-25	#	$7.25		$9.78	$13.00	$9.66	$13.00		2.43%	287.888	2.42%
Jan-26	#	$7.25		$10.02	$14.00	$9.88	$14.00		2.43%	294.352	2.25%
Jan-27	#	$7.25		$10.26	$15.00	$10.10	$15.00		2.43%	300.884	2.22%
Jan-28		$7.25		$10.51	$15.36	$10.33	$15.34		2.43%	307.606	2.23%
Jan-29		$7.25		$10.77	$15.73	$10.57	$15.69		2.43%	314.636	2.29%
Jan-30		$7.25		$11.03	$16.11	$10.81	$16.04		2.43%	321.681	2.24%

Red indicates increases in Federal minimum wage rate.

^ Legal ruling raising the Florida minimum wage rate to $7.31

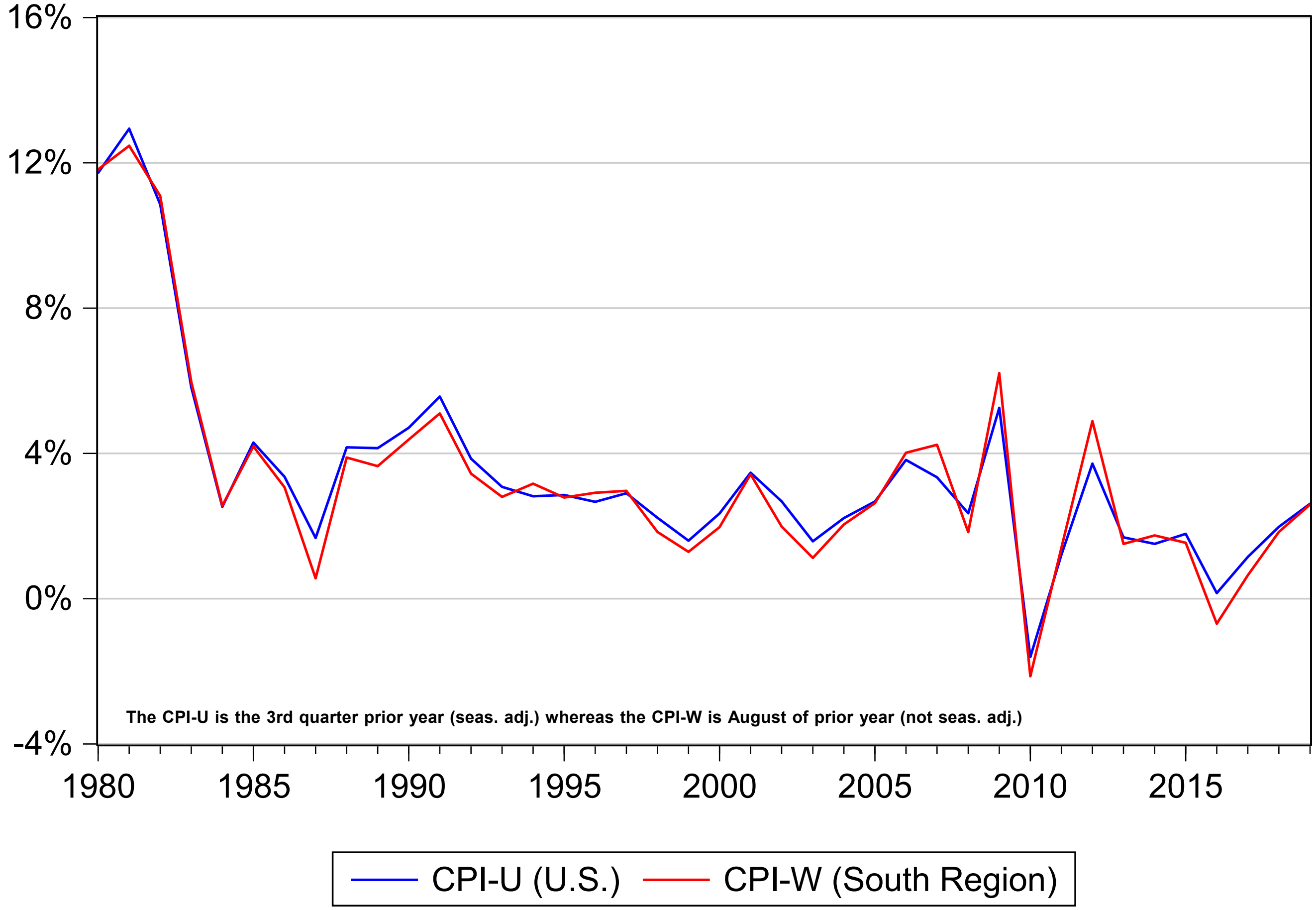
▪ Florida minimum wage held constant, even when CPI-W change over the year is negative

Under the proposed amendment the wage increase is effective September 30 of the prior year, as required by s. 448.110(4)(a), F.S

* CPI-Urban Wage Earners and Clerical Workers - All Items - South Region (not seasonally adjusted) - August of prior year

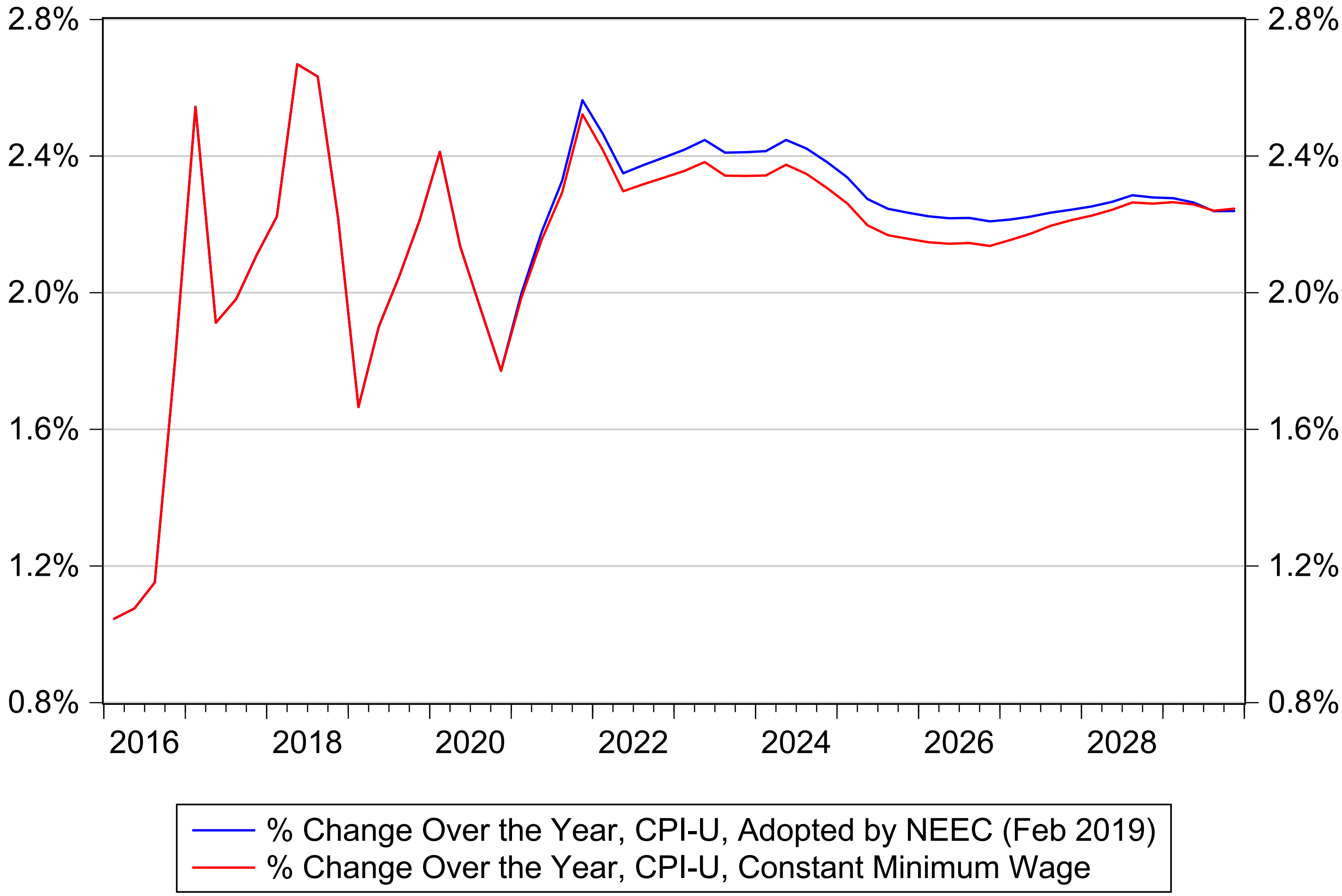
** CPI-All Urban Consumers - All Itemes (seasonally adjusted) - 3rd quarter of prior year. (February 2019 NEEC forecast) Forecast shown in blue shading; History shown in beige shading revised to match US Dept of Labor, Bureau of Labor Statistics, as of 3/27/2019.

CPI-U (U.S.) vs. CPI-W (South Region)
Growth-Over-The-Year
(For Discussion Purposes Only)



Adjusted CPI-U, Removing Minimum Wage

(For Discussion Purposes Only)



Tab 4

Reports

CENTER FOR ECONOMIC AND POLICY RESEARCH

Why Does the Minimum Wage Have No Discernible Effect on Employment?

John Schmitt

February 2013

Center for Economic and Policy Research
1611 Connecticut Avenue, NW, Suite 400
Washington, D.C. 20009
202-293-5380
www.cepr.net

Contents

About the Authors

John Schmitt is a Senior Economist at the Center for Economic and Policy Research in Washington, D.C.

Acknowledgements

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Executive Summary

The employment effect of the minimum wage is one of the most studied topics in all of economics. This report examines the most recent wave of this research – roughly since 2000 – to determine the best current estimates of the impact of increases in the minimum wage on the employment prospects of low-wage workers. The weight of that evidence points to little or no employment response to modest increases in the minimum wage.

The report reviews evidence on eleven possible adjustments to minimum-wage increases that may help to explain why the measured employment effects are so consistently small. The strongest evidence suggests that the most important channels of adjustment are: reductions in labor turnover; improvements in organizational efficiency; reductions in wages of higher earners ("wage compression"); and small price increases.

Given the relatively small cost to employers of modest increases in the minimum wage, these adjustment mechanisms appear to be more than sufficient to avoid employment losses, even for employers with a large share of low-wage workers.

Introduction

The employment effect of the minimum wage is one of the most studied topics in all of economics. This report examines the most recent wave of this research – roughly since 2000 – to determine the best current estimates of the impact of increases in the minimum wage on the employment prospects of low-wage workers. The weight of that evidence points to little or no employment response to modest increases in the minimum wage. The report also reviews evidence on a range of possible adjustments to minimum-wage increases that may help to explain why the measured employment effects are so consistently small.

Empirical Research on the Minimum Wage

The volume of research on the employment impact of the minimum wage is vast and a complete review is beyond the scope of this report. Instead, I provide a quick summary of the state of the debate as of the early 2000s and then concentrate on the main developments over the last decade.

Pre-2000s

In 1977, the Minimum Wage Study Commission (MWSC) undertook a review of the existing research on the minimum wage in the United States (and Canada), with a particular focus on the likely impact of indexing the minimum wage to inflation and providing a separate, lower, minimum for younger workers. Four years and $17 million later, the MWSC released a 250-page summary report[1] and six additional volumes of related research papers.[2] In their independent summary of the research reviewed in the MWSC, Brown, Gilroy, and Kohen, three economists involved in producing the report, distinguished between employment effects on: teenagers (ages 16-19), where they concluded that a 10 percent increase in the minimum wage reduced teen employment, most plausibly, from between zero and 1.5 percent; young adults (ages 20-24), where they believed the employment impact is "negative and smaller than that for teenagers"; and adults, where the "direction of the effect...is uncertain in the empirical work as it is in the theory."[3, 4] Their summary of the theoretical and empirical research through the late 1970s suggested that any "disemployment" effects of the minimum wage were small and almost exclusively limited to teenagers and possibly other younger workers.

For a decade, the MWSC's conclusions remained the dominant view in the economics profession. By the early 1990s, however, several researchers had begun to take a fresh look at the minimum wage. The principal innovations of what came to be known as "the new minimum wage research" were the use of "natural experiments" and cross-state variation in the "bite" of the minimum wage.

1 Minimum Wage Study Commission (1981)
2 For an overview of the workings of MWSC and a review of its main findings, see Eccles and Freeman (1982). For a lengthy review of the MWSC's finding, prepared by three economists involved in preparation of the MWSC report, see Brown, Gilroy, and Kohen (1982).
3 Brown, Gilroy, and Kohen (1982), p. 524.
4 The employment impact on adults is uncertain in theory because an increase in the minimum wage might encourage employers to replace some (presumably lower productivity) teenagers with more (presumably higher productivity) adults.

Natural experiments sought to reproduce in the real world some of the features of a laboratory experiment. In the context of the minimum wage, these natural experiments typically measured the employment impact of a single instance of a policy change (an increase in a state or the federal minimum wage) by comparing a group of workers directly affected by the change (teenagers in a state where the minimum wage increased, for example) with a similar group that was not affected (teenagers in a neighboring state where the minimum did not change).

Without a doubt, the most influential of the studies using a natural experiment was David Card and Alan Krueger's (1994) paper on the impact on fast-food employment of the 1992 increase in the New Jersey state minimum wage.[5] In advance of the 1992 increase in the New Jersey state minimum wage, Card and Krueger conducted their own telephone survey of fast-food restaurants in New Jersey and neighboring Pennsylvania. They repeated the survey after the increase had gone into effect and then compared the change in employment in New Jersey's restaurants (the minimum wage treatment group) with what happened in Pennsylvania (the control group). They found "no evidence that the rise in New Jersey's minimum wage reduced employment at fast-food restaurants in the state."[6, 7]

The "New Minimum Wage" research also emphasized research methods based on important differences in the "bite" of the federal minimum across the states. Any given increase in the federal minimum, the thinking went, should have more impact in low-wage states, where many workers would be eligible for an increase, than it would in high-wage states, where a smaller share of the workforce would be affected. Card, for example, divided the U.S. states into three groups – low-impact, medium-impact, and high-impact – according to the share of their teenage workforce that would be affected by the 1990 and 1991 increases in the federal minimum wage. His analysis concluded: "Comparisons of grouped and individual state data confirm that the rise in the minimum wage raised average teenage wages... On the other hand, there is no evidence that the rise in the minimum wage significantly lowered teenage employment rates..."[8]

Card and Krueger's book *Myth and Measurement: The New Economics of the Minimum Wage* is the best (though early) summary of these two strands of the "new minimum wage" research. Their detailed review of studies using a variety of methods and datasets to examine restaurant workers, retail employment, and teenagers, concludes: "The weight of this evidence suggests that it is very unlikely that the minimum wage has a large, negative employment effect."[9]

Myth and Measurement also inspired a considerable response from economists more critical of the minimum wage. David Neumark and William Wascher's book *Minimum Wages* brings together much of this critique, with an emphasis on their own work. In Neumark and Wascher's assessment, the most reliable recent research on the minimum wage has built on the earlier time-series analysis that informed the main conclusions of the MWSC. This new generation of time-series analysis typically

5 Other important studies along these lines include Card's (1992a) analysis of the impact of the 1988 increase in California's state minimum wage and Katz and Krueger's (1992) study of the impact of the 1990 and 1991 increases in the federal minimum wage.
6 Card and Krueger (1994), p. 792.
7 Economists David Neumark and William Wascher (2000) criticized Card and Krueger's study, arguing that the survey was poorly designed and implemented. Card and Krueger (2000) responded by confirming their original results using payroll records from a virtual census of fast-food restaurants in New Jersey and eastern Pennsylvania.
8 Card (1992b), p. 36.
9 Card and Krueger (1995), pp. 389-390.

applies modern econometric techniques to state-level data on teenagers (and sometimes less-educated workers). Neumark and Wascher's conclusion is that "...the preponderance of evidence supports the view that minimum wages reduce the employment of low-wage workers."[10]

Since the early 2000s

At the turn of the century, the minimum-wage debate had two poles: on the one side, researchers broadly identified with the "new minimum-wage research" (though without Card and Krueger, who, since their 2000 re-analysis of their famous New Jersey fast-food study, have not returned to write on the minimum wage); and critics of the minimum wage and the new minimum-wage research, the most prolific of whom have been Neumark and Wascher. The last decade has seen a continued outpouring of research from both camps, and the emergence of what economist Arindrajit Dube has called a "fourth generation" of research on the minimum wage that "tries to make sense of the sometimes contradictory evidence."[11]

In the next two sections of this report, I first summarize the findings of two statistical "meta-studies" (studies of studies) and two, more qualitative, literature reviews of this research; then, take a closer look at several of the most important and influential studies published in the last decade.

Meta-studies

Meta-studies are "studies of studies" that use a set of well-defined statistical techniques to pool the results of a large number of separate analyses. Meta-study techniques effectively increase the amount of data available for analysis and can provide a much sharper picture of statistical relationships than is possible in any individual study. Meta-studies are widely used in medicine, where the results of many small clinical trials can be combined to produce much more accurate estimates of the effectiveness of different kinds of treatments.

Hristos Doucouliagos and T. D. Stanley (2009) conducted a meta-study of 64 minimum-wage studies published between 1972 and 2007 measuring the impact of minimum wages on teenage employment in the United States. When they graphed every employment estimate contained in these studies (over 1,000 in total), weighting each estimate by its statistical precision, they found that the most precise estimates were heavily clustered at or near zero employment effects (see **Figure 1**). Doucouliagos and Stanley's results held through an extensive set of checks, including limiting the analysis to what study authors' viewed as their best (usually of many) estimates of the employment impacts, controlling for possible correlation of estimates within each study, and controlling for possible correlation of estimates by each author involved in multiple studies. Doucouliagos and Stanley concluded that their results "...corroborate [Card and Krueger's] overall finding of an insignificant employment effect (both practically and statistically) from minimum-wage raises."[12] In

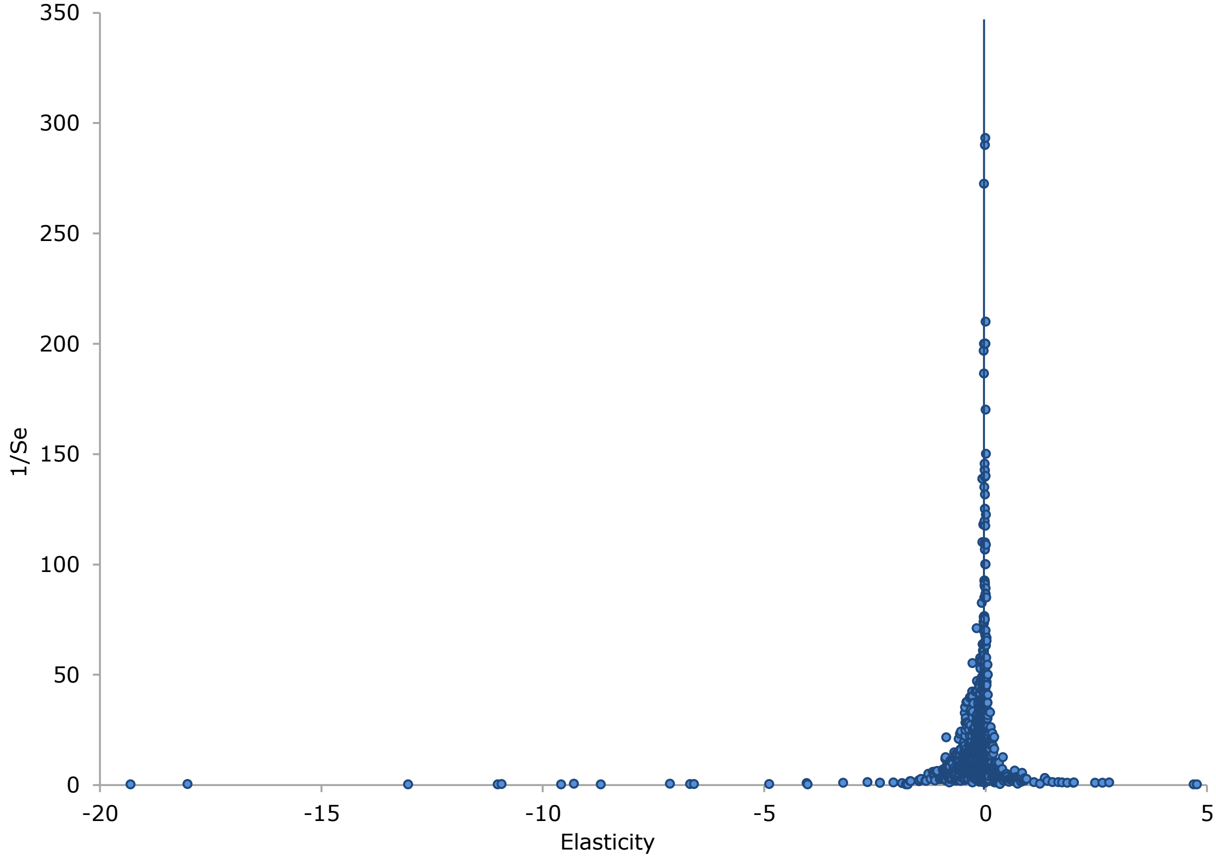
10 Neumark and Wascher (2008), p. 104.

11 Dube detects "...four generations of minimum wage research: the older time series literature, the first wave of the "new minimum wage" research that featured both case study and state-panel approaches, a third generation of follow-up work largely based on these two methodologies, and a fourth generation of recent work that tries to make sense of the sometimes contradictory evidence." (2011, p. 763)

12 Doucouliagos and Stanley (2009), p. 422. Doucouliagos and Stanley put the size of the effects they find into perspective: "A 10 per cent increase in the minimum wage reduces employment by about 0.10 per cent... But even if this adverse employment effect were true, it would be of no practical relevance. An elasticity of -0.01 has no meaningful policy implications. If correct the minimum wage could be doubled and cause only a 1 per cent decrease in teenage employment." (2009, pp. 415-16)

their view: "Two scenarios are consistent with this empirical research record. First, minimum wages may simply have no effect on employment... Second, minimum-wage effects might exist, but they may be too difficult to detect and/or are very small."[13]

FIGURE 1
Trimmed Funnel Graph of Estimated Minimum-Wage Effects (n = 1,492)



Source: Doucouliagos and Stanley (2009).

Paul Wolfson and Dale Belman have carried out their own meta-analysis of the minimum wage, focusing on studies published only since 2000. They identified 27 minimum wage studies that produced the necessary elasticity estimates and corresponding standard errors, yielding 201 employment estimates in total. They then produced a range of meta-estimates, controlling for many features of the underlying studies, including the type of worker analyzed (teens or fast food workers), whether the study focused on the supply or the demand side of the labor market, who the authors of the study were, and other characteristics. The resulting estimates varied, but revealed no statistically significant negative employment effects of the minimum wage: "The largest in magnitude

13 Doucouliagos and Stanley (2009), p. 422. Doucouliagos and Stanley also "find strong evidence of publication selection for significantly negative employment elasticities" (2009, p. 422) They conclude: "Even under generous assumptions about what might constitute 'best practice' in this area of research, little or no evidence of an adverse employment effect remains in the empirical research record, once the effects of publication selection are removed." (p. 423)

are... positive [and] statistically significant... Several are economically irrelevant though statistically significant and several others [are] slightly larger but...statistically insignificant."[14]

Reviews

Meanwhile, Neumark and Wascher (2006, 2007) conducted a qualitative review of the research since the early 1990s on the employment effects of the minimum wage in the United States, other OECD countries, several Latin American countries, and Indonesia.[15] In their summary remarks, focusing on the U.S. experience, they note:

> "What may be most striking to the reader who has managed to wade through our lengthy review of the new minimum wage research is the wide range of estimates of the effects of the minimum wage on employment, especially when compared to the review of the earlier literature by Brown et al. in 1982 [for the Minimum Wage Study Commission]. For example, few of the studies in the Brown et al. survey were outside of the consensus range of −.1 to −.3 for the elasticity of teenage employment with respect to the minimum wage. In contrast, even limiting the sample of studies to those focused on the effects of the minimum wage of teenagers in the United States, the range of studies comprising the new minimum wage research extends from well below −1 to well above zero."[16]

Based on their subjective weighting of the quality of the research and the reliability of the resulting estimates, Neumark and Wascher conclude:

> "Although the wide range of estimates is striking, the oft-stated assertion that the new minimum wage research fails to support the traditional view that the minimum wage reduces the employment of low-wage workers is clearly incorrect. Indeed, in our view, the preponderance of the evidence points to disemployment effects."[17]

By their calculations, of the 33 studies "providing the most credible evidence; 28 (85 percent) ... point to negative employment effects."[18]

The Neumark and Wascher review, however, is considerably more subjective and arguably less relevant to the United States than the two meta-studies discussed earlier. Only 52 of the 102 studies reviewed by Neumark and Wascher analyzed U.S. data. Of these, Neumark and Wascher designated 19 as "most credible," five of which were their own studies.[19] The Neumark and Wascher (2006) review also excludes several important papers that were not published until after the review was completed, including the important contributions of Arindrajit Dube, William Lester, and Michael Reich (2010) and Sylvia Allegretto, Dube, and Reich (2011) (to which we will return to below).[20]

14 Wolfson and Belman (forthcoming), p. 10.

15 An abbreviated version of their findings, with a few additional studies added, appears in chapter three of Neumark and Wascher (2008). For a critical review of Neumark and Wascher's book, see Dube (2011).

16 Neumark and Wascher (2006), p. 120.

17 Neumark and Wascher (2006), p. 121.

18 Neumark and Wascher (2006).

19 Following the procedure that Neumark and Wascher appear to have used, I count Sabia (2006) as two studies because it has two separate entries in their Table 1.

20 In their subsequent book, Neumark and Wascher (2008) do critique a pre-publication version of the Dube, Lester, and Reich paper.

Wolfson and Belman (forthcoming) also produced an extensive qualitative review of minimum wage research since 2000, including a significant number of studies published too late for inclusion in Neumark and Wascher (2006, 2008). Of the studies they reviewed, 40 analyzed U.S. data. Fourteen of these found negative employment effects; thirteen found no effects; one found positive effects; and twelve, a mixture of negative, positive, and no effects. To sort out these conflicting findings, Wolfson and Belman appealed to their meta-study, which as noted earlier, concluded that there were no statistically and economically meaningful employment losses associated with the minimum wage.

A closer look at several key recent studies

This section takes a closer look at several of the most important studies conducted over the last decade.

Dube, Lester, and Reich (2010)

Probably the most important and influential paper written on the minimum wage in the last decade was Dube, Lester, and Reich (2010)'s study,[21] which offered a comprehensive reappraisal of both the new minimum wage research and its critics. The study was built around a key methodological innovation, which essentially generalized Card and Krueger's New Jersey study to make it nationally representative, and identified a significant weakness in much of the earlier minimum-wage research based on the analysis of state employment patterns, which had failed to control for regional differences in employment growth that were unrelated to the minimum wage.

The most convincing critique of Card and Krueger's (1994, 2000) study of the increase in the New Jersey minimum wage (relative to Pennsylvania, where the minimum wage did not go up) was that it is difficult to generalize from a single case study. Even a perfect experiment will have random error that could affect the results in a single experiment. Imagine that the minimum wage had a small, but real, negative employment effect. Random errors will lead the results of separate tests to be distributed around this hypothetical negative employment effect, sometimes producing a larger disemployment effect than the "true" level, sometimes producing a smaller disemployment effect than what is "true" – even zero or positive measured disemployment effects. By this thinking, Card and Krueger's experiment could have been perfectly executed, but still represent only one result from a distribution of possible outcomes. Absent other information, the best estimate of the true effect of the minimum wage would be Card and Krueger's actual results, but we cannot convincingly rule out, based on that single case, that the effects were in truth larger or smaller than what was observed in the case of New Jersey in 1992.

In recognition of this problem, Dube, Lester and Reich (2010) essentially replicated Card and Krueger's New Jersey-Pennsylvania experiment thousands of times, by comparing employment differences across contiguous U.S. counties with different levels of the minimum wage. The three economists carefully constructed a data set of restaurant employment in every quarter between 1990 and 2006 in the 1,381 counties in the United States for which data were available continuously over the full period.[22] They also matched these employment data with the level of the federal or state minimum wage (whichever was higher) in the county in each quarter of each year in the sample. They then compared restaurant employment outcomes across a subset of 318 pairs of bordering

21 The paper first circulated in 2007.

22 They drew the data from the Quarterly Census of Employment and Wages, which collects data from unemployment insurance records, a virtual census of employees in the United States. There were a total of 3,081 counties in total in the United States over the period they analyzed.

counties where the prevailing minimum wage could differ, depending on the level of the federal and state minimum wage.

Their methodology effectively generalizes the Card and Krueger New Jersey-Pennsylvania study, but with several advantages. First, the much larger number of cases allowed Dube, Lester, and Reich to look at a much larger distribution of employment outcomes than was possible in the single case of the 1992 increase in the New Jersey minimum wage. Second, since they followed counties over a 16-year period, the researchers were also able to test for the possibility of longer-term effects. Finally, because the relative minimum wage varied across counties over time, the minimum wage in a particular county could, at different points in time, be lower, identical to, and higher than the minimum wage in its pair, providing substantially more experimental variation than in the New Jersey-Pennsylvania (and many similar) studies. Using this large sample of border counties, and these statistical advantages over earlier research, Dube, Lester, and Reich "...find strong earnings effects and no employment effects of minimum wage increases."[23]

Dube, Lester, and Reich's study also identified an important flaw in much of the earlier minimum-wage research based on the analysis of state-level employment patterns. The three economists demonstrated that overall employment trends vary substantially across region, with overall employment generally growing rapidly in parts of the country where minimum wages are low (the South, for example) and growing more slowly in parts of the country where minimum wages tend to be higher (the Northeast, for example). Since no researchers (even the harshest critics of the minimum wage) believe that the minimum wage levels prevailing in the United States have had any impact on the *overall* level of employment, failure to control for these underlying differences in regional employment trends, Dube, Lester, and Reich argued, can bias statistical analyses of the minimum wage. Standard statistical analyses that do not control for this "spatial correlation" in the minimum wage will attribute the better employment performance in low minimum-wage states to the lower minimum wage, rather than to whatever the real cause is that is driving the faster overall job growth in these states (good weather, for example). Dube, Lester, and Reich use a dataset of restaurant employment in all counties (for which they have continuous data from 1990 through 2006), not just those that lie along state borders and are able to closely match earlier research that finds job losses associated with the minimum wage. But, once they control for region of the country, these same earlier statistical techniques show no employment losses. They conclude: "The large negative elasticities in the traditional specification are generated primarily by regional and local differences in employment trends that are unrelated to minimum wage policies."[24, 25]

Independently of Dube, Lester, and Reich, economists John Addison, McKinley Blackburn, and Chad Cotti used similar county level data for the restaurant-and-bar sector to arrive at similar conclusions. Addison, Blackburn, and Cotti found no net employment effect of the minimum wage in the restaurant-and-bar sector. More importantly, using reasoning similar to Dube, Lester, and Reich, they also concluded that the standard state panel-data techniques that have typically yielded negative employment effects of the minimum wage appear to be biased toward finding that result: "Our evidence does not suggest that minimum wages reduce employment once controls for trends in county-level sectoral employment are incorporated. Rather, employment appears to exhibit an

23 Dube, Lester, and Reich (2010), p. 961.
24 Dube, Lester, and Reich (2010), p. 962.
25 Note that several prominent studies since 2000 that use state panel data and estimation techniques of this type do not control for or address the "spatial heterogeneity" identified by Dube, Lester, and Reich. See, for example, Burkhauser, Couch, and Wittenburg (2000), Neumark and Wascher (2007), and Sabia (2009).

independent downward trend in states that have increased their minimum wages relative to states that have not, thereby predisposing estimates towards reporting negative outcomes."[26]

Allegretto, Dube, and Reich (2011)

Sylvia Allegretto, Dube, and Reich (2011) applied the insights of Dube, Lester, and Reich (2010) to teen employment over the period 1990-2009. Their work made at least two important contributions to the policy debate. First, they analyzed teen employment, rather than industry employment, making their results more directly comparable to the bulk of earlier research on the minimum wage. Second, they included data covering the deep recession that ran from December 2007 through June 2009, allowing them to measure any possible interactions between the minimum wage and strong economic downturns.[27]

Allegretto, Dube, and Reich analyzed data on teenagers taken from the Current Population Survey (CPS) for the years 1990 through 2009.[28] Because the CPS sample is smaller than the QCEW data used in the county-analysis, Allegretto, Dube, and Reich instead tracked teen employment at the state level. When they produced standard statistical analyses of the kind used in much of the research since the mid-1990s on teen employment, the three economists found results similar to those found in that earlier research (a 10 percent increase in the minimum wage reduces teen employment slightly more than 1 percent). But, once they controlled for different regional trends, the estimated employment effects of the minimum wage disappeared, turning slightly positive, but not statistically significantly different from zero.

Allegretto, Dube, and Reich also investigated whether the impact of the minimum wage is greater in economic downturns. They "...do not find evidence that the effects are systematically different in periods of high versus low overall unemployment."[29]

Hirsch, Kaufman, and Zelenska (2011)

Barry Hirsch, Bruce Kaufman, and Tatyana Zelenska (2011) studied the impact of the 2007-2009 increases in the federal minimum wage on a sample of 81 fast-food restaurants in Georgia and Alabama. In principle, the size of the minimum-wage increase was identical across all the restaurants studied, but, in practice, the impact of the increase varied because there was significant variation in pay across the restaurants. Their paper makes an important contribution to the policy debate because it seeks to shift the discussion toward understanding why, in their words, "[d]espite decades of research, pinning-down the labor market effects of [the minimum wage] has proven elusive."[30] In particular, they propose looking at a range of possible "channels of adjustment" to minimum wage increases and examine evidence on some of these potential channels.

Hirsch, Kaufman, and Zelenska gathered two kinds of data. The first were electronic payroll data obtained from the three owners of the 81 establishments. The data covered a three-year period from January 2007 through December 2009, which brackets the July 2007, July 2008, and July 2009

26 Addison, Blackburn, and Cotti (2012), p. 412. This research first circulated in 2008, at about the same time that Dube, Lester, and Reich's work first appeared.
27 Of course, Dube, Lester, and Reich (2010) included data covering the 1990-91 and 2001 recessions.
28 The detailed data on restaurant employment that Dube, Lester, and Reich (2010) used in their study do not contain information on workers' characteristics such as age, so Allegretto, Dube, and Reich (2011) used the smaller CPS data set.
29 Allegretto, Dube, and Reich (2011), p. 238.
30 Hirsch, Kaufman, and Zelenska (2011), p. 1.

increases in the federal minimum wage. These data allowed the researchers to conduct before-and-after tests of changes in wages and employment at the restaurants. If the minimum wage had a negative effect on employment, they would expect to observe larger increases in wages at the lower-wage restaurants, accompanied by bigger declines in employment. In fact, they found: "...in line with other recent studies, that the measured employment impact is variable across establishments, but overall not statistically distinguishable from zero. The same absence of a significant negative effect is found for employee hours, even when examined over a three-year period."[31]

Hirsch, Kaufman, and Zelenska also collected data through separate interviews with managers and employees, using a survey designed to investigate channels of adjustment to the minimum wage – other than changes in employment or hours.[32] The other channels they considered included: price increases; changes to the internal wage structure (including slower pay increases for higher-wage workers); reductions in turnover; "operational and human resource efficiencies;" reductions in non-labor costs; reductions in customer service; and lower profits.

After analyzing the establishment data on wages, employment, and hours, Hirsch, Kaufman, and Zelenska concluded that while wages did rise after the federal minimum-wage increase, any employment and hours changes were not statistically distinguishable from zero. Based on the rest of the information they gathered in their survey and interviews with employers and employees, they write:

> "...our study offers a new [three-part] explanation for the small and insignificant [minimum wage] employment effects found in the literature... first... is that even large increases in the [minimum wage] may be modest as compared to other cost increases that business owners must routinely offset or absorb... The second is that a [minimum-wage] cost increase flows through more adjustment channels than economists have typically considered. And the third is that managers regard employment and hours cuts as a relatively costly and perhaps counter-productive option, regarding them as a last resort."[33]

Hirsch, Kaufman, and Zelenska's empirical investigation of the wage, employment, and other impacts of the federal minimum wage is subject to a number of reasonable critiques. The most important of these (as was the case with Card and Krueger's 1994 and 2000 New Jersey studies) is that it is difficult to generalize from only one minimum wage experiment, particularly when the analysis is based on the experience of only 81 restaurants, all in the same chain, all owned by a only three franchisees in just two states. Nevertheless, the employment effects they find lie at the consensus estimate in the two most recent meta-studies: little or no negative employment outcomes. The key contribution of this paper, however, is its focus on the wide range of ways that employers respond to minimum-wage increases other than adjusting employment or hours.

Sabia, Burkhauser, and Hansen (2012)

Joseph Sabia, Richard Burkhauser, and Benjamin Hansen (2012) used research methods similar in spirit to the original Card and Krueger New Jersey study to analyze the effects of an increase (in three steps) in the New York state minimum wage from $5.15 per hour in 2004, to $7.15 per hour in 2007 (a cumulative 39 percent increase). They compared the effect of the increase on the

31 Hirsch, Kaufman, and Zelenska (2011), p. 32.
32 In the summer of 2009, they interviewed or surveyed 66 of the 81 managers and 1,649 of the 2,640 employees (Hirsch, Kaufman, and Zelenska, 2011, p. 12).
33 Hirsch, Kaufman, and Zelenska (2011), p. 33.

employment of less-educated 16-to-29 year olds in New York with similar workers in nearby Pennsylvania, Ohio, and New Hampshire, which experienced no increase in the minimum wage over the same period. The three economists also compared employment outcomes for less-educated 16-to-29 year olds in New York with better-educated New York state workers of the same age.[34]

Their analysis shows that the minimum-wage increases in New York raised the wages of less-skilled younger workers relative both to similar workers in the control states and to better-educated workers of the same age in New York state. But, they also found: "...robust evidence that raising the New York minimum ... significantly reduced employment rates of less-skilled, less-educated New Yorkers." Their estimates implied "...a median elasticity of around -0.7, large relative to consensus estimates ... of -0.1 to -0.3 found in the literature."[35]

The Sabia, Burkhauser, and Hansen study, however, is subject to the same critique applied to Hirsch, Kaufman, and Zelenska (and Card and Krueger before them). Sabia, Burkhauser, and Hansen analyzed only one experience of the minimum wage. Even if the effects of the minimum wage were, in truth, zero, we would expect to see a distribution of estimates around zero, including both positive and negative estimates. As Doucouliagos and Stanley demonstrated in their large meta-study of employment effects through the middle of the 2000s, the minimum-wage literature on teenagers showed a range of positive and negative effects, but also a large spike of the most accurate estimates at, or very near, zero. Wolfson and Belman’s meta-study, which focused on the period from about 1990 through 2010, confirms Doucouliagos and Stanley's findings with more recent research. Given how far the Sabia, Burkhauser, and Hansen estimates lie outside this consensus range, the burden of proof would seem to fall on Sabia, Burkhauser, and Hansen to explain why their study of a single experiment with the minimum wage should outweigh the cumulative experience of scores of studies of the U.S. minimum wage since the early 1990s.

Adjustment Channels

The standard competitive model makes stark predictions about the employment effects of the minimum wage: a binding minimum wage will price at least some low-wage workers out of jobs and will unambiguously lower employment. Why, then, does the bulk of the best statistical evidence on the employment effects of the minimum wage cluster at zero or only small employment effects? This section attempts to answer that question, adopting and adapting the simple "channels of adjustment" framework proposed by Hirsch, Kaufman, and Zelenska.

Hirsch, Kaufman, and Zelenska argue for a "channels of adjustment" approach through which cost increases associated with the minimum wage change "...the behavior of firms, with impacts on workers, consumers, owners, and other agents."[36] Hirsch, Kaufman, and Zelenska analyze the possible channels of adjustment emphasized by three different theoretical approaches to the minimum wage: the standard competitive model; the "institutional" model; and the (dynamic) "monopsony" model.

34 Sabia, Burkhauser, and Hansen (2012) also constructed a synthetic control group of individuals drawn from a larger collection of states, designed to most closely match the characteristics of the "treated" New York state group. These tests produced qualitatively similar results to the ones discussed here.

35 Sabia, Burkhauser, and Hansen (2012), p. 23.

36 Hirsch, Kaufman, and Zelenska (2011), p. 1.

Competitive model

The competitive model generally emphasizes adjustment through declining employment (or hours). But, the same competitive model also allows for other possible channels of adjustment, including higher prices to consumers, reductions in non-wage benefits such as health insurance and retirement plans, reductions in training, and shifts in the composition of employment. If the only channel of adjustment available is employment, the competitive model implies that binding minimum wages will reduce employment. But, the existence of other possible channels of adjustment means that minimum wages could have little or no effect on employment, even within a standard competitive vision of the labor market.

Institutional model

The institutional model, as Hirsch, Kaufman, and Zelenska note, was the "dominant paradigm for evaluating the minimum wage" from the time the federal minimum wage was first established in the 1930s through the decade of the 1950s. The institutional view has several key features, including: "rejection of a well-defined downward sloping labor demand curve; labor markets that are imperfectly competitive, institutionally segmented, socially embedded, and prone to excess supply; and the importance of technological and psycho-social factors in firm-level production systems and internal labor markets ... as determinants of cost and productivity."[37]

This institutional approach to the labor market allows for several additional channels of adjustment to a minimum-wage increase. Probably the most important of these concern productivity. Employers may respond to a minimum-wage increase by exerting greater managerial effort on productivity-enhancing activities, including the reorganization of work, setting higher performance standards, or demanding greater work intensity. In the competitive model, firms are assumed already to be operating at peak efficiency, but in the institutional framework, firms are assumed to often operate below their peak efficiency because it is costly to managers and to workers to identify, implement, and maintain practices that continuously maximize efficiency.[38] In this context, a minimum-wage increase gives new incentives to employers to undertake additional productivity-improving practices. Alternatively, a higher minimum wage may also boost productivity through "efficiency wage" effects. A strong theoretical and empirical basis exists for the idea that wages set above the competitive market rate can induce workers to work harder,[39] either to ensure that they keep their job[40] or in reciprocity for the higher wages paid.[41, 42]

Another important potential channel of adjustment in the institutional model is the possibility that a higher minimum wage, by increasing spending power of low-wage workers, might act as a form of economic stimulus, spurring greater demand for firms' output, at least partially offsetting the rise in wage costs.[43]

37 Hirsch, Kaufman, and Zelenska (2011), p. 5. For an excellent discussion of the institutional framework as it relates to the minimum wage, see Kaufmann (2010).
38 Kaufman (1999, 2010).
39 Katz (1986).
40 Shapiro and Stiglitz (1984).
41 Akerlof (1982).
42 See Hirsch, Kaufman, and Zelenska (2011), pp. 5-7 for additional possible channels of adjustment under the institutional model.
43 See Hall and Cooper (2012).

As a result of these various alternative channels of adjustment, the institutional model suggests that the minimum wage "may have, particularly in the short-run, an approximately zero or small positive employment effect."[44]

Dynamic monopsony model

The dynamic monopsony model is a third theoretical approach to the labor market that opens up additional channels of adjustment.[45] The most important new channel is the possibility that the minimum wage reduces the costs of turnover to low-wage employers.

The key difference between the standard competitive model and the monopsony model concerns the circumstances employers face when it comes to recruiting and retaining staff. In the competitive model, employers can hire all the labor they desire by paying the prevailing market wage; and, in the event that a worker quits, employers can instantly replace that worker with an identically productive worker at the same wage. By contrast, in the dynamic monopsony model, employers, even those operating in low-wage labor markets, face real costs associated with hiring new workers. These costs flow from inevitable frictions in the labor market. Workers incur costs (time, effort, financial expenditures) to find job openings; and, workers must limit their job searches to openings that fit their geographic, transportation, and scheduling constraints. To overcome these frictions, employers must either pay above the going wage (to draw extra attention to the particular vacancy) or wait (with implied costs in lost output) until they are able to fill the vacancy with a worker willing to accept that particular opening at the going rate.

At first glance, these frictions seem to work against low-wage employers, who must pay higher wages to attract additional workers. In reality, however, these frictions put low-wage workers at a significant disadvantage relative to their employers. Employers must pay above the going rate to fill vacancies quickly (or wait longer until the vacancy is filled at the going rate) because unemployed workers face real barriers (transportation, scheduling, information, financial, and others) to locating suitable jobs. Low-wage employers are well-positioned to take advantage of these difficulties. Even though employers must pay new workers a higher wage to fill a vacancy quickly, employers are able to pay their current workers – who had to overcome various frictions to find their current job – below their "marginal product."

In the monopsony model, employers are unlikely to pay higher wages in order to fill vacancies because they would then have to raise the pay of their existing workers to match the pay offered to their last hire. As a result, in monopsonistic settings, employers habitually operate with unfilled vacancies, rather than raising the wage for their entire workforce. In this context, raising the minimum wage can actually increase employment by raising the wages of the existing workforce to the "competitive" level (no existing jobs are lost because these workers were being paid below their "marginal product") and filling existing vacancies (which increases overall employment).[46]

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44 Hirsch, Kaufman, and Zelenska (2011), p. 6, citing Lester (1946, 1960).

45 Traditional monopsony models assume that the labor market is characterized by a single employer who hires all of the large number of possible workers. The standard example is an isolated "company town" with many workers and only one large employer. By using the term "dynamic monopsony" economists are attempting to keep some of the analytical features of the standard monopsony model, while emphasizing that the source of the monopsony power does not flow from being a single employer, but rather from the dynamics –especially, the frictions– of the low-wage labor market.

46 For a detailed, technical discussion of dynamic monopsony, see Manning (2003).

TABLE 1
Total wage bill impact of recent minimum-wage increases

	Minimum wage (nominal dollars)	Legislated increase (percent)	Number of full-time equivalent workers affected (thousands)	Share of all employees (percent)	Share of all hours worked (percent)	Average hourly increase for workers receiving an increase (dollars)	Total annual cost of wage increase (billions of dollars)	Total annual wage bill, in sweep (billions of dollars)	Total annual wage bill, all workers (billions of dollars)	Total increase as share of wage bill, in sweep (percent)	Total increase as share of wage bill, all workers (percent)
1989	3.35										
1990	3.80	13.4	3,612,491	4.8	3.6	0.32	2.4	26.2	2,267.4	9.2	0.11
1991	4.25	11.8	4,199,152	5.6	4.2	0.34	3.0	34.2	2,369.0	8.7	0.13
1995	4.25										
1996	4.75	11.8	2,959,023	3.8	2.8	0.41	2.5	26.8	3,068.8	9.4	0.08
1997	5.15	8.4	4,902,738	6.0	4.5	0.26	2.7	49.9	3,242.7	5.3	0.08
2006	5.15										
2007	5.85	13.6	1,214,946	1.3	1.0	0.49	1.2	13.6	5,317.6	9.1	0.02
2008	6.55	12.0	1,936,789	2.1	1.6	0.45	1.8	24.5	5,536.5	7.4	0.03
2009	7.25	10.7	2,407,638	2.7	2.0	0.37	1.9	34.5	5,546.5	5.4	0.03

Notes: Authors' analysis of Current Population Survey.

Size of Adjustment

The three distinct theoretical approaches to the minimum wage suggest a large number of possible channels of adjustment. Before reviewing the evidence on these various channels, however, it is useful to have an idea of the size of the adjustment that a typical minimum-wage increase requires.

Table 1 presents data on the wage costs of last three rounds of federal minimum wage increases: the 1990-91 increases (from $3.35 to $4.25); the 1996-97 increases (from $4.25 to $5.15); and the 2007-2009 increases (from $5.15 to $7.25). Each of the annual increases in the statutory level of the minimum wage was in the range of about 10 percent per year (a low of 8.4 percent to a high of 13.6 percent – see column two). The average increase in the wage costs of affected workers, however, was in all cases smaller than the increase in the statutory rate, ranging from a low of 5.3 percent to a high of 9.4 percent (see next-to-last column). The lower average actual increase simply reflects that not all of the workers who receive a pay boost after a minimum-wage increase receive the full increase (because they are already earning something above the old federal minimum, but below the new federal minimum). Even more importantly, the total direct wage cost of each of these minimum-wage increases was tiny relative to the total wage bill paid by employers – consistently less than 0.1 percent of total wages paid. Relative to the wage costs of minimum-wage workers, the size of each recent minimum-wage increases was modest (between about 5 and 10 percent of total wage costs for minimum-wage workers).[47] Relative to the total wage costs in the economy (that is including the wages of all employees, not just those earning the minimum wage), the wages costs of recent minimum-wage increases are very small.[48]

The size of these increases is directly relevant to the evaluation of possible channels of adjustment. For the typical minimum-wage increase, one or more of these alternative channels of adjustment – whether they are related to productivity increases, cuts in profits, reductions in earnings of higher earners, higher prices to consumers, or other mechanisms – must cope with what are relatively small total cost increases, when expressed as either a share of the total wages paid to minimum-wage workers or as a share of the total wages paid to all workers.

Possible Channels

1. Reduction in hours worked

The minimum wage does not raise the cost of hiring *workers* – it raises the cost of hiring an *hour of work* performed by those workers. Even within the competitive framework, employers might choose to respond to a minimum-wage increase by reducing workers' hours, rather by reducing the total number of workers on payroll.[49]

If firms were to adjust entirely by cutting hours (that is, they used no other adjustment channel), a minimum-wage increase could still raise the living standard of minimum-wage workers, even in a competitive model of the labor market. Imagine, for example, that the minimum wage increased wages by 20 percent and lowered the number of hours worked by 10 percent. A part-time worker working, say 20 hours per week, would experience a 10 percent fall in hours to, 18 hours a week, but

47 Moreover, these increases were typically preceded and followed by years when the minimum wage did not change at all.
48 The cost of minimum-wage increases is even smaller when expressed as a share of total compensation – wages plus non-wage benefits such as health insurance.
49 Michl (2000).

would be paid 20 percent more for each of these 18 hours worked, for a net increase in weekly pay of 8 percent. Even if the reduction in hours was so large that it exactly offset the increase in the hourly wage, minimum-wage workers would still be better off after the increase because they would be earning exactly what they made before, but would now be working fewer hours per week to earn it. Hours adjustments would only reduce a worker's standard of living if the fall in hours were steeper than the rise in wages.[50]

The empirical evidence on hours effects is not conclusive. Based on indirect evidence, Dube, Lester, and Reich's study of the minimum wage across contiguous counties tentatively suggests that "the fall in hours is unlikely to be large."[51] Neumark and Wascher's review of the evidence concludes that "the question of how employers adjust average hours in response to a minimum wage increase is not yet resolved."[52]

2. Reductions in non-wage benefits

Within the competitive framework, employers might respond to a minimum-wage increase by lowering the value of non-wage benefits, such as health insurance and pension contributions.

The empirical evidence, however, points to small or no effects along these lines. Based on their review of research as of the mid-1990s, Card and Krueger conclude: "The quantitative importance of nonwage offsets in response to a minimum-wage increase is an open question."[53] Their own study of fast-food restaurants in New Jersey showed no tendency for employers to cut the most common nonwage benefit offered, which was free or low-priced meals.[54] Simon and Kaestner's somewhat more recent review of the "relatively few studies of the effect of minimum wages on fringe benefits and working conditions"[55] also reports small or no effects of the minimum wage on nonwage benefits.[56] Simon and Kaetner's own analysis of data from the Current Population Survey found that: "...minimum wages have had no discernible effect on fringe benefits (specifically, on the receipt of health insurance, on whether the employer paid the whole premium cost, on whether family health insurance was provided, and on receipt of employer pensions)."[57]

3. Reductions in training

Another channel of adjustment consistent with the competitive framework is the possibility that employers might reduce their expenditures on job training for low-wage workers.

The empirical evidence is not conclusive. In their review of the recent research on the minimum wage and training, Neumark and Wascher write: "Summing up all of the evidence on training, we

50 Given the high level of turnover in many low-wage jobs, the distinction between employment and hours adjustments might be less important than it first seems. If low-wage jobs are typically of short duration and low-wage workers cycle in and out of low-wage jobs during the course of the year, even a reduction in the number of low-wage jobs might, in practice, look to low-wage workers like only a reduction in hours. Low-wage workers would spend somewhat more time in between jobs, but be paid more for each job they did land. As a result, depending on the elasticities involved (the responsiveness of employment to minimum-wage changes), their annual hours could fall, but their annual incomes could rise.
51 Dube, Lester, and Reich (2010), p. 956.
52 Neumark and Wascher (2008), p.78.
53 Card and Krueger (1995), p. 169.
54 Card and Krueger (1994).
55 Simon and Kaestner (2004), p. 53.
56 Citing Wessels (1980); Alpert (1986); Card and Krueger (1994); Royalty (2000).
57 Simon and Kaestner (2004), p. 67.

can only conclude that the evidence is mixed. Our own research tends to find negative effects of minimum wages on training, but most of the other recent research finds little evidence of an effect in either direction."[58]

One reason that the research has not identified clear effects of the minimum wage on training may be that the institutional model provides a better description of the labor market than the standard competitive model. In the institutional model, employers may respond to a higher wage floor by *increasing* training for low-wage workers in order to raise their productivity to a level commensurate with their new, higher earnings.[59]

4. Changes in employment composition

Employers may adjust to a higher minimum wage by "upgrading" the skill level of their workforce, rather than cutting the level of their staffing. This process could conceivably work against the employment prospects of less-educated and less-experienced workers, especially, the argument goes, black and Latino teens. As Walter E. Williams argues:

> "...when faced with legislated wages that exceed the productivity of some workers, firms will make adjustments in their use of labor. One adjustment is not only to hire fewer youths but also to seek among them the more highly qualified candidates. It turns out for a number of socioeconomic reasons that white youths, more often than their black counterparts, have higher levels of educational attainment and training. Therefore, a law that discriminates against low-skilled workers can be expected to place a heavier burden on black youths than on white ones."[60]

Donald Deere, Kevin Murphy, and Finis Welch (1995) and Sabia, Burkhauser, and Hansen (2012) make arguments along these lines in their studies of workers with less than a high school degree.[61]

As Allegretto, Dube, and Reich note, however, a theoretical case can be made that minimum wages might instead *improve* the relative employment prospects of disadvantaged workers: "An alternative view suggests that barriers to mobility are greater among minorities than among teens as a whole. Higher pay then increases the returns to worker search and overcomes existing barriers to employment that are not based on skill and experience differentials."[62] A higher minimum wage could help disadvantaged workers to cover the costs of finding and keeping a job, including, for example, transportation, child-care, and uniforms.

Allegretto, Dube, and Reich's (2011) own research on the employment effect of the minimum wage on teens looks separately at the effects on white, black, and Hispanic teens. For the period 1990 through 2009, which includes three recessions and three rounds of increases in the federal minimum wage, they find no statistically significant effect of the minimum wage on teens as a whole, or on any of the three racial and ethnic groups, separately, after they control for region of the country. Using a

58 Neumark and Wascher (2008), p. 207.
59 In their analysis of the minimum wage and training, Acemoglu and Pischke (2001) use a noncompetitive, but not explicitly "institutional" model and arrive at a similar conclusion: "In contrast, in noncompetitive labor markets, minimum wages tend to increase training of affected workers because they induce firms to train their unskilled employees."
60 Williams (2011), pp. 45-46
61 Deere, Murphy, and Welch also studied outcomes for minority youth.
62 Allegretto, Dube, and Reich (2011), p. 228, who cite Raphael and Stoll (2002) on this point.

similar methodology, Dube, Lester, and Reich (2012) detect no evidence that employers changed the age or gender composition in the restaurant sector in response to the minimum wage. In a study of detailed payroll records for a large retail firm with more than 700 stores, Laura Giuliano (2012) found that teens from more affluent areas increased their labor supply (and employment) after the 1996-1997 increases in the minimum wage, while employment of teens in less affluent areas experienced no statistically significant change in employment. Recent research by Sabia, Burkhauser, and Hansen (2012) finds job losses among younger, less-educated workers, but not older, less-educated workers. The Sabia, Burkhauser, and Hansen findings, however, are subject to the critiques mentioned earlier – they find job losses well outside the range of the bulk of earlier research and their results are based on a single state-level experiment with the minimum wage and may not be representative.

5. Higher prices

Employers may respond to a higher minimum wage by passing on the added costs to consumers in the form of higher prices. In a purely competitive economy, where all firms are experiencing the same increase in labor costs in response to a minimum-wage increase, economic theory predicts that at least a portion of the cost increase will be passed through to consumers.

Sara Lemos has conducted a comprehensive review of the 30 or so academic papers on the price effects of the minimum wage. She concludes: "Despite the different methodologies, data periods and data sources, most studies reviewed above found that a 10% US minimum wage increase raises food prices by no more than 4% and overall prices by no more than 0.4%"; and "[t]he main policy recommendation deriving from such findings is that policy makers can use the minimum wage to increase the wages of the poor, without destroying too many jobs or causing too much inflation."[63] Neumark and Wascher agree with Lemos's assessment about the likely price effects (while disagreeing with her conclusions about the overall usefulness of the minimum wage): "Both because of the relatively small share of production costs accounted for by minimum wage labor and because of the limited spillovers from a minimum wage increase to wages of other workers, the effect of a minimum wage increase on the overall price level is likely to be small."[64] Other recent research by Daniel Aaronson, Eric French, and James MacDonald on restaurant pricing, a sector with a high share of low-wage workers suggests that the price effects are likely to be lower than the upper bounds suggested by Lemos. Aaronson, French, and MacDonald "find that a 10 percent increase in the minimum wage increases prices by roughly 0.7 percent."[65]

6. Improvements in efficiency

The "institutional" model of the labor market suggests that employers may respond to a minimum-wage increase with efforts to improve operational efficiency including "tighter human resource practices..., increased performance standards and work effort, and enhanced customer services."[66] Employers might prefer these kinds of adjustments to cutting employment (or hours) because employer actions that reduce employment can "hurt morale and engender retaliation"[67] In

63 Lemos (2008), p. 208.
64 Neumark and Wascher (2008), p. 248.
65 Aaronson, French, and MacDonald (2008), p. 697. In their study of the San Francisco citywide minimum wage, Dube, Naidu, and Reich found that prices "increased significantly" at fast-food restaurants, but not at table-service restaurants (2007, p. 542).
66 Hirsch, Kaufman, and Zelenska (2011), p. 7.
67 Hirsch, Kaufman, and Zelenska (2011), pp. 6-7.

institutional models – different from competitive models where firms are always assumed to be operating at peak efficiency – firms generally have some scope for increasing output, albeit usually at a cost of greater managerial effort.

Little direct evidence exists on operational and human resource efficiencies as a channel of adjustment. Hirsch, Kaufman, and Zelenska's study of the impact of the federal minimum-wage increase on 81 fast-food restaurants in Georgia and Alabama, however, asked fast-food managers specifically about scope for efficiency improvements in response to the minimum-wage rise. About 90 percent of managers indicated that they planned to respond to the minimum-wage increase with increased performance standards such as "requiring a better attendance and on-time record, faster and more proficient performance of job duties, taking on additional tasks, and faster termination of poor performers."[68] Roughly the same share of managers said that they sought to "boost morale" by presenting the minimum-wage increase as a "challenge to the store" and using this as a way "to energize employees to improve productivity"[69] Based on their interviews with store managers, Hirsch, Kaufman, and Zelenska suggest that a minimum-wage increase may function as a "catalyst or shock that forces managers to step out of the daily routine and think about where cost savings can occur." [70, 71]

7. "Efficiency wage" responses from workers

A higher minimum wage may also motivate workers to work harder, independently of any actions by employers to increase productivity. According to "efficiency wage" theory, wages above the competitive-market rate may elicit greater work effort for several reasons. As Carl Shapiro and Joseph Stiglitz (1984) have argued, higher pay increases the cost to workers of losing their job, potentially inducing greater effort from workers in order to reduce their chances of being fired.[72] George Akerlof (1982), arguing from a more sociological point of view, has suggested that workers may see higher wages as a gift from employers, leading workers to reciprocate by working harder.[73]

While a large body of research has attempted to test for the existence of "efficiency wages," few studies directly address the theoretical or empirical link between efficiency wages and the minimum wage. James Rebitzer and Lowell Taylor (1995), for example, have developed a formal model that demonstrates that a minimum wage in the context of efficiency wages "may increase the level of employment in low wage jobs." But, to my knowledge, there are no studies testing for efficiency wage effects in connection with the U.S. minimum wage.

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68 Hirsch, Kaufman, and Zelenska (2011), p. 27.

69 Hirsch, Kaufman, and Zelenska (2011), pp. 28-29.

70 Hirsch, Kaufman, and Zelenska (2011), p. 29.

71 Card and Krueger report that the "Dollar General Corporation noted in its 1992 annual report that the impact of the 1992 minimum wage hike was minimized due to "greater employee productivity." (1995, p. 323) It is not clear whether Dollar General viewed these changes as related to management's cost-saving efforts or "efficiency wage" considerations (the next channel of adjustment considered here) or some other channel.

72 Shapiro and Stiglitz (1984).

73 Efficiency wages may work through other channels, some covered elsewhere here, others less relevant to the minimum wage, see, for example, Katz: "Efficiency wage theories suggest that firms may find it profitable to pay workers' wages above the market clearing level since such wage premiums can help reduce turnover, prevent worker malfeasance and collective action, attract higher-quality employees, and facilitate the elicitation of effort by creating feelings of equitable treatment among employees." (1986, pp. 270-271)

8. Wage compression

Employers faced with higher wage costs for their low-wage workers may also seek to make up for these costs by cutting the earnings of higher-wage workers. Large changes over time within the United States, as well as large differences across countries, in the relative pay of high- and low-wage workers suggest that employers have some scope in setting relative wages. In the specific context of a minimum-wage increase, Hirsch, Kaufman, and Zelenska found that almost half of the employers they interviewed said that, in the wake of a federal minimum-wage increase, they "would delay or limit pay raises/bonuses for more experienced employees."[74] Broader studies of the U.S. economy also conclude that the minimum wage compresses the overall wage distribution.[75] These empirical findings give some support to the possibility that employers may compensate for higher wage costs at the bottom by cutting wages of workers who nearer to the top.

9. Reduction in profits

Employers may also absorb the extra costs associated with a minimum-wage increase by accepting lower profits.[76] Unfortunately, "there is almost a complete absence of any study directly examining the impact of minimum wages on firm profitability"[77] Card and Krueger (1995) report the results of several attempts to analyze the impact of minimum-wage increases on firm profits in the United States, but found only a "mixed" and "tentative" effect. More recently, Mirko Draca, Stephen Machin, and John Van Reenen analyzed British firm-level data and concluded that "wages were significantly raised, and firm profitability was significantly reduced by the minimum wage introduction." [78]

10. Increases in demand (minimum wage as stimulus)

Particularly when the economy is in a recession or operating below full employment, a minimum-wage increase may also increase demand for firms' goods and services, offsetting the increase in employer costs.

Since the minimum wage transfers income from employers (who generally have a high savings rate) to low-wage workers (who generally have a low savings rate), a minimum-wage rise could spur consumer spending. This increase in spending could potentially compensate firms for the direct increase in wage costs.

Doug Hall and David Cooper (2012), for example, estimate that an increase in the minimum-wage from its current level of $7.25 per hour to $9.80 per hour by July 2014 would increase the earnings low-wage workers by about $40 billion over the period. The result, they argue, would be a significant increase in GDP and employment:

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74 Hirsch, Kaufman, and Zelenska (2011), p. 28.

75 See, for example, DiNardo, Fortin, and Lemieux (1996), and Autor, Mannning, and Smith (2010).

76 In the competitive labor-market case, Neumark and Wascher note: "prices rise to match the increase in marginal costs associated with a higher minimum wage, but, as a result, output and profits decline." (2008, p. 243) In the case of dynamic monopsony, however, as Card and Krueger explain: "...if a minimum wage forces the firm to pay slightly more than its optimally-selected wage, then the firm will offset virtually all of this extra cost by savings from being able to fill vacancies more rapidly, having lower turnover, improved morale, etc. Any decline in profitability is of second-order magnitude..." (1995, p. 323).

77 Draca, Machin, and Van Reenen (2011), p. 130.

78 Draca, Machin, and Van Reenen (2011), p. 149. They also found "no significant effects on employment or productivity." (p. 130)

> "Using... standard fiscal multipliers to analyze the jobs impact of an increase in compensation of low-wage workers and decrease in corporate profits that result from a minimum-wage increase, we find that increasing the national minimum wage from $7.25 to $9.80... would result in a net increase in economic activity of approximately $25 billion over the phase-in period and... generate approximately 100,000 new jobs."[79]

11. Reduced turnover

The "dynamic monopsony" model of the labor market is sometimes referred to as a "frictions model"[80] because these models take seriously the idea that workers and employers must contend with important deviations from the smooth functioning of the standard, perfectly competitive model. Perhaps the most important frictions in the low-wage labor market involve the high rate of turnover (which is assumed to be zero in the standard competitive model). Because many low-wage workers are constrained by scheduling responsibilities (child care, for example), transportation limitations (lack of a reliable car or inadequate public transportation), and only partial information about available vacancies in their local labor market, employers paying the "going wage" often face significant recruitment costs in the form of unfilled vacancies, rapid turnover, and related screening and training expenses.

In frictions models, a higher minimum wage makes it easier for employers to recruit and retain employees, lowering the cost of turnover. These cost savings may compensate some or all of the increased wage costs, allowing employers to maintain employment levels.[81] Moreover, if the minimum wage reduces the number and the average duration of vacancies, the employment response to a minimum-wage increase could even be positive.[82]

Dube, Lester, and Reich (2012) adapted their "contiguous counties" methodology (Dube, Lester, Reich, 2010), which they had used to measure the effect of differences in minimum wages on restaurant employment across U.S. counties, to look at the effect of the minimum wage on labor turnover among teens and restaurant workers. They find "...striking evidence that separations, new hires, and turnover rates for teens and restaurant workers fall substantially following a minimum wage increase..."[83] Their findings, using nationally representative data, are consistent with local case studies of the minimum wage and related "living wage" laws, including Dube, Naidu, and Reich's (2007) analysis of the San Francisco city-wide minimum wage; Fairris (2005) studying local government contractors in Los Angeles; Howes (2005) on homecare workers in California; and Reich, Hall, and Jacobs (2005) on workers at the San Francisco airport.[84]

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79 Hall and Cooper (2012), p. 9.
80 Dube, Lester, Reich (2012).
81 This raises the question of why employers don't already pay the higher wages. The short answer is that some firms already do so. The key issue here is that both strategies – lower wages and high turnover versus higher wages and low turnover – can both be profitable. Employers choose the strategy that they prefer or that works best for them, but both strategies can succeed, side-by-side, in the market place. The minimum wage limits employers' choices to strategies that are consistent with wages at least as high as the minimum wage.
82 The costs of turnover can be high, even for low-wage workers. See, for example, the CLASP-CEPR Turnover Calculator, http://www.cepr.net/calculators/turnover_calc.html or Boushey and Glynn (2012).
83 Dube, Lester, Reich (2010), p. 2.
84 All cited in Dube, Lester, and Reich (2012).

Discussion

Across all of the empirical research that has investigated the issue, minimum-wage increases are consistently associated with statistically significant and economically meaningful increases in the *wages* of affected workers. At the same time, what is striking about the preceding review of possible channels of adjustment – including employment – is how often the weight of the empirical evidence is either inconclusive (statistically insignificant or positive in some cases and negative in others) or suggestive of only small economic effects.

One plausible explanation for these findings is that employers (and workers) respond on multiple fronts to any increase in the minimum wage. Individual establishments will follow different paths that depend on a complex set of circumstances that economists – operating with what is, even in the best of circumstances, a limited set of data – cannot fully capture or explain. Some employers may cut hours; others, fringe benefits; still others, the wages of highly paid workers. Some employers may raise prices (particularly if their competitors are experiencing similar cost increases in response to the minimum wage). Some employers may see their profits fall (along with those of their competitors), while others may reorganize the work process in order to lower costs. Some of the strongest evidence suggests that many employers may experience declines in costly turnover. And workers may respond to the higher wage by working harder. Any of these channels might be sufficient to eliminate the need for employment cuts or reduce the size of employment cuts to a level below where they can be reliably measured.

Employers and workers at the same establishment may follow more than one of these adjustment paths at the same time. Given the modest costs associated with historical increases in the minimum wage, it seems entirely plausible that small adjustments across a few of these margins could more than compensate for the higher wage floor.

Some of these adjustment paths reduce the benefit of the minimum wage to affected workers (reductions in non-wage benefits or training), but most have an ambiguous effect (reductions in hours or increased work effort) or no effect (lower profits or wage compression within a firm) on the well-being of low-wage workers. And some adjustment channels arguably improve workers' well-being (lower turnover or increased consumer demand).

The strongest evidence suggests that the most important channels of adjustment are: reductions in labor turnover; improvements in organizational efficiency; reductions in wages of higher earners ("wage compression"); and small price increases.

Conclusion

Economists have conducted hundreds of studies of the employment impact of the minimum wage. Summarizing those studies is a daunting task, but two recent meta-studies analyzing the research conducted since the early 1990s concludes that the minimum wage has little or no discernible effect on the employment prospects of low-wage workers.

The most likely reason for this outcome is that the cost shock of the minimum wage is small relative to most firms' overall costs and only modest relative to the wages paid to low-wage workers. In the traditional discussion of the minimum wage, economists have focused on how these costs affect employment outcomes, but employers have many other channels of adjustment. Employers can reduce hours, non-wage benefits, or training. Employers can also shift the composition toward higher skilled workers, cut pay to more highly paid workers, take action to increase worker productivity (from reorganizing production to increasing training), increase prices to consumers, or simply accept a smaller profit margin. Workers may also respond to the higher wage by working harder on the job. But, probably the most important channel of adjustment is through reductions in labor turnover, which yield significant cost savings to employers.

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Living Wage Laws in Practice

The Boston, New Haven and Hartford Experiences

MARK D. BRENNER

STEPHANIE LUCE

POLITICAL ECONOMY RESEARCH INSTITUTE

PERI

University of Massachusetts Amherst

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Mark D. Brenner,
Assistant Research Professor, PERI
&
Stephanie Luce,
Assistant Professor, University of Massachusetts-Amherst Labor Center

POLITICAL ECONOMY RESEARCH INSTITUTE
PERI
University of Massachusetts Amherst

Amherst, MA: University of Massachusetts,
Political Economy Research Institute

About the authors

Mark Brenner is an Assistant Research Professor at the Political Economy Research Institute at the University of Massachusetts-Amherst. He is a nationally recognized expert on the economic impact of living wage ordinances, and has participated in living wage policy formulation in cities around the country: including Los Angeles and Santa Monica, CA; New Orleans, LA; and most recently Santa Fe, NM. He is currently exploring the challenges involved in defining and measuring a living wage in a global context.

Stephanie Luce is an Assistant Professor at the University of Massachusetts-Amherst Labor Center. She is the author of *Fighting for a Living Wage* (Cornell University Press), and co-author with Robert Pollin of *The Living Wage: Building a Fair Economy* (New Press). She has also researched the effects of globalization on jobs and workers, including work tracking the global movement of jobs. Prior to the Labor Center, she worked at the U.S. Department of Labor, the Center on Wisconsin Strategy, and the Political Economy Research Institute.

Political Economy Research Institute
Gordon Hall / 418 N. Pleasant St., Suite A
Amherst, MA 01002

http://www.umass.edu/peri

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Chapter 2

The Impact of Living Wage Laws on City Contracting

For many firms, labor costs account for a significant portion of their overall costs. If living wage laws force companies to raise wages for a sizable portion of their workforce, then the price of their services—and therefore contract costs paid by cities—might rise. What's more, if living wage laws raise the cost of doing business with cities, they might also discourage some firms from bidding on service contracts, undermining competition and opening the door to even higher prices from remaining bidders. Although these are indeed possible outcomes from living wage implementation, have they in fact occurred?

Examining the evidence from other cities as well as New Haven, Boston, and Hartford, we found a modest overall impact on contract costs and bidding, and a somewhat mixed picture both within and between cities. For example, contract costs actually fell in two of our three cities after living wage implementation, while contract costs rose in one city.[1] The impact of a living wage law on individual contracts often varied widely, reflecting the type of services they cover and the way cities conduct the bidding. We further found that competitive bidding remains strong under living wage ordinances, and that such laws may even boost the number of bidders on city contracts. On balance, these experiences imply that a living wage law is only one of many factors influencing the cost and competitiveness of city procurement.

The Record in Other Cities

Living wage laws have been in place in many cities around the country for quite some time. What impact have those cities experienced? Fortunately, a growing body of evidence is beginning to shed light on that question. For example, two studies examined Baltimore's living wage law, implemented in 1995. One study, conducted after the first year of implementation, reported that the total cost of 19 contracts had risen only a quarter of one percent since the law took effect. The other, conducted three years later, found that the cost of 26 contracts had risen just 1.2 percent. In both cases the rate of inflation was higher, so real costs actually fell.

Both studies also found that the impact on individual contracts varied substantially. For example, the contract for Baltimore's bus services—by far the largest—rose by just 2 percent. The cost of a small janitorial contract, in contrast, rose by 47 percent, while the cost of a contract for summer food services fell by 12 percent.[2]

Another review of 13 living wage laws across the country found that city and county officials in every location reported higher contract costs, with the absolute amount of overall cost increases varying widely. Unfortunately, in many cases officials did not compare these cost increases with the total value of covered contracts or the rate of inflation, so we cannot judge whether relative costs actually rose or fell in real terms.

As with the Baltimore experience, officials in each city reported considerable variation in changes in the costs of individual contracts. For example, the cost of a janitorial contract rose 22 percent in Warren, Mich., while the real cost of three human service contracts declined in Dane County, Wisc. In Corvallis, Ore., an analysis in June 2001 found that the total cost of 31 contracts covered by a living wage ordinance had risen 13 percent—much faster than the inflation rate of 3.5 percent.[3]

Some cities have taken active steps to mitigate the costs of their living wage laws. For example, in a one-year report filed in February 2000, Pasadena city manager Cynthia Kurtz found that the cost of five contracts rose by $168,000 (the report did not specify the total contract cost). However, according to Steve Mermell, who oversees Pasadena's living wage law, the city had actually budgeted $340,000 to cover an expected cost increase. Officials negotiated with their contractors to split the higher costs, agreeing in exchange to extend existing contracts rather than put them out for competitive bid.

In a similar case, Multnomah County, Ore., reported a 5 percent rise in total contract costs for covered services after implementing its living wage policy. However, costs would have risen 27 percent under the old contracts: the county saved funds by consolidating janitorial services at the Department of Corrections, the courthouse, and the county jail into a single contract. This appears to be an example of "relational contracting"—wherein the parties recognize "that for all intents and purposes they depend on one another," and "that it's in their self-interest to establish a long-term cooperative relationship."[4]

Evidence also shows that living wage ordinances can boost municipalities' satisfaction with service contracts. In Multnomah County, the contractor's performance rating rose from 2 out of 5 before the living wage to 4 out of 5 six months after it took effect. These gains may reflect a drop in annual turnover among janitors, which fell from 60 to 25 percent over the same period.

Some of these studies reveal contradictory effects of living wage laws on bidding patterns. For example, one of the two Baltimore studies found that the total number of bids the city received fell from 93 before the law took effect to 76 after (the number of bidders rose on three contracts and fell on eight). An official in Ypsilanti Township, Mich., in contrast, reported that major contracts attracted "more bidders than ever before, at even better rates," after the living wage took effect, forcing them to "be tighter and provide less of a profit margin." City officials in Alexandria, Va., noted a similar boost to competitive bidding after the city adopted its living wage law.[5]

"Evidence shows that living wage ordinances can boost municipalities' satisfaction with service contracts.

In Corvallis, Ore., several firms indicated that they would not bid on city business because of the living wage, yet every vendor the city contacted submitted a bid, "and the bids have continued to be competitive," according to the city finance director. In Hayward, Calif., the acting finance director reported that all contracts remained competitively bid, and that "productivity and service quality have not been adversely affected."[6]

How We Approached Our Three Cities

To further investigate the impacts of living wage laws on contract costs and competitive bidding, we compared experiences in New Haven, Boston, and Hartford before and after they implemented their ordinances. Because the scope of the law in each city varies, and because the cities differ in the amount of contracting they pursue, we found dramatic differences in the number of covered contracts among the three (*Table 2.1*).

For example, because Boston's law does not restrict its coverage to specific services, the city reported 219 covered contracts in September 2001. Some 53 of these contracts were effectively exempt, leaving 166 with a total value of close to $137 million.[7] Although this large number of contracts would be ideal for analyzing the effects of the city's living wage law, the cost of obtaining copies of each contract proved prohibitive. Thus we restricted our study to "high-impact" contractors—those reporting at least five employees earning between $8.71 (the living wage floor in fiscal year 2000–01) and $12 an hour. To identify high-impact contractors, we relied on quarterly reports that covered vendors must file with the Living Wage Division of the Office of Jobs and Community Services. Those reports include the number of employees falling within several wage ranges.

That strategy made results among the three cities more comparable, as both New Haven and Hartford restrict their living wage laws to low-wage sectors such as janitorial and security guard services (*Table 2.2*). The contracts we excluded from our Boston analysis, moreover,

TABLE 2.1 – Contracts Covered by Living Wage Laws in Boston, Hartford, and New Haven, as of June 2001*

City	Covered contracts	Total contract value
Boston		
Total	219	$201,819,829
Covered	166	$136,803,560
Exempt	53	$65,016,269
Hartford	2	$1,184,959
New Haven	7	$596,574

Source: Authors' calculations based on data obtained from the three cities.
Note: In Boston, "requirement" contracts are exempt from the living wage law. The city taps such contracts—which set the upper limit of work a vendor can perform—only as needed. A vendor with such a contract for automotive repairs, for example, may never actually perform any work.
* Boston data are through September 2001.

cover professional services such as legal, engineering, and architectural services, which are unlikely to have experienced significant cost increases as a result of the living wage law. Overall we found that 25 contract holders in Boston met our criteria—18 of them nonprofits.
We asked city departments to provide copies of the contracts we intended to analyze, and only one (Elderly Services) failed to comply with our request. Even so, we could not match many of these contracts with equivalent services performed before the living wage law took effect. To compensate, we added several special-education contracts from the Boston Public Schools to our analysis, because that sector experienced the heaviest impact from the living wage law. (The law forced nearly 60 percent of special-education contractors to raise wages, as we show in Chapter 3.) In all we obtained information on 28 contracts in Boston, 22 of which applied to special education, with a total value of $41 million. Those contracts represented some 30 percent of the total value of all covered service contracts at that time.[8]

In marked contrast to Boston, New Haven's law affected some 15 service contracts at the time of our data collection. However, the city had funded only 8 of those both before and after the law took effect. Because the city merged 2 of these contracts in fiscal year 2001–02, we focused on 7 contracts with a value of nearly $600,000.

In Hartford, the living wage law had affected only 2 contracts worth $1.2 million when we collected our data, although the city reports that the law will eventually affect 8 contracts. Both the contracts covered services, as no economic development projects had yet come under the

TABLE 2.2 – Services Covered by Living Wage Laws in Boston, Hartford, and New Haven

City	Service
Boston	Adult education
	Architectural and engineering services
	Assisted living*
	Consulting services
	Childcare services*
	Cleaning services*
	Community learning center services*
	Computer services and support
	Educational consulting
	General repair services
	Janitorial services*
	Legal services
	Security guard services*
	Special education*
	Supportive housing*
	Temporary office assistance*
	X-ray services*
Hartford	Security guard services
	Temporary office assistance
New Haven	Busing services
	Food services
	Janitorial services
	Security guard services

Source: Authors' analysis of data obtained from the three cities.

* "High-impact" services are those where at least one contractor reports a concentration of low-wage workers. The study focused on those services.

law's purview. That experience is not uncommon: many cities whose living wage law covers economic development aid actually apply the law to few if any projects.[9]

The Impact of Living Wage Laws on Bidding Patterns

How have living wage laws affected competitive bidding in our three cities? In Boston and Hartford the number of bids either stayed the same or grew after the living wage law took effect, while in New Haven the number of bids declined by three. Overall, we found that the total bids for all three cities declined by only one after living wage implementation (*Table 2.3*).

TABLE 2.3 – Total Number of Bids Before and After Implementation of the Living Wage

Service	Before	After	Difference
Boston (*high-impact firms only*)			
X-ray services, Suffolk County Jail	3	1	-2
Temporary office help, Dept. of Neighborhood Development	5	9	4
Janitorial services, Police Dept.	9	7	-2
Security services, Library	3	4	1
Cleaning services, Prop. Management Office	6	5	-1
Boston subtotal	26	26	0
Hartford			
Temporary office help, citywide	3	3	0
Security services, citywide	7	9	2
Hartford subtotal	10	12	2
New Haven			
Security services, Main Library	5	5	0
Janitorial services, Health Office	5	4	-1
Janitorial services, Police Station	9	5	-4
Janitorial services, Main Library	4	4	0
Janitorial services, Branch Libraries	3	4	1
Janitorial services, Senior Center	3	3	0
Food preparation services, Child Develop't	1	2	1
Bus services, Parks Dept.	1	1	0
Bus services, Child Develop't	1	1	0
New Haven subtotal	32	29	-3
All cities total	68	67	-1

Source: Authors' analysis of data obtained from the three cities.

Within each city we saw wide variation among individual contracts. More than a third of all contracts saw no change in the number of bidders, nearly a third saw increases, and bids declined for nearly 30 percent. Declines in the number of bidders were most prevalent in Boston, occurring for three of five types of services. (We excluded special-education contracts here because Boston does not award them through competitive bidding. Instead, special-ed facilities must first receive state certification and then win selection by the Boston Public Schools as placement sites.) Given that less than a third of contracts saw declines in the number of bidders after living wage implementation, forces *other* than the living wage law seem to be exerting at least as strong an effect on the number of firms willing to compete for contracts. Reinforcing experiences in Baltimore and other cities, we did find that bidding patterns varied systematically across a few sectors. One example is janitorial services: the number of bidders declined for four of seven janitorial and cleaning contracts after the living wage took effect. That total includes two contracts in New Haven, where winning bids usually come from small, individually owned and managed janitorial companies, and two in Boston, where large, commercial building services firms tend to compete for the city's janitorial contracts.

Two out of three security guard contracts, in contrast, saw an increase in the number of bidders, as did one of two temporary office assistance contracts. In these cases, the living wage floor may have actually improved bidding by reducing the ability of vendors to undercut their competition. As New Haven's Controller Mark Pietrosimone noted, the living wage ordinance "puts all vendors on equal footing…[and] it has leveled off undercutting," forcing contractors to compete with one another along dimensions other than wages and benefits, such as service quality.[10] Experience in Hartford sheds light on why and how that occurs.

"For some services, living wage laws can dramatically increase the number of bidders.

Expanding the Bidding Pool: Security Guard Contracting in Hartford

In September 1999, a month after passing its living wage law, Hartford solicited bids for a new city contract for security guard services. The contract was scheduled to begin on January 1, 2000, and run through December 31, 2001. The initial request solicited proposals for some 54,000 hours of security guard services over the two-year period, and firms submitted their bids in the form of an hourly rate the city would pay for each hour of services actually performed. Two companies bid on the contract, including Command Security, which had won the last contract for these services.

That number of bids was much lower than in past years: seven companies had bid during the 1997 round, and five had done so during the 1993 round. (The contract was not competitively bid in 1995; the city extended Effective Security's 1993 contract for two years.) Most firms decided not to compete with Command Security—the incumbent contractor— in 1999, perhaps because the Hartford-based company was guaranteed special consideration under a provision giving preference to local businesses. That provision had been decisive when the city awarded Command Security the contract in 1997.

Upon review, city officials realized that the contract was subject to the new living wage ordinance but that they had not informed contractors. The officials determined that the

TABLE 2.4 – Bids for Hartford Security Guard Contracts

Bidder	1997	1999 Round 1	1999 Round 2
Command Security Corp.	**$9.75**	**$10.07**	$14.96
Metro Loss Prevention	$9.87		
Elite Security	$9.90		
Tri-City Security Services	$10.38		$18.85
Burns International Security	$10.49		$19.35
Pinkerton Security Services	$11.50	$10.56	$15.65
Wackenhut Corp.	$13.34		
Lance Investigations			**$14.58**
Argus Security Group			$14.61
Jo-Ryu Security			$17.77
Novas Security			$18.55
Al Washington and Associates			$18.62

Source: Authors' analysis of data obtained from the city of Hartford.
Note: Bids for Hartford's security guard contract are made on the basis of an hourly billable rate charged to the city. The values are reported as they were submitted in each year; that is, we have not adjusted them for inflation.

contract should be re-bid, and this time included information on the living wage in all materials they sent to prospective bidders. In this second round the city received nine bids, including new bids from the two companies that had bid during the first round (*Table 2.4*). Hartford's living wage law seems to have sparked a dramatic increase in the number of bidders.

The living wage ordinance was not the only factor underlying the quadrupling of bidders. One second-round bidder, Argus Security Group, pointed out that the city of Hartford did a better job of advertising the request for proposals in the second round. Argus representative Pat Paboway said that the firm would have probably entered the first-round bidding had it been aware of the opportunity.

Still, a closer look at the record shows that the living wage may also have leveled the playing field, encouraging more companies to bid. An analysis by the city two years after implementing the living wage found that under the prior contract, Command Security had employed 10 security guards earning $6.77 and 2 guards earning $6.60 per hour. The former group did not receive health benefits while the latter did, but in both cases the guards were earning only about a dollar above the state minimum wage of $5.65. According to the Bureau of Labor Statistics, those wages were nearly 30 percent below the average hourly wage for security guards in the Hartford area at the time ($9.45), and 20 percent below the median ($8.38).

An analysis of Command Security's contract reveals that wage costs accounted for more than two-thirds of the hourly bid price prior to the living wage. (The company charged the city $9.75 per hour, while the highest-paid guards were earning $6.77.) This suggests that firms paying higher wages were at a disadvantage when competing with Command Security in the city security guard market when the only floor was the statewide minimum of $5.65. By setting a wage floor well above the state minimum wage, Hartford's ordinance substantially enlarged the market for security guard services.

Rod Murdoch of Tri-City Security Services confirmed that his company decided to enter the Hartford security guard market because "the playing field had been leveled." Tri-City, he said, often receives opportunities to work in "low-ball" niches, where the guards make little money and the company's margins are thin. However, he said, Tri-City prefers to work in "'middle niches,' where the guards are making more in the range of $9 to $10 and the company's margins aren't so thin." He also maintained that Tri-City prefers to work with the private sector because the public sector often has more contract requirements but, in his opinion, is unwilling to pay for them. "We'll provide a guard with certain credentials," he said, "but you must be willing to pay for it."

"The living wage may also have leveled the playing field, encouraging more companies to bid.

Donald Coursey of Al Washington and Associates concurred that he considers the municipal contracting market problematic, "because cities are usually obliged to take the lowest bid, which means that there is an incentive to low-ball, and it's hard to compete against that. It means you end up paying people minimum wage, which is very unstable, because people can make that money anywhere, and they may just disappear tomorrow, and the city is calling up saying, 'Where is my guard?' and you are hamstrung, and in the process your reputation gets ruined." He added, "Most companies with any business sense would concentrate on a higher-wage niche, because there is more stability involved, and it gives you better control of the business, and allows you to preserve your reputation." Coursey held that any firm with a long-term approach to working in the security guard industry would avoid the low-wage end of the market.

Mark Cratin of Lance Investigations similarly reported that his company usually avoids low-wage guard work, instead seeking out contracts in which guards can earn at least $10 an hour. He argued that the low-bid method is inefficient; his firm sat out the 1999 bidding on the Hartford security guard contract for precisely that reason. These results reinforce the argument that cities can exert a major impact on the market in which they procure services, a theme we return to in the concluding chapter.

TABLE 2.5 – Real Annual Contract Costs before and after Living Wage Implementation (in 2001 dollars)

City	Before	After	Difference
Boston *(high-impact firms only)*			
Special education (*number of contracts=22*)	$18,356,900	$15,078,551	-18%
Non–special education (*number of contracts=6*)	$1,414,013	$ 1,372,230	-3%
Total (*number of contracts=28*)	$19,770,913	$ 16,450,781	-17%
Hartford (*number of contracts=2*)	$465,338	$617,416	33%
New Haven (*number of contracts=9*)	$692,697	$611,411	-12%

Source: Authors' calculations based on data collected from the three cities.
Note: As noted in the text, for each contract we compared the cost prior to the living wage with the cost afterward. For consistency, we calculated the annual cost of multi-year contracts, and adjusted for inflation by expressing those costs in 2001 dollars.

The Impact on Contract Costs in Our Three Cities

How have living wage laws affected city contract costs? In Boston, we found that the total annual cost of the 28 contracts we analyzed fell markedly in real terms—from $20 million to $17 million, or 17 percent—after the city implemented its living wage ordinance. A 19 percent drop in the 22 special-education contracts drove this decline. However, the 6 other contracts also declined by 3 percent. New Haven similarly registered a 12 percent decline in annual contract costs after implementing its living wage law. The overall cost of the 2 Hartford contracts, in contrast, rose sharply—by 33 percent (*Table 2.5*).[11]

To better understand these results, we examined average cost changes across all the contracts in our study. At first glance, a more detailed view seems to show that living wage laws boosted the average cost of a service contract in these three cities. In Boston, special-education contracts rose an average of 3 percent, while the other contracts rose an average of 7 percent. In New Haven, the average contract rose 0.3 percent, while in Hartford it rose 29 percent *(Table 2.6*).

However, we find a different story when we factor in the size of the contracts, weighting them according to their total dollar value. Adjusting for contract size is important when we want to get a sense of whether a city will experience overall cost increases owing to the living wage. In

TABLE 2.6 – Average Real Annual Change in Contract Costs under the Living Wage (in 2001 dollars)

City	Unweighted	Weighted
Boston *(high-impact firms only)*		
Special education *(number of contracts=22)*	3%	-9%
Non–special education *(number of contracts=6)*	7%	16%
Total *(number of contracts=28)*	3%	-7%
Hartford *(number of contracts=2)*	29%	33%
New Haven *(number of contracts=9)*	0.3%	-11%

Note: To account for the size of each contract, the figures in column two are calculated using weights. Specifically, the percentage change in each contract's cost is weighted according to the proportion of the overall annual cost that each contract comprises.

this case we find that Boston's special-education contracts declined an average of 9 percent, while non–special-education contracts rose 16 percent. New Haven's contracts declined by an average of 11 percent, while Hartford's rose an average of 33 percent. Except for non–special-education contracts in Boston—which reflect a substantial increase in the cost of temporary office services—these results mirror the total average annual changes reported in Table 2.5.

What forces underlie the remarkably different cost outcomes between Boston and New Haven, on the one hand, and Hartford on the other? The most obvious influence is the different nature of services contracted out in Boston. A much higher proportion of Boston's contracts apply to human services such as special education, where reimbursement rates are set by state and federal agencies. These contracts are not competitively bid, and their fixed reimbursement rates do not allow contractors to pass on higher labor costs to the city.

However, contract costs also declined in Boston even for some competitively bid services such as X-ray and janitorial services. The major difference among the three cities seems to be that Hartford bid both its contracts on a unit-cost basis. Under that approach, cities ask vendors to submit the rate they will charge for each hour of work they perform, rather than to submit a bid for the total value of the work. This approach encourages firms to apply "cost-plus" markups, and thus appears ill-suited to holding down total contract costs. Indeed, we find that most contracts bid on a unit-cost basis in Boston and New Haven display a similar pattern. Because of the systematic impact unit-cost bidding appears to exert on contract costs, their dynamics merit more attention.

How Unit Costs Change under Living Wage Laws

Behind the changes in contract costs reported in Table 2.6, we find a clear pattern of cost increases for security guard services and temporary office assistance in all three cities. Officials rely on unit-cost bidding for these services because they can rarely anticipate their exact need for them in advance. That approach opens the door for significant cost increases under a living wage law.

For example, the winning bidder for security guard services in Hartford raised the average markup—the difference between what the city paid and the amount the vendor paid its workers—from $3.12 to $4.36 after living wage implementation. Some of this undoubtedly reflected higher payroll taxes and worker's compensation payments stemming from the living wage. The company may also have passed on raises for employees not working on city contracts, or raises for employees earning above the living wage. Mandated wage increases for part of a company's workforce are expected to create pressure to raise wages for workers not covered by the mandate. But as the next chapter shows, non-mandated wage increases under living wage ordinances are actually relatively modest. This implies that the firm may have padded its bid not only to recuperate the indirect costs of the living wage, but also to maintain or boost its profit margin on each hour worked.[12]

Higher contract costs after living wage laws take effect are more common in cities where unit-price bidding is more prevalent. Indeed, contractors bidding on unit prices often appear to pass higher labor costs back to the city more than dollar for dollar, as with security guard services in Hartford. While that case represents the extreme among our cities, almost all contracts bid on a unit-cost basis experienced the problem.[13]

The Hartford case also shows that efforts to consolidate services can hold down markups and unit prices even under unit-cost bidding. For example, the real unit cost for security guard services in Hartford grew by 43 percent. In contrast, 6 of the 12 unit prices for temporary office assistance bid both before and after living wage fell, and only 2 rose by more than 15 percent. While these results may partly reflect the market for temporary office services in Hartford, they may also reflect a conscious strategy by bidders to hold down the unit prices of some services while raising them for others in an effort to win the contract for consolidated services. Evidence from Boston and New Haven also suggests that in cases where they consolidated services, even those bid on a unit-cost basis, the cities were able to prevent higher labor costs from translating into higher prices. In sum, when cities bundle service contracts—such as by awarding a single contract for cleaning all libraries rather than a separate contract for each building—firms appear to lower the amount of overhead they add to their bids.[14] Our results suggest that consolidating service contracts can cut cost pass-through by contractors as much as 20 percent (see Appendix 2).

Do Living Wage Laws Force Cities to Curtail Services?

Concern often arises that cities will curtail services if living wage mandates force contract costs to rise. However, higher contract costs have not prompted our three cities to cut public services. The contract for security guard services at the Boston Public Library is a good example. Unit prices rose nearly 39 percent in real terms after living wage implementation, but the city actually expanded the number of guard hours at the library and total contract costs rose by nearly 60 percent. Diane Collins, who oversees the contract, believes that higher wages actually spurred positive changes that helped sustain the level of services. She agreed that "The guards seem a little happier than the batch that was here before. Plus, they seem to be here longer. Before the living wage, you'd see new faces all the time. With higher wages, the guards seem to take the work more seriously and provide better service."

Joanne Keville-Mulkern, contracting specialist for the Boston Public Schools, reported that the living wage ordinance has not forced the city to curtail services for which BPS contracts, nor have human service agencies proved less willing to bid on city contracts. However, she did express the concern, shared by many of Boston's nonprofit contractors, that if living wage mandates generate significant costs, providers will have no way to pass those costs through to the city, as federal and state agencies set their reimbursement rates. Although this dilemma was not a real issue under the original law, nonprofits were concerned that the September 2001 expansion may lead to hardship.

Overall, staff members responsible for implementing the living wage law in the three cities confirmed our findings that its impact on costs and competitive bidding has been modest. In New Haven, where the ordinance mandates that the city evaluate its impact each year, staff members found only a 6 percent increase in the cost of busing for field trips. They also noted that the workforce for several contracts was unionized, so workers already received wages higher than the living wage threshold. When discussing the Boston law with the Providence City Council, Mimi Turchinetz, director of Boston's Living Wage Division, attested: "We have not seen a decrease in competition for these contracts. We also have not seen increased costs to maintain city contracts. Vendors and the city have successfully absorbed the cost of the living wage ordinance. There has been no adverse financial impact on the city. The living wage ordinance has been good for Boston."

Endnotes

1. Are negotiated contract costs an accurate benchmark of the real costs of procuring services? Bidders may submit artificially low bids to win contracts, only to renegotiate more favorable terms after a contract is awarded. One analyst has labeled this the "hold-up" phenomenon (Hirsch 1991). If such a practice is common, our analysis will understate the true costs of living wage laws.

Interviews with officials in all three cities revealed no evidence that renegotiation is occurring. For example, Diane Collins, who oversees the living wage for the Boston Public Library, held that library staff members invest time up front to ensure that bids describe the work accurately, and that vendors cannot renegotiate the terms of their contract. According to Collins, one director told a vendor "that if they wanted to go that route, the library would exercise their right to void the contract and re-award it 30 days later to another firm." New Haven controller Mark Pietrosimone recounted a similar incident in which the city rebid a cleaning contract after the firm tried to renegotiate it.

2. For details of the first Baltimore study, see Weisbrot and Sforza-Roderick (1996), and for details on the second, see Niedt et al. (1999).

3. For the 13-city review, see Elmore (2003). For details on Corvallis, Ore., see Brewer (2001).

4. This quote comes from Sclar (2000). Multnomah County data come from Facilities and Property Management Division (n.d.). For more on relational contracting, see Sclar (op. cit.).

5. These quotes are drawn from Elmore (2003).

6. The quotes on the Corvallis experience come from Brewer (2001), while those on Hayward come from Finance Director's Office (2000).

7. The contracts that were effectively exempt from Boston's law fell into a category known as "requirement contracts." These are contracts for services that may be performed if the city has a need for them (e.g. auto glass repair, locksmith services, and plumbing and electrical repair). Living wage requirements are only applied should the city make use of more than $100,000 of these services, a phenomenon that we found rarely, if ever, occurs.

8. As noted, Boston dramatically expanded its living wage ordinance in September 2001, raising the wage floor to $10.25 per hour, lowering contract thresholds to $25,000, and lowering the full-time-equivalent threshold to 25 employees for nonprofits. Because of the long process of phasing in these new provisions, we restricted our analysis to contracts covered under the original provisions of the law.

9. See Brenner et al. (2002) for a discussion of how often cities apply living wage laws to recipients of economic development assistance.

10. Elmore (2003).

11. Some contracts are annual while others span multiple years, so we calculated the annual costs for each. Like most cities, Boston, Hartford, and New Haven implemented the living wage law gradually as contracts expired and were rebid or renewed. To account for this phasing in, we compared a contract from the cycle before the living wage took effect to the one negotiated during the ensuing cycle. Where the scope of services clearly changed over time, we adjusted contract values accordingly.

12 Without additional information on the actual overhead costs of the winning contractor, we could not evaluate whether its profit margins actually rose or fell after living wage implementation.

13. One exception was New Haven's nutrition programs for children, where costs declined even though the city bids the contracts on a unit-cost basis. That result probably reflected the high proportion of non-labor costs involved in preparing meals compared with other services bid on a unit-cost basis.

14. Of course, consolidating contracts will not be practical for many services. See Pollin et al. (1999) for a more detailed discussion.

Minimum Wages and Firm Profitability

Author(s): Mirko Draca, Stephen Machin and John Van Reenen

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Minimum Wages and Firm Profitability†

By Mirko Draca, Stephen Machin, and John Van Reenen*

We study the impact of minimum wages on firm profitability, exploiting the changes induced by the introduction of a UK national minimum wage in 1999. We use pre-policy information on the distribution of wages to implement a difference-in-differences approach. Minimum wages raise wages, but also significantly reduce profitability (especially in industries with relatively high market power). This is consistent with a simple model where wage gains from minimum wages map directly into profit reductions. There is some suggestive evidence of longer run adjustment to the minimum wage through falls in net entry rates. (*JEL* J31, J38, L25)

In debates on the economic impact of labor market regulation, much work has focused on minimum wages. Although the textbook competitive labor market model implies that wage floors raise the wages of the low paid and have a negative impact on employment (George J. Borjas 2004; Charles Brown 1999), the empirical literature is less clear-cut. Many studies have rigorously demonstrated that minimum wages significantly affect the structure of wages by increasing the relative wages of the low paid (e.g., John DiNardo, Nicole M. Fortin, and Thomas Lemieux 1996).[1] However, in spite of the large number of studies, empirical evidence on employment effects is considerably more mixed (see the recent comprehensive review by David Neumark and William L. Wascher 2007). Some have found the expected negative impact on employment,[2] yet others have found no impact or sometimes even a positive effect of minimum wages on jobs.[3]

* Draca: Centre for Economic Performance, London School of Economics, Houghton Street, London WC2A 2AE, and University College London (e-mail: m.draca@lse.ac.uk); Machin: Department of Economics, University College London, Gower Street, London WC1E 6BT, and Center for Economic Performance (London School of Economics, LSE), and Institute for the Study of Labor (IZA) (e-mail: s.machin@ucl.ac.uk); Van Reenen: Centre for Economic Performance, London School of Economics, Houghton Street, London WC2A 2AE and National Bureau of Economic Research, Center for Economic and Policy Research (CEPR), and IZA (e-mail: j.vanreenen@lse.ac.uk). This is a significantly revised version of Draca, Machin, and Van Reenen (2008). We would like to thank John Abowd, Josh Angrist, Charles Brown, Alan Manning, Steve Pischke, participants at seminars in the Low Pay Commission, Milan, LSE, the Society of Labor Economists' conference in Boston, and the Department of Work and Pensions conference at the University of York for a number of helpful comments. Three anonymous referees have given extremely helpful discussions of an earlier draft.

† To comment on this article in the online discussion forum, or to view additional materials, visit the article page at http://www.aeaweb.org/articles.php?doi=10.1257/app.3.1.129.

[1] See also Lemieux (2006) for some recent evidence on the United States and DiNardo and Lemieux (1997) for a comparison with Canada.

[2] See the discussion of time series studies in Brown, Curtis Gilroy, and Andrew Kohen (1982) and Brown (1999) or the US cross-state panel evidence of Neumark and Wascher (1992) and the recent longer run analyses of Neumark and Olena Nizalova (2007).

[3] Examples here are Richard Dickens, Machin, and Alan Manning (1999) and David Card and Alan B. Krueger (1994).

In light of this, it is natural to ask how firms are able to sustain higher wage costs induced by the minimum wage. This paper explores the possibility that firm profit margins are reduced. A second possibility is that firms simply pass on higher wage costs to consumers in the form of price increases. However, there is scant evidence on this score.[4] Indeed, even with some positive price response, part of the higher wage costs may not be fully passed on to consumers and the minimum wage could eat directly into profit margins. A third possibility is that minimum wages may "shock" firms into reducing managerial slack and improving efficiency. We examine this productivity story but do not find any evidence for it.

Given this discussion, it is surprising that there is almost a complete absence of any study directly examining the impact of minimum wages on firm profitability. This is the focus of this paper. We adopt an identification strategy using variations in wages induced by the introduction of the national minimum wage (NMW) in the United Kingdom as a quasi-experiment to examine the impact of wage floors on firm profitability. The introduction occurred in 1999 after the election of the Labor government that ended 18 years of the Conservative administration. To date there is evidence that the NMW increased wages for the low paid, but had little impact on employment,[5] and so this provides a ripe testing ground for looking at whether profitability changed.

Our work does uncover a significant negative association between the NMW introduction and firm profitability. We report evidence showing wages were significantly raised, and firm profitability was significantly reduced by the minimum wage. There is also some evidence of bigger falls in margins in industries with relatively high market power, but no significant effects on employment or productivity in any sector. Our findings can be interpreted as consistent with a simple, no behavioral response model, where wage gains from minimum wages map into profit reductions. There is a hint of a selection effect in the longer run as net entry rates fall in the most affected industries, but although the magnitude of the effect is nontrivial, it is statistically insignificant.

The rest of the paper is structured as follows. In Section I, we discuss a model of profit responsiveness to wage changes from which we derive our empirical strategy. Section II discusses the data and the characterisation of firms more likely to be affected by the minimum wage introduction. Section III gives the main results on wage and profitability effects and tests their robustness. Section IV offers some further investigations using other datasets (care homes), other outcomes, and sectoral heterogeneity. Section V concludes.

[4] This was the conclusion of the survey on minimum wages and prices by Sara Lemos (2008). For exceptions on restaurant prices see Daniel Aaronson (2001); Aaronson and Eric French (2007); and Denis Fougere, Erwan Gautier, and Herve le Bihan (2008). The only United Kingdom evidence to our knowledge is Jonathan Wadsworth (2010) who finds limited effects on prices.

[5] See Machin, Alan Manning, and Lupin Rahman (2003) and Mark B. Stewart (2004).

I. Motivation and Modelling Strategy

A. *The Scope for Minimum Wages to Impact on Profitability*

Following Orley Ashenfelter and Robert S. Smith (1979), consider a profit-maximizing firm employing a quantity of labor (L) at wage rate (W), using other factors at price R, and selling its output at price P. Profits are maximized at $\Pi(W, R, P)$given the values of W, R, and P. The derivative of the profit function with respect to the wage rate is $\partial\Pi/\partial W = -L(W, R, P)$, the negative of the demand for labor. In turn, the second derivative is $\partial^2\Pi/\partial W^2 = -\partial L/\partial W$.

In this setting, the introduction of a minimum wage (M) at a level above that of the prevailing wage reduces firm profits by $\Delta\Pi = \Pi(W, R, P) - \Pi(M, R, P)$. Using a second-order Taylor series this can be approximated as

$$\Delta\Pi \cong -L\Delta W + \frac{1}{2}\frac{\partial L}{\partial W}(\Delta W)^2, \tag{1}$$

where $\Delta W = M - W$. The terms on the right-hand side of equation (1) correspond to the "wage bill" ($-L\Delta W$) and "labor demand" ($½(\partial L/\partial W)(\Delta W)^2$) effects on profits. Note that equation (1) can be rewritten as

$$\Delta\Pi \cong -WL\left(\frac{\Delta W}{W} + \frac{\eta}{2}\left(\frac{\Delta W}{W}\right)^2\right), \tag{2}$$

where $\eta = (W/L)(\partial L/\partial W) < 0$.

In a situation of "no behavioral response," that is no impact on labor demand, the second order effect in (2), $((\eta/2)(\Delta W/W)^2)$, is zero, and the fall of profits that would result from the imposition of a minimum wage M is equal to the proportionate change in the wage multiplied by the wage bill. In the case of a labor demand effect, the second term can offset this profit loss to the extent that firms can substitute away from low-wage workers into other factors (e.g., capital).

Equation (2) also serves to illustrate the inverse relationship between a firm's initial wage and the post-policy change in its profits. It shows that the lower the initial wage, the greater the fall in profits associated with the imposition of a minimum wage. The difference-in-difference models we consider in our empirical modelling strategy (described below) will operationalize this idea by defining treatment groups of more affected firms, and comparison groups of less affected firms, based on their wages prior to the policy introduction.

Normalizing profits on sales revenues, S, to define a profit margin shows that, for the no behavioral response model, in a statistical regression context, the coefficient on the increase in wages caused by the minimum wage ($\Delta W/W$) should simply be equal to the share of the wage bill in total revenue (WL/S):

$$\Delta(\Pi/S) = -\theta\left(\frac{\Delta W}{W}\right), \tag{3}$$

where $\theta = (WL/S)$.

More generally, to the extent there is substitution away from labor, the coefficient on the wage increase, θ, will be less (in absolute terms) than the (initial) wage bill share of revenue. Interestingly, we will show that our empirical results cannot generally reject the simple relationship in equation (3).

It is worth noting that this is consistent with the results in the rather different context of John Abowd's (1989) study of union wage increases and firm performance. Abowd (1989) estimates a version of equation (2) examining the effects of unanticipated increases in the wage bill ("union wealth") on the present discounted value of profits as reflected in changes in stock market values ("shareholder wealth"). He also finds that he cannot reject the simple model where the second order effect is zero. Abowd (1989) interprets this as evidence for strongly efficient union bargains as he focuses on a sample of unionized contracts. Strongly efficient (implicit) bargaining is also an alternative interpretation of our findings as well.[6]

It is worth focusing on some of the economic issues underlying the adjustment mechanisms implicit in the second order term of equation (1). Obviously, the magnitude of these mechanisms depend on the elasticity of the labor demand curve, η. One element of this will be the degree to which labor is substitutable for other factors. Another will be the degree to which the higher wage costs can be passed on to consumers in the form of higher prices. For example, under perfect competition price equals marginal cost, so all the wage costs are reflected in higher prices for consumers. In most oligopoly models, by contrast, mark-ups will fall as some of the wage increase is born by firms (see online Appendix A). Consequently, in our empirical work, we explicitly distinguish between industries with different degrees of product market competition as we expect heterogeneity in the minimum wage effects along this dimension (i.e., a larger effect in the less competitive industries).

The model focuses on the short-run responses of incumbent companies, rather than the long-run equilibrium when the number of firms varies.[7] We believe that the short run is still interesting as researchers cannot be sure how long is the long run (we look up to three years after the introduction of the minimum wage). Since firms that employ low-wage workers may well exit the market, the relevant margin of adjustment will be more exit and less entry. We also examine this explicitly in our empirical analysis.

Finally, when the product market is imperfectly competitive, there may also be effects of the minimum wage on profitability in both the short run and the long run. Appendix A in Draca, Machin, and Van Reenen (2008) discusses these models in some detail, but it is sufficient to note that positive price cost margins are an equilibrium phenomenon in standard industrial organization models such as Cournot or differentiated product Bertrand. For example, consider a Cournot oligopoly where firms have heterogeneous marginal costs and constant returns to scale. Introducing a minimum wage has a differential impact on the firm employing more low-skilled

[6] Although we find this explanation less plausible as the minimum wage mainly binds on those firms and sectors where unions are not present or, if they are, are very weak.

[7] Note that the short-run negative impact on profits will be larger in competitive labor markets than monopsonistic labor markets (see Card and Krueger 1995). In the latter model, there is an offsetting positive effect on profitability when wages increase as worker turnover declines.

workers causing this firm to lose market share and suffer a fall in its price cost margin. However, so long as profits do not fall below the exit threshold, the firm will remain in the market with lower profitability.

B. *Modelling Strategy*

The approach we take to identify minimum wage effects in the context of the above theoretical discussion is in line with the existing literature that analyzes the impact of national minimum wages. Typically, we look at a group of firms that were more affected by the NMW introduction than a comparison set of firms.[8] By "more affected," we mean those firms where wages are likely to increase due to the imposition of the minimum wage. This quasi-experimental setting enables us to compare what happened to profitability before and after NMW introduction in low-wage firms as compared to what happened to profitability across the same period for a comparison group of firms where wages were not affected as much (or at all) by the NMW introduction.

For ease of exposition, we begin our discussion of modelling by thinking in terms of a discrete treatment indicator of the minimum wage policy for a set of low-wage firms with a pre-policy introduction wage, W^{pre}, beneath the minimum wage threshold M. A treatment indicator variable can be defined as $T = 1$ for below minimum wage firms (where $W^{pre} < M$), and $T = 0$ for a set of firms whose pre-policy wage exceeds the threshold.[9]

We can evaluate the impact of minimum wages on firm profitability by comparing what happens before and after minimum wage introduction across these treatment and control firms. For this procedure to be valid, we first need to establish that our choice of affected firms behave as we would expect in response to NMW introduction. The expected response would be that wages rise by more in the $T = 1$ firms before and after introduction as compared to the $T = 0$ firms.

A difference-in-difference estimate of the wage impact of the NMW is $\left(\overline{w}^{T=1}_{NMW=1} - \overline{w}^{T=1}_{NMW=0}\right) - \left(\overline{w}^{T=0}_{NMW=1} - \overline{w}^{T=0}_{NMW=0}\right)$, where $w = \ln(W)$, NMW is a dummy variable equal to 1 for time periods when the NMW was in place (and 0 for pre-policy periods) and a bar denotes a mean. For example, $\overline{w}^{T=1}_{NMW=1}$ is the mean ln(wage) for the treatment group in the post-policy period. This difference-in-difference estimate is just the simple difference in means unconditional on other characteristics of firms. It can easily be placed into a regression context. If $T = 1$ for firms with a pre-policy ln(wage), $w_{i,t-1}$, less than the ln(minimum wage), mw_t, and 0 otherwise, we can enter the indicator function $I(w_{i,t-1} < mw_t)$ into a ln(wage) equation for firm i in year t as follows:

$$w_{it} = \alpha_1 + \beta_1 X_{it} + \delta_1 Y_t + \theta_1 I(w_{i,t-1} < mw_t) + \psi_1[I(w_{i,t-1} < mw_t)\, NMW_t] + \varepsilon_{1it}, \tag{4}$$

[8] See, among others, Card's (1992) analysis of state variations in low pay incidence to identify the employment impact of the US federal minimum wage, or Stewart's (2002) similar analysis of regional variations in the United Kingdom NMW.

[9] We also consider various continuous measures of treatment intensity discussed below.

where X is a set of control variables; Y denotes a set of year effects (hence a linear term in NMW_t does not enter the equation since it is absorbed into the time dummies); and ε_{1it} is a random error. Here, the regression corrected difference-in-difference estimate of the impact of NMW introduction on the ln(wage) is the estimated coefficient on the low wage treatment dummy in the periods when the NMW was in operation, ψ_1.

After ascertaining whether the NMW impacts on wages in the expected manner, we move on to consider whether profitability was affected differentially between the treatment group firms ($T = 1$) and comparison group firms ($T = 0$). We look at unconditional and conditional difference-in-difference estimates in an analogous way to the wage effects. Thus, we can estimate the unconditional difference-in-difference in profit margins, defined as the ratio of profits to sales Π/S, as $[(\overline{\Pi/S})^{T=1}_{NMW=1} - (\overline{\Pi/S})^{T=1}_{NMW=0}] - [(\overline{\Pi/S})^{T=0}_{NMW=1} - (\overline{\Pi/S})^{T=0}_{NMW=0}]$, and the conditional difference-in-difference, ψ_2, from the regression model

$$(5) \qquad \left(\frac{\Pi}{S}\right)_{it} = \alpha_2 + \beta_2 Z_{it} + \delta_2 Y_t + \theta_2 I(w_{i,t-1} < mw_t) + \psi_2[I(w_{i,t-1} < mw_t)\, NMW_t] + \varepsilon_{2it},$$

where the controls are now Z, and ε_{2it} is the error term.

If we compare the econometric models (4) and (5) to the economic models of (1)–(3), we see immediately that the no behavioral response model corresponds to a restriction on the coefficients in equations (4) and (5), i.e.

$$(6) \qquad \psi_2 = -\theta\psi_1.$$

We present formal tests of this restriction in the empirical section.

The main issue that arises with any nonexperimental evaluation of treatment effects is, of course, whether the comparison group constitutes a valid counterfactual. The key conditions are that there are common trends and stable composition of the two groups (see Richard Blundell et al. 2004). Much of our robustness analysis below focuses on whether these two conditions are met, for example, by examining pre-policy trends and carrying out pseudo-experiments (or falsification tests) in the pre-policy period.

II. Data

A. *Basic Description of FAME Data*

Accounting regulations in the United Kingdom require private firms (i.e., those unlisted on the stock market) to publicly report significantly more accounting information than their US counterparts. For example, even publicly quoted firms in the United States do not have to give total employment and wage bills, whereas this is required in the United Kingdom.[10] Accounting information on UK companies is

[10]The lack of publicly available information on private sector firms and on average remuneration may be a reason for the absence of US studies in this area.

stored centrally in Companies House. It is organized into electronic databases and sold commercially by private sector data providers such as Bureau Van Dijk (BVD) from whom we obtained the FAME (Financial Analysis Made Easy) database.[11]

The great advantage of this data is that it covers a much wider range of companies than is standard in firm level analyses and, in particular, it includes firms not listed on the stock market. This means we are able to include many of the smaller and medium-sized firms that may be disproportionately affected by the NMW. Furthermore, the data also covers nonmanufacturing firms where many low-wage workers are employed. By contrast, plant level databases in the United Kingdom and United States typically cover only the manufacturing sector[12] and do not have as clear a measure of profitability as exists in the (audited) company accounts. However, UK accounting regulations do have reporting exemptions for some variables for the smaller firms, so our analysis is confined to a subsample that do report the required information.[13]

Since FAME contains annual accounting information, we have firms reporting accounts with different year-end dates. Since the NMW was introduced on April 1, 1999, we therefore consider the subset of firms that report their end of year accounts on March 31 of each year (these are firms who report in the UK financial year). The accounting period for these firms will match exactly the period for which the NMW was in force. Around 21 percent of firms in FAME that have the accounting data we require report on this day, which corresponds to the end of the tax year in the United Kingdom.[14]

We use data on profits before interest, tax, and depreciation from the FAME database and model profitability in terms of the profit to sales ratio. There is a long tradition in firm-level profitability studies to use this measure, as it is probably the best approximation available in firm-level accounts data to price-cost margins.[15] To allow for capital intensity differences, we also control for firm-specific capital to sales ratio.[16]

B. *Other Data*

We have also matched in industry-level variables aggregated up from the Labor Force Survey (similar to the US CPS). These are used as control variables in the

[11] FAME is the United Kindom's part of BVD's AMADEUS dataset of European company accounts used by many authors (e.g., Nicholas Bloom and Van Reenen 2007).

[12] The Annual Business Inquiry (ABI) database does cover nonproduction sectors, but this database is not available until the late 1990s. The US Longitudinal Research Database (LRD) only covers manufacturing.

[13] These firms will tend to be larger than average as the very smallest firms have the least stringent reporting requirements.

[14] If we estimated our basic models on the whole FAME sample irrespective of reporting month, we obtained very much the same pattern of results as our basic findings in Table 2. The estimated effects were a little smaller in magnitude, most likely because of attenuation toward zero owing to measurement error in defining treatment.

[15] For example, see Machin and Van Reenen (1993) and Margaret E. Slade (2004). Although there are many reasons why accounting and economic profits may diverge (Franklin M. Fisher and John J. McGowan 1983), there is much evidence that they are, on average, highly positively correlated. The relationship between the profit-sales ratio and price-cost margins will also break down if there are not constant returns to scale. In this case, controlling for capital intensity is important in allowing for differential fixed costs across firms, and that is what we do empirically in the regression-corrected difference-in-difference estimates.

[16] We also checked that dropping the capital sales ratio did not change the results as some of the effect of the NMW may have come from firms substituting away from more expensive labor toward capital equipment.

analysis and include (at the three-digit industry level) the proportion of part-time workers, female workers, and union members. We also include skills proxied by the proportion of all workers who have college degrees in a particular region by two-digit industry cell. The control variables in the regression models also include a set of region, two-digit industry and time dummies. Exact variable definitions are given in the Data Appendix. Online Appendix Table B1 shows the characteristics of the treatment and comparison groups for each model.[17]

Finally, the magnitude of the minimum wage increases over our "Policy on" period should be clarified. This period lasts from April 1, 1999 until March 31, 2002 (the end of our sample). Along with the introduction of the minimum wage, there were two upratings of the minimum during this time. The first occurred in October 2000 and saw the minimum wage rise by 10 pence to £3.70. The second uprating a year later was more substantial taking the minimum up to £4.10. Together these upratings constitute a 13.9 percent increase in the minimum between 1999 and 2002.[18] Small cell sizes prevent us from estimating separate models for the 2000 and 2001 upratings.[19]

C. *Defining Treatment and Comparison Groups*

FAME has a total remuneration figure that can be divided by the total number of employees to calculate an average wage.[20] This creates a challenge in terms of defining our treatment and comparison groups since any given level of average wages is, in principle, compatible with a range of different within-firm wage distributions. This makes it hard to measure accurately how exposed each firm's cost structures are to the wage shock brought about by the minimum wage. Any continuous measure of treatment intensity based on the firm average wage is inevitably coarse.

We have used information from FAME, the Labor Force Survey (LFS) and the British Workplace Employment Relations Survey (WERS) to both construct and validate our treatment group indicators. Specifically, the main results use average firm wages from FAME to define our treatment and comparison groups, but we also use LFS information for the industry-level analysis of entry and exit. We use within-establishment information from matched worker-establishment data in WERS to consider the association between low-pay incidence and average wages to assess the effectiveness of this empirical strategy.[21]

To investigate the impact of the minimum wage we have defined our treatment group, T, based upon average remuneration information from FAME. For our initial

[17] Interestingly, the profitability of low-wage firms is higher at the median and mean than comparison group firms. This is not true for firms as a whole, where there is a positive correlation between average firm wages and profits per worker (e.g., Van Reenen 1996). It is because we are focusing on the lower part of the wage distribution that this correlation breaks down.

[18] By contrast, the consumer price index grew by 6.3 percent over the same period.

[19] For example, less than 9 percent of firms report annually on September 30 (i.e., the 12 months immediately before the October upratings).

[20] In almost all firms in the data we use, employment refers to average employment over the accounting period. Firms can report employment at the accounting year or the average over the year, but the overwhelming number of our firms report averaged employment.

[21] Unfortunately, direct linking of data of WERS and FAME is not possible due to confidentiality restrictions.

analysis, we define $T = 1$ for firms with average remuneration of less than £12,000 in the accounting year prior to minimum wage introduction ("low-wage firm").[22] Average remuneration in the treatment group for this threshold is £8,400 which, after allowing for a deduction for nonwage costs (such as employers' payroll tax, pension contributions, etc.), is equivalent to a £3.90 hourly wage for a full-time worker and is close to the NMW (introduced at £3.60 per hour). For our research purposes, the key issue is that the wages of firms beneath the threshold we choose have a significant wage boost from the NMW relative to higher wage firms, and we consider this in detail in our analysis. One aspect of this is that we have extensively experimented with the threshold cut-off, and we discuss this in detail below. We also look at associations with the pre-policy average wage in the firm. This gives a continuous indicator that we can use to compare with the binary treatment variables based upon being beneath a particular wage threshold.

D. *The Usefulness of Average Wages to Define Treatment*

How accurate are these treatment group definitions at identifying firms most affected by the minimum wage regulation? This hinges on how segregated low-wage workers are between firms. Our threshold-based definition will be more effective if subminimum wage employees are concentrated in particular firms at the lower end of the wage distribution.

To assess the usefulness of the approach we adopt, we look at segregation and wages in the 1998 cross-section of WERS.[23] This contains matched worker and establishment data that allows us to look at within-workplace wage distributions and explore the association between average wages and the intensity of low-wage workers. For 26,509 workers in 1,783 WERS workplaces we computed the proportion of workers paid less than £3.60 per hour (the value of the minimum wage when introduced in 1999) and the average hourly wage in the workplace. There is a strong, negative association between the two variables (a correlation coefficient of -0.61, p-value < 0.001). In Figure 1, we plot the proportion of workers paid at or below the minimum wage against the establishment's average annual wage. This proportion of minimum wage workers tapers off rapidly after an average annual wage of £10,000, supporting the idea that exposure to the minimum wage can be proxied by using an average wage threshold that is around this level. Workplaces with average annual wages of £12,000 or less (our main threshold defining the treatment group) contain 87 percent of all minimum wage workers. These patterns give some support to our idea that the "at risk" group of minimum wage workers is concentrated in firms that pay low average wages.

[22] In earlier versions of this paper, we also combined the low-wage firm information with industry-region "cell" data on the proportion of workers beneath the minimum wage in the year before it came into being. Using LFS data, we defined a low-wage industry-region cell if more than 10 percent of workers in the given firm's two-digit industry by region cell in the pre-policy period are paid below the minimum wage. In practice this made little difference to the overall pattern of results, and so we do not report this material (see Draca, Machin, and Van Reenen 2008 for all the results).

[23] WERS is a stratified random sample of British establishments and has been conducted in several waves since 1980. It has been extensively used by economists and industrial relations experts to study a range of issues. Mark Culley et al. (1999) give details of the survey.

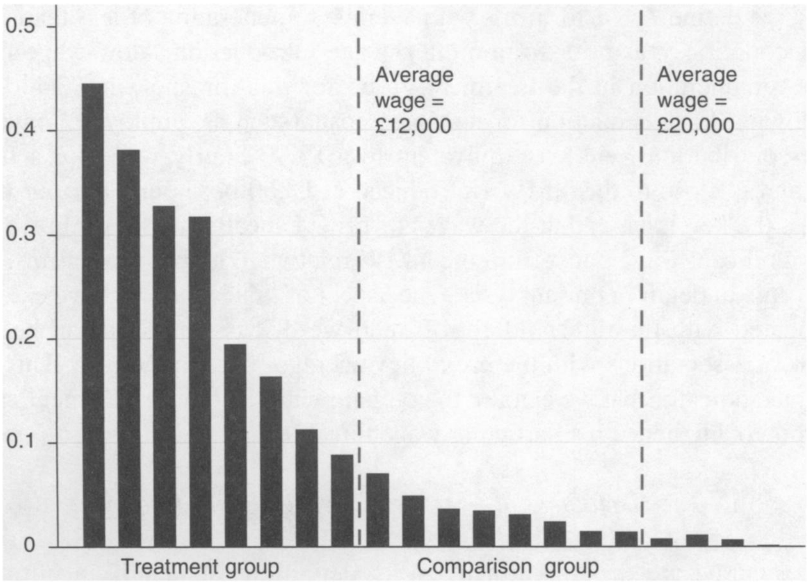

FIGURE 1. VALIDATION OF AVERAGE WAGE DATA
(*Comparison of proportion of low-wage workers and establishment average wages, WERS 1998*)

Notes: The y-axis shows the proportion of workers paid below the minimum wage (£3.60 per hour) in the establishment. The x-axis shows the average annual wage at the workplace. This is divided into bins for 5 percentiles from lowest (left) to highest (right)—a total of 20 bins up to an annualized wage of £24,000. We mark the relevant thresholds for our analysis with vertical lines. The £12,000 line represents the main treatment group threshold used in our analysis of the FAME data. The £20,000 line is the cut-off for the upper bound of the comparison group used in the FAME analysis.

Source: These figures are derived from the worker-establishment data (26,509 workers in 1,783 workplaces) from the 1998 Workplace Employee Relations Survey (WERS).

III. Main Results

A. *Changes in Wages Before and After the Introduction of the National Minimum Wage*

It is important to see whether we are able to observe a clear change or "twist" in the firm average wage distribution as the minimum wage was introduced. To consider this, we started our analysis by calculating the change in average wages in the year immediately before and immediately after NMW introduction for every firm at each percentile of the pre-policy firm wage distribution. If the firms in the FAME data exhibit some of the low pay patterns outlined above for WERS, the minimum wage introduction should raise average firm wages by more in low-wage firms. Thus, we would expect there to be larger changes in firm wages for the lowest percentiles of the distribution.

The results given in Figure 2 very clearly confirm this hypothesis. In the post-NMW introduction year from April 1, 1999 to March 31, 2000 (labeled "1999–2000 change," and denoted by the solid line), the wage change tapers off steadily beyond

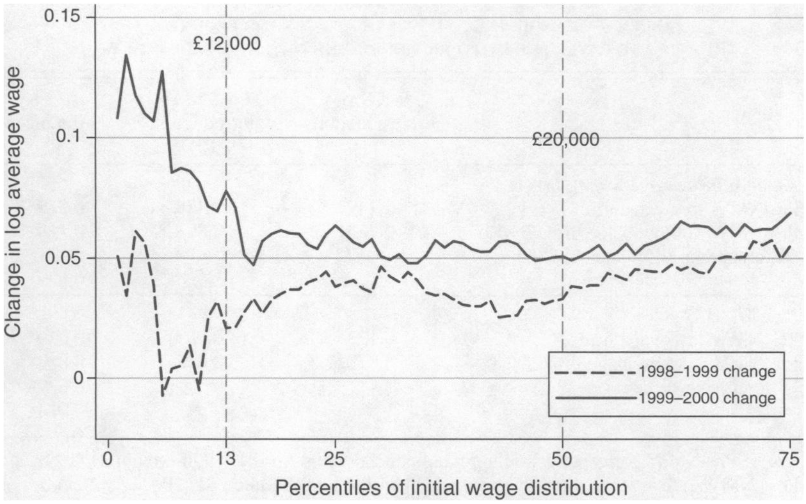

Figure 2. Change in ln(average wage) by Percentile in the Financial Year Before and After NMW Introduction

Notes: The horizontal axis indicates the percentile in the firm wage distribution for a given firm in the initial period, the pre-policy financial year up to March 31, 1999. The vertical axis shows the proportionate change in average firm wages (between the pre-policy financial year and the post-policy financial year) for each firm ranked by where it began in the wage distribution. Pre-policy is defined as the financial year April 1, 1998–March 31, 1999. Policy on is defined as the financial year April 1, 1999–March 31, 2000. We show the threshold for the treatment groups by hatched vertical lines. In the baseline specifications firms with average wages below £12,000 (the thirteenth percentile) are in the treatment group and firms with average wages between £20,000 (the median) and £12,000 are in the control group.

Source: The data is taken from the FAME database of company accounts.

the lowest decile of the firm average wage distribution. After the thirteenth percentile, firms appear to have had a similar increase in nominal wages of around 5.6 percent. Importantly, there is no evidence of much faster wage growth for the bottom decile in the pre-policy year (labeled "1998–1999 change," and denoted by the dotted line). In fact, wage growth in the bottom thirteen percentiles was on average 2.6 percent in the 1998–1999 financial year compared to 9.9 percent in the following year. A spike is seen for the bottom few percentiles of the wage distribution in both years, which is consistent with the notion of some transitory measurement error at the low end of the wage distribution generating mean reversion in both periods. Reassuringly, the general picture follows a similar pattern to that found for individual-level wage data (Dickens and Manning 2004) and, again, provides encouraging evidence that our definition of the treatment group is useful.

It is critical that we identify wage effects from the treatment group definitions so that our analysis of profitability consequences is validated by the minimum wage introduction having a bigger 'bite' on low-wage firms. To make this a tighter definition, we have also defined the comparison group to be those firms with average wages above the £12,000 treatment threshold, but less than £20,000 (the median firm wage), by removing any firms with above £20,000 average wages from the main analysis. We do so since these firms are quite different in terms of

TABLE 1—CHANGES IN FIRM AVERAGE WAGES AND PROFITABILITY BEFORE AND AFTER THE INTRODUCTION OF THE NATIONAL MINIMUM WAGE

	Pre-NMW introduction (1)	Post-NMW introduction (2)	Difference (3)
Panel A. ln(average wage), lnW			
Pre-NMW low-wage firm, $T = 1$	2.149	2.378	0.229
Pre-NMW not low-wage firm, $T = 0$	2.775	2.893	0.118
Difference-in-difference			0.111***
			(0.029)
Panel B. Π/S			
Pre-NMW low-wage firm, $T = 1$	0.128	0.089	−0.039
Pre-NMW not low-wage firm, $T = 0$	0.070	0.058	−0.012
Difference-in-difference			−0.027**
			(0.014)

Notes: Pre-NMW corresponds to the three financial years April 1, 1996–March 31, 1999 and Post-NMW refers to the three financial years April 1, 1999–March 31, 2002. $T = 1$ indicates the treatment group and $T = 0$ indicates the comparison group. Pre-NMW Low-wage firm—the treatment group is defined as firms with an average wage equal to or below £12,000 per annum in the pre-policy financial year up to March 31, 1999; the comparison group is defined as firms with average wages between £12,000 and £20,000 in the pre-policy financial year up to March 31, 1999. Standard errors in parentheses are clustered by firm and sample size is 4,112 (there are 951 firms).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

their characteristics, and therefore subject to different unobservable trends from the treatment group. We are careful to test for the sensitivity of the results to definitions of these thresholds.

B. *Firm-Level Estimates: Wages and Profitability*

The upper panel of Table 1 presents unconditional difference-in-differences in the mean $\ln(wage)$ for the discrete categorization of treatment and comparison groups, for the three years before and after NMW introduction.[24] It is evident that wages rose significantly faster among the low-wage firms when the minimum wage became operational. Wage growth across the pre- and post-NMW three year time period was higher at 22.9 log points in the lower initial wage group ($T = 1$) as compared to wage growth of 11.8 log points in the higher initial wage group ($T = 0$). The difference-in-difference of 11 percentage points is strongly significant in statistical terms. This is consistent with the hypothesis that the NMW significantly increased wages for low-wage firms.[25]

[24] Note that we are looking across the six financial years from April 1, 1996 to March 31, 2002 (three years before the policy and three years afterward). In Figure 2, we simply looked one year before and after the policy introduction.

[25] As we saw in Figure 1, in 1998 (the year prior to the introduction of the NMW in 1999), on average, 25 percent of workers in the treatment group were at or below the minimum wage compared to 3 percent in the comparison group. Based upon this 22 percentage point difference, our coefficients would have to be scaled up by a factor of 4.5 if we considered the more radical experiment of switching a firm from having *none* of its workers covered to having *all* of its workers covered by the minimum wage.

TABLE 2—WAGES AND PROFITABILITY BEFORE AND AFTER INTRODUCTION OF THE NATIONAL MINIMUM WAGE (NMW), 1997–2002

	Period before and after NMW introduction, 1997–2002 ($N = 4{,}112$)	
	Change in ln(*average wage*), $\Delta \ln W$	Change in gross profit margin, $\Delta(\Pi/S)$
Panel A. Treatment = low-wage firm		
Pre-NMW low wage firm	0.090***	−0.029**
	(0.026)	(0.012)
Test of no behavioral response	p-value = 0.663	
Panel B. Treatment = −pre-policy ln(W)		
− Pre-NMW ln(W)	0.188***	−0.032**
	(0.033)	(0.015)
Test of no behavioral response	p-value = 0.144	

Notes: Coefficients estimated by ordinary least squares and standard errors in parentheses below are clustered by firm (there are 951 firms). The pre-NMW period covers the three pre-policy financial years April 1, 1996–March 31, 1999, and the post-NMW period covers the three financial years April 1, 1999–March 31, 2002. Low-wage firm pre-NMW—treatment group is defined as firms with an average wage equal to or below £12,000 per annum in the pre-policy financial year up to March 31, 1999. The comparison group is defined as firms with average wages between £12,000 and £20,000. Pre-NMW ln(W)–indicates that a continuous measure of the wage (in the pre-policy year up to March 31, 1999) is used for treatment intensity. Controls include two-digit industry dummies; 18 regional dummies; the proportion of workers who are graduates (by region and two-digit industry); and union membership, part-time work, and female employment rates (by three-digit industry classification). "Test of no behavioral response" implements equation (3) in the text.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

An analogous set of descriptive results is presented for firm profitability in panel B of Table 1. It is clear that, while profit margins fell by 0.039 between the pre- and post-NMW periods in the pre-NMW low-wage firms, they only fell by 0.012 in the pre-NMW higher wage firms. Thus, there is a negative difference-in-difference of −0.027. This difference is statistically significant and is preliminary evidence that profit margins were squeezed in firms that were "at risk" from the introduction of the minimum wage.

Comparing these results with the simple models in Section I, we find that no behavioral response model does surprisingly well. Using the average wage bill to sales ratio of 0.27 (see Online Appendix Table B1), the implied change of profit margins using the estimated wage gains in Table 1 and equation (3) is −0.030 (= −0.111 × 0.27). This is only slightly above the empirically estimated profitability reduction of −0.027 in Table 1, suggesting only minor offsetting adjustments (the second-order term in equation (2)). Below, we will see that this conclusion broadly holds up to more rigorous econometric testing.

Table 2 reports results from statistical difference-in-difference wage and profitability regressions that additionally control for firm and industry characteristics. The upper panel A shows results for the binary low-wage firm indicator, while the lower panel B uses a continuous measure, the negative of the pre-policy average wage (we report the negative in order to have signs on coefficients that are consistently defined with the low-wage dummy). The basic pattern of results from the unconditional models of Table 1 are confirmed in these conditional specifications. For the binary indicator in the upper panel, the estimated effects show a 9.0 percentage point in wages and a 0.029 fall in profit margins (similar to Table 1). The same pattern of results is observed for the (negative of the) continuous pre-NMW wage, reported in panel B. There is a significant positive connection between wage growth and the negative of the pre-NMW wage, and a significant negative association with profitability. When compared to average profits in the low-wage firms in the pre-policy period, the results for the binary low-wage firm model imply a sizable 22.7 percent (−0.029/0.128) fall in profit margins. The p-values from F-tests of the no behavioral response model are at the bottom of each panel and, again, indicate that we cannot reject the simple model underlying equation (3).

C. *Further Probing of the Baseline Results*

There are many reasons to probe these baseline results more deeply. The first, and obvious, reason is to judge the sensitivity of our definition of pre-policy low wages. Because we do not have data on the individual workers within our FAME firms, we rely on pre-policy low-wage status as being a function of the average wage in the firm. This is less than ideal, even though we have (at least partially) validated its use above with the WERS data, and it is important to study whether the results are robust to alternative ways of defining the threshold between treatment and comparison groups.

We therefore re-estimated the models in Table 2 for a range of different wage thresholds, running from an average wage of £10,000 at £1,000 intervals up to £15,000. The results are reassuring in that they all establish a significant NMW effect of reducing profit margins, with magnitude of the impact varying and becoming slightly larger (in absolute terms) for lower thresholds as we would expect (so there is a bigger impact on the very low-wage firms).[26]

A second possible concern is that our results are simply picking up a relationship between changes in profit margins and initial low-wage status that exists, but has nothing to do with the NMW introduction. We have thus looked at estimates, structured in the same way, from periods *before* the NMW was introduced. One such "placebo experiment" is reported in Table 3, where we examine an imaginary introduction of the NMW on April 1, 1996 (instead of April 1999) and repeat our analysis of wage and profitability changes. Table 3 very much reinforces the results, as we are unable to find any difference in margins between low- and high-wage firms in the period when the policy was not in place. This is consistent with the NMW introduction being the factor that caused margins to fall in low-wage firms.

A related issue is the possibility of pre-sample trends (possibly due to mean reversion) in the wage model. If initially low-wage firms had lower than average profitability growth even in the absence of the policy this would be conflated with the

[26]The profitability impacts for the different $T = 1$ thresholds were: −0.029 (0.014) for £10,000; −0.027 (0.013) for £11,000; −0.029 (0.012) for £12,000; −0.024 (0.010) for £13,000; and −0.014 (0.009) for £14,000.

Table 3—Wages and Profitability before and after Introduction of a Placebo National Minimum Wage (NMW), 1993–1999

	Period before and after "imaginary NMW" introduction, 1993–1999, ($N = 4{,}550$)	
	Change in ln(*average wage*), $\Delta \ln W$	Change in gross profit margin, $\Delta(\Pi/S)$
Panel A. Treatment = low-wage firm		
Pre-"imaginary NMW" low-wage firm	0.033	0.015
	(0.028)	(0.011)
Panel B. Treatment = −pre-policy ln(W)		
−Pre-"imaginary NMW" ln(W)	0.079	0.012
	(0.106)	(0.029)

Notes: Coefficients estimated by ordinary least squares and standard errors in parentheses below are clustered by firm (there are 1,047 firms). The pre-"imaginary NMW" period covers the three financial years April 1, 1993–March 31, 1996 and the post-"imaginary NMW" period covers the three financial years April 1, 1996–March 31, 1999. Low-wage firm pre-"imaginary NMW" treatment group is defined as firms with an average wage equal to or below £12,000 per annum in the pre-policy financial year up to March 31, 1996. The comparison group is defined as firms with average wages between £12,000 and £20,000. Pre-"imaginary NMW" ln(W)–indicates that we use a continuous measure of the wage (in the Pre-"Imaginary NMW" year up to March 31, 1996) is used for treatment intensity. Controls include two-digit industry dummies; 18 regional dummies, the proportion of workers who are graduates (by region and two-digit industry); and union membership, part-time work, and female employment rates (by three-digit industry classification).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

causal effect of the NMW impact on profits. The evidence from Table 3 suggested that there is no trend for wages or profitability in the pre-policy period. Nevertheless, we investigated this issue in more detail by estimating the profits model of Table 2 with a rolling threshold from £10,000 to £15,000 for both the policy and pseudo-experiment periods. That is, we estimate the model for thresholds at each £100 interval in this range and plot the coefficients (see Figure 3). In the policy-on period there is a consistently negative effect of around 2–3 percent no matter how we draw the exact profit threshold. By contrast, in the pre-policy period, there is essentially a zero effect with the point estimates actually positive and around 1 percent.

Draca, Machin, and Van Reenen (2008) report a number of further robustness tests. First, a statistical matching technique by trimming the sample according to the propensity scores of the treatment and comparison groups did not affect the pattern of results.[27] As discussed earlier, our sample seems well chosen with relatively few observations needing to be trimmed to ensure common support. More importantly, the estimated effect of the policy on wages and profitability is significant and similar

[27]The basic method used is that of James J. Heckman, Hidehiko Ichimura, and Petra E. Todd (1997), where propensity scores are estimated and the sample is then trimmed to exclude poorly matched observations without common support. To generate the propensity scores, we used a probit model that included all the control variables used in Table 2.

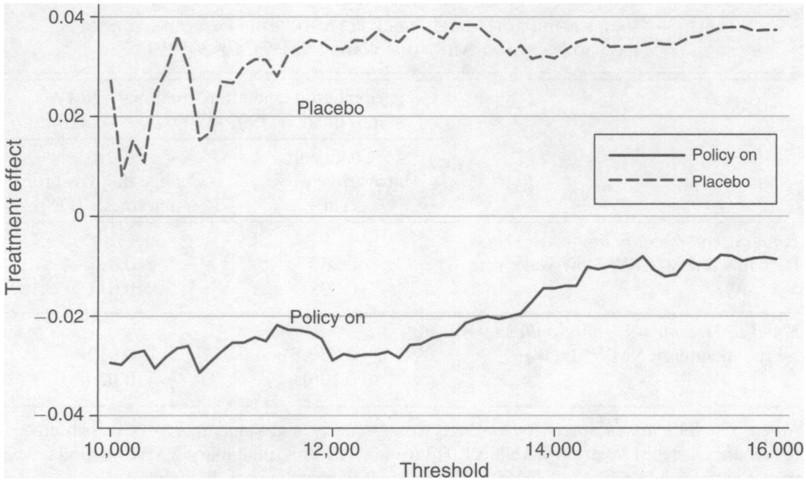

FIGURE 3. VARYING TREATMENT EFFECT COEFFICIENTS IN FAME DIFFERENCE-IN-DIFFERENCE PROFITABILITY MODELS

Notes: The baseline models are as per pre-NMW low-wage model in Table 2 (policy on period) and Table 3 (pre-policy period). The vertical axis shows the estimated treatment effects. The horizontal axis shows thresholds are shifted in units of £100 to define treatment group ($T = 1$) as firms with pre-policy wages of under the threshold and comparison group with firms with average wages over the threshold and under £20,000. The baseline model is then re-defined and re-estimated using 50 successive treatment group wage thresholds between £10,000 and £15,000. The policy on sample period covers the six financial years from April 1, 1996 to March 31, 2002, NMW introduction on April 1, 1999. The pre-policy (pseudo-experiment) period covers the six financial years from April 1, 1993 to March 31, 1999, with an "imaginary" NMW introduction on April 1, 1996.

Source: Data taken from the FAME database of company accounts.

to those in the baseline low-wage firm specification.[28] Second, we included a full set of three-digit industry time trends. Although this is a strong test, the profitability effect was almost identical when these industry time trends were included with an estimate of −0.032 (0.015).

IV. Further Investigation of the Minimum Wage Effect

The baseline results of Section III show very clearly that low-wage firms in the FAME data experienced faster wage growth coupled with falling profit margins before and after the introduction of the UK NMW. The results also seem consistent with the no behavioral response theoretical model introduced in Section II. The model has a number of other salient features that we explore more fully in this section, in an attempt to understand the effect of minimum wages on firm profitability and mechanisms that underpin the negative effect our baseline results have uncovered.

[28] Few observations are lost under propensity score matching because the comparison group is already chosen to be of relatively low-wage firms (under £20,000 average annual wages). If we had used the entire FAME sample (including firms with average wages of over £20,000), we would have had to lose the vast majority of the sample to ensure that the comparison group had common support with the treatment group.

A. *Minimum Wages and Profitability in UK Residential Care Homes*

Here, we look at the wage and profitability effects of the minimum wage in a rather different context, UK residential care homes.[29] There are three reasons to focus on care homes to juxtapose with the FAME results. First, it is a very low-wage sector, so it offers a good testing ground for studying minimum wage effects on profitability and other economic outcomes.[30] Second, the sector is price regulated so one of the margins of adjustment (passing on higher wage costs in higher prices) is constrained. Finally, we have individual level data, so we can observe the entire within-firm wage distribution in this exercise, something we could not do in the FAME dataset.

The more sophisticated definition of treatment we are able to use is the initial firm wage gap relative to the minimum, namely the proportional increase in a firm's wage bill required to bring all of its workers up to the minimum wage. This variable, *GAP*, is defined as

$$GAP_i = \frac{\sum_j h_{ji} \max(W_{ji}^{min} - W_{ji})}{\sum_j h_{ji} W_{ji}}, \tag{7}$$

where h_{ji} is the weekly hours worked by worker j in firm i; W_{ji} is the hourly wage of worker j in firm i; and W_{ji}^{min} is the minimum wage relevant for worker j in firm i.

For care homes, we do not have accounting data, and so the profit variable we study is a derived one based on total revenues less total costs. Total revenue of each home is measured directly as the product of the number of beds, the home-specific average price of beds, and the home occupancy rate. Total costs are calculated by dividing the total firm wage bill by the share of labor in total costs.[31] Home profitability is then defined as the ratio of profits to revenue.

We therefore estimate the following care homes specification

$$\Delta\left(\frac{\Pi}{S}\right)_{it} = \eta_0 + \eta_1 GAP_{i,t-1} + \eta_2 Z_{i,t-1} + \xi_{it}, \tag{8}$$

where ξ_{it} is the equation error. Under the no behavioral response model, the coefficient on *GAP* (η_1) should be equal to the wage bill share of revenues.

Table 4 presents estimates of home-level wage change and profitability change equations for the period surrounding NMW introduction (1998–1999). Panel A

[29] To date these data have mostly been used for studies of minimum wage effects on wages and jobs (e.g., Machin, Manning, and Rahman 2003), but see also Machin and Manning's (2004) test of competitive labor market theory.

[30] Prior to the minimum wage introduction in April 1999, average hourly wages were very low in the sector (at around £4 per hour). On average, 32.2 percent of workers were paid below the incoming minimum wage with this figure falling to 0.4 percent after the introduction of the policy.

[31] Total sales and profits are not reported directly in the care homes data. We calculated them from the underlying home-specific components. Sales (S) is calculated as Occupancy Proportion × Number of Beds × Average Price (all reported in the survey). The wage bill (WB) and the share of labor in total costs ($SHARE$) are also reported directly in the data. We can then calculate total costs (TC) as the ratio of the wage bill to the labor share ($WB/SHARE$). Profits are then simply sales less total costs ($S - TC$). Profitability is the ratio of profits to sales, $(S - TC)/S$.

TABLE 4—NATIONAL MINIMUM WAGE INTRODUCTION AND WAGES AND PROFITABILITY IN CARE HOMES, 1998–1999

	Period before and after NMW introduction, 1998–1999	
Panel A. Wages	Change in ln(*average wage*), $\Delta \ln W$	
Pre-NMW wage gap	0.861***	0.886***
	(0.045)	(0.052)
Controls	No	Yes
Panel B. Profitability	$\Delta(\Pi/S)$, Change in profit margin	
Pre-NMW wage gap	−0.433***	−0.492***
	(0.173)	(0.202)
Controls	No	Yes

Notes: Coefficients estimated by ordinary least squares. Robust standard errors are in parentheses under coefficients. Sample covers 454 nursing homes in 1998 and 1999. Initial pre-minimum wage period ($t-1$) controls include workforce characteristics (proportion female, mean worker age, proportion with nursing qualifications), the proportion of residents paid for by the government ("DSS"), region dummies, and month dummies.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

focuses on wages, and presents results showing that wages clearly rose by more in homes with a larger pre-NMW wage gap. Panel B shows profitability estimates, where the coefficient on the pre-NMW wage gap variable is estimated to be negative and significant. In the column 2 specification with controls, the coefficient is −0.492. Thus, there is clear evidence of profitability falls in homes that were more affected by the minimum wage introduction. This very much corroborates the FAME findings of the previous section.

There was also some evidence that wages rose more in the pre-policy period (1992–1993) in homes with a bigger initial wage gap.[32] Nevertheless, the relationship is much weaker in the earlier period, so the trend-adjusted estimate is statistically significant and large in magnitude (at 0.678). Under the no behavioral response model, the coefficient on the initial wage gap measure should equal the share of the wage bill in sales. The (trend adjusted) point estimate on the wage gap term in the profitability equation turns out to be −0.396 for the model with controls (and −0.343 for the no controls specification), which in absolute terms is very close to the wage bill to sales ratio in our sample of care homes (0.398). Hence, like the FAME results the magnitude of the estimated impact in care homes is very much in line with what we would expect from the simple no behavioral response model.

B. *Sectoral Heterogeneity: Industries with High and Low Market Power*

As noted in Section I, a condition for the existence of long-run effects of minimum wages on profitability is that there is some degree of imperfect competition in the product market. To examine this idea in Table 5, we split industries into "high-" and

[32]We define a counterfactual minimum wage at the same percentile of the wage distribution as the real 1999 minimum, so we can compute a GAP measure for the earlier pre-policy time period. Note that this is the only previous wage change information that exists, as the data was not collected in other (nonelection) years.

TABLE 5—SPLITTING INTO HIGH- AND LOW-MARKET POWER INDUSTRIES

Outcome	High-market power industries	Low-market power industries
Panel A. Wages		
Treatment = low-wage firm	0.109***	0.081**
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.035)	(0.038)
Panel B. Profits		
Treatment = low-wage firm	−0.037**	−0.014
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.018)	(0.014)
Test of no behavioral response	p-value = 0.646	p-value = 0.531
Panel C. Employment		
Treatment = low-wage firm	0.104	−0.012
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.142)	(0.121)
Panel D. Labor productivity		
Treatment = low-wage firm	0.075	0.113
$N = 1{,}943$ (high); $N = 2{,}169$ (low)	(0.066)	(0.090)
Panel E. Exit		
Treatment = low-wage firm	−0.023	−0.002
$N = 1{,}150$ (high); $N = 1{,}206$ (low)	(0.023)	(0.027)

Notes: This table shows the results from a series of separate regressions for the low-wage firm models (Column 1 of Table 2, panel A). The dependent variable is indicated in the first row, column 1 is on the sub-sample of firms in high-market power industries, and column 2 is the sub-sample of firms in the low market power industries. High-market power industries are defined as those with higher than the median value of the industry-level Lerner Index in the firm's three-digit industry. Low-market power industries are defined as those with below the median value of the industry-level Lerner Index in the firm's three-digit industry. Coefficients estimated by ordinary least squares and standard errors in parentheses below are clustered by firm. Employment is the ln(total number of workers in the firm). Labor productivity is ln(sales/employment). "Exit" is defined for two cohorts in 1996 (pre-NMW) and 1999 post-NMW and indicates whether the firm ceased to exist in the subsequent three years (see text). Controls include two-digit industry dummies; 18 regional dummies, the proportion of workers who are graduates (by region and two-digit industry); and union membership, part-time work, and female employment rates (by three-digit industry classification).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

"low-" competition industries based on a proxy for the Lerner Index (constructed as in Philippe Aghion et al. 2005). Consistent with the idea of imperfect competition, the effects of the NMW policy on profitability were stronger in the less competitive sectors (defined as those with above the median value of three-digit industry Lerner index). Table 5 shows that the impact of the policy on wages was not so different (10.9 percent versus 8.1 percent). By contrast, the effect of the minimum wage on profitability was almost two-and-a-half times as large in the less competitive industries as in the more competitive sectors (as well as being significant only in the less competitive sectors).

Under perfect competition, an industry facing a common increase in marginal costs will pass on the higher wage costs in the form of higher prices to consumers. In less competitive sectors, however, firms will generally adjust by reducing their profit margins, rather than just through prices. Therefore, the evidence in Table 5 is consistent with the idea that the strongest effects of the NMW on profitability will be in the less competitive sectors.

TABLE 6—FIRM ENTRY AND EXIT (*by three-digit industry*)

	Period before and after NMW introduction, 1996–2001, ($N = 1{,}020$)	Period before and after "imaginary NMW" introduction, 1994–98, ($N = 850$)	Difference
Panel A. Change in industry entry rates			
Pre-NMW low pay proportion	0.021	0.057*	−0.036
	(0.015)	(0.032)	(0.038)
Panel B. Change in industry exit rates			
Pre-NMW low pay proportion	−0.013	−0.028	0.015
	(0.016)	(0.018)	(0.024)
Panel C. Change in industry net entry rates			
Pre-NMW low pay proportion	0.034	0.085**	−0.051
	(0.025)	(0.027)	(0.037)

Notes: Entry rate is the proportion of firms who are newly registered in a year in a three-digit industry. Exit rate is the proportion of firms who are deregistered in the year. Net entry is entry rate–exit rate. Standard errors (in parentheses) are clustered by three-digit industry. Pre-NMW low pay proportion is the proportion of workers with an hourly wage less than £3.60 in the three-digit industry in real terms over the pre-policy period (the minimum wage threshold of £3.60 is deflated by the retail price index for the years 1994–1998). All specifications include controls for two digit industry dummies, time dummies, and the proportion of employees in the three-digit industry that are female, part time, and the proportion of employees in the three-digit industry that are female, part time, and unionized.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Data taken from value-added tax (VAT) Registrations and Deregistration Data, Department of Trade and Industry (DTI).

C. *Effects of Minimum Wage on Other Outcomes: Employment, Productivity, Exit, and Entry*

We also examined the effect of the NMW policy on other firm outcomes in the lower part of Table 5, again split by high and low market power sectors. We do not find any significant negative effects on employment, consistent with some of the minimum wage literature (e.g., Card and Krueger 1994). The presence of no significant employment effect is also consistent with our tests of the no behavioral response model. Similarly, there does not appear to be any effect of the policy introduction on labor productivity (as predicted by the "shock" theory).

The FAME database identifies four categories of inactive firms, namely firms that are dissolved, liquidated, in receivership, or currently nontrading.[33] Hence, we have defined all firms in these categories as "exiting" firms. We examine three year death rates for a cohort alive April 1, 1999 (i.e., did they exit by March 31, 2002) compared to a cohort alive on April 1, 1996 (i.e., did they exit by 1999). In the final row of Table 5, there is no evidence of any faster increase in exit rates in initially low-wage firms following the minimum wage introduction either in the whole sample or

[33] So exits by takeover are *not* coded to be unity in this definition as takeovers may be regarded as a sign of success rather than failure. Redefining the dependent variable to be unity if the exit is to a takeover does not change the qualitative nature of the results.

in subsectors. The same is true in models of the probability of closure of care homes (see Machin and Joan Wilson 2004).

There are two possible problems with this firm-level analysis of exit. First, we ignore the possible entry-deterring effect of the minimum wage, and second, there may be pre-policy trends.[34] Table 6 takes both of these into account. Obviously, we cannot implement this at the firm level, as entrants do not have a pre-policy wage for the entrants. However, we can examine an alternative dataset containing all entrants and exits in each three-digit sector (from the Department of Trade and Industry's VAT Registration Database).[35]

The three panels of Table 6 show one-year entry rates, one-year exit rates, and the difference between the two ("net entry") three-digit industries. Column 1 shows estimated coefficients on a pre-NMW low-pay proportion in the period surrounding NMW introduction. Column 2 does the equivalent experiment for an imaginary/placebo policy (as in Table 3) introduced in 1996, and column 3 presents the trend-adjusted difference-in-differences. Although the first row shows that entry rates appear to perversely increase for low-wage firms after the minimum wage, there does appear to be some positive pre-policy trend in column 2, suggesting a negative trend-adjusted effect of the NMW policy on entry. Similarly, trend-adjusted exit rates in panel B are 1.5 percentage points higher after the minimum wage was introduced. The final row shows that trend-adjusted net entry rates had fallen by about 5.1 percentage points in the low-wage industries after the NMW introduction. This effect is large in magnitude, but not statistically significant. These results do hint that in the long run a margin of adjustment may be in the dimension of lower rates of net entry into the sectors most affected by the NMW.[36] There is little within firm change, but the margin of adjustment may be through the long-run number of firms.

V. Conclusions

This paper considers a very under-studied research question on the economic impact of minimum wages by looking at empirical connections between minimum wage legislation and firm profitability. Using the quasi-experiment of the introduction of a national minimum wage to the UK labor market in 1999, we utilize pre-policy information on the distribution of wages to construct treatment and comparison groups and implement a difference in differences approach. We report evidence showing wages were significantly raised, and firm profitability was significantly reduced by the minimum wage introduction. There is also some evidence of bigger falls in margins in industries with relatively high market power, but no effects on firm employment or productivity. Somewhat surprisingly, our findings are consistent with a simple "no behavioral

[34] Running the pseudo-policy experiment of Table 3 gave a coefficient on the policy variable of 0.021 with a standard error of 0.106 for employment and 0.077 with a standard error of (0.053) for productivity.

[35] Unlike the firm data, we cannot distinguish between exit due to takeover and exit due to bankruptcy. Online Appendix Table B2 describes some key features of these data.

[36] Our further investigations indicated that there were minimal differences in entry and exit rates between high- and low-market power industries. For example, when split by market power, the corresponding estimates for column 1, panel A, in Table 6 were 0.025 (0.022) for high and 0.019 (0.020) for low.

response" model where wage gains from minimum wages map into profit reductions. There is a hint that the long-run adjustment may be through lower rates of net entry.

There are, of course, a number of caveats to our results. It would have been useful to have data on prices and quality to see if these may also have adjusted in response to minimum wages.[37] It would also be useful to have more information on the within firm distribution of workers in other sectors besides care homes. A fuller integration of theory and empirical work in the context of imperfect competition in both product and labor markets is another fruitful research area for the future. Overall given the total sparsity of evidence of the impact of minimum wage floors on firm profitability, we believe this study is an important contribution looking at the impact of labor market regulation on *firms* as well as the more developed and extensive evidence base that exists studying the impact on individuals.

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The Spending and Debt Response to Minimum Wage Hikes†

By Daniel Aaronson, Sumit Agarwal, and Eric French*

Immediately following a minimum wage hike, household income rises on average by about $250 per quarter and spending by roughly $700 per quarter for households with minimum wage workers. Most of the spending response is caused by a small number of households who purchase vehicles. Furthermore, we find that the high spending levels are financed through increases in collateralized debt. Our results are consistent with a model where households can borrow against durables and face costs of adjusting their durables stock. (*JEL* D12, D14, D91, J38)

Many US social insurance programs provide economic assistance to low-income households. Yet there is little evidence on the spending response to income changes among such households. In this paper, we estimate the magnitude, composition, distribution, and timing of the income, spending, and debt responses to minimum wage hikes among households with adult minimum wage workers. We find that spending and debt rise substantially for a small set of these households following a minimum wage hike. These findings are consistent with a model where households can borrow against durables and face costs of adjusting their durables stock, suggesting that borrowing constraints and adjustment costs are important factors driving spending patterns among low-income households.

Using panel data from the Consumer Expenditure Survey (CEX), Survey of Income and Program Participation (SIPP), Current Population Survey (CPS), and administrative bank and credit bureau data, we identify households with adult minimum wage workers when the household is first observed. We then measure their spending, income, and debt before and after a minimum wage hike. Identification is based on a fixed effects procedure that compares households with minimum wage

*Aaronson: Federal Reserve Bank of Chicago, 230 South La Salle Street, Chicago, IL 60604 (e-mail: daaronson@frbchi.org); Agarwal: National University of Singapore, 15 Kent Ridge Drive, Singapore, 119245 (e-mail: ushakri@yahoo.com); French: Federal Reserve Bank of Chicago, 230 South La Salle Street, Chicago, IL 60604 (e-mail: efrench@frbchi.org). We thank Jeff Campbell, Chris Carroll, John Ham, Leslie McGranahan, Robert Moffitt, Victor Rios-Rull, Stephen Shore, Nick Souleles, Richard Spady, Irina Telyukova, Bill Wascher, Jim Ziliak, the referees, seminar participants at the Chicago Fed, Board of Governors, Federal Trade Commission, University of Illinois-Urbana Champaign, University of Alberta, Johns Hopkins, University of Maryland, Western Ontario, AEA, APPAM, and SED meetings, NBER Summer Institute, Micro and Macro Labor Market Models Conference at UCSB, and Jon Davis, Dan DiFranco, Phil Doctor, Charles Doss, Kyung Park, Shani Schechter, and Zach Seeskin for outstanding assistance. The views expressed in this paper do not necessarily reflect those of the Federal Reserve Bank of Chicago or the Federal Reserve System.

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workers in states that experience a minimum wage increase to similar households in states that do not.

We present four key empirical findings. First, a $1 minimum wage hike increases household income by roughly $250 and spending by approximately $700 per quarter (in 2005 dollars) in the year following a minimum wage hike. These findings are corroborated by independent data showing that debt rises substantially after a minimum wage increase. Second, the majority of this additional spending comes from a small number of households purchasing debt-financed new vehicles.[1] Third, total spending increases within one quarter of a minimum wage increase and not prior, despite legislation typically passing 6 to 18 months before enactment. Finally, high levels of durables spending and debt accumulation persist for several quarters after a minimum wage hike. These results are robust to changes in sample selection criteria and covariates. Furthermore, we find that a minimum wage hike has no income or spending effect on households with workers earning at least double the minimum wage, providing further evidence that our estimates are not the result of omitted variables.

We consider whether various permutations of the life-cycle model can fit the facts above. Two canonical models—the permanent income model and the buffer stock model with no borrowing—fail to do so. If households were spreading an income gain over their lifetime, as in the permanent income hypothesis, the short-run spending increase should be much smaller than what we observe in the data. Augmenting the permanent income model to account for durables raises the predicted short-term spending response. It is still an order of magnitude smaller than what our empirical estimates imply, however. Moreover, a buffer stock model in which households cannot borrow against durable goods generates a spending response of approximately $200 and fails to explain why some minimum wage households increase their debt after a minimum wage hike.

Next, we consider an augmented buffer stock model in which households are collateral constrained—i.e., they can borrow against part, but not all, of the value of their durable goods. If households face collateral constraints, small income increases can generate small down payments, which in turn can be used for large durable goods purchases. With a 40 percent down payment, each additional dollar of income can be used to purchase $\frac{1}{0.4} =$ \$2.50 of durable goods.

While this model fits the data better than the others, it still underpredicts the total spending response. Furthermore, it does not match the highly concentrated distribution of additional spending. Augmenting the model to allow for a cost of adjusting durables better replicates the skewness of the spending responses, but produces a smaller mean spending response. Assuming more widespread borrowing

[1] A large response in durables spending is consistent with many papers that focus on sizable disposable income changes, including those based on tax refunds (Parker 1999, Souleles 1999, and Parker et al. 2010), the Earned Income Tax Credit (EITC) (Barrow and McGranahan 2000; Adams, Einav, and Levin 2009), job loss (Browning and Crossley 2009), expansions in public health insurance programs (Leininger, Levy, and Schanzenbach 2010), and other large income changes (Krueger and Perri 2008). Moreover, Adams, Einav, and Levin (2009); Souleles (1999); Leininger, Levy, and Schanzenbach (2010); and Parker et al. (2010) also find evidence that much of this additional durable spending is on vehicles. Other papers find no response in durable spending (e.g., Browning and Collado 2001, and Hsieh 2003) or a highly imprecise response (e.g., Coulibaly and Li 2006). Our reading of the literature is that positive effects tend to be found in papers based on large income gains among more liquidity constrained households.

constraints among minimum wage households, the model generates an almost \$700 spending response.

Models where households can borrow against durable goods are increasingly common for understanding the dynamics of consumer durables (Fernandez-Villaverde and Krueger 2011, Campbell and Hercowitz 2003), housing (Carroll and Dunn 1997; Attanasio, Leicester, and Wakefield 2011; Hryshko, Luengo-Prado, and Sorensen 2010; Cerletti and Pijoan-Mas 2012), and entrepreneurship (Kaboski and Townsend 2011). There is little direct micro evidence, however, on the quantitative importance of the constraint. Our paper provides such evidence.

In the aggregate, the spending effect that we estimate is nontrivial. For example, CPS data show that 7.3 million households earned at least 20 percent of total household income from adult minimum wage earnings in 2006. Our estimated \$700 average quarterly spending response thus translates into an additional \$5 billion (= 7.3 million × \$700) in spending per quarter in the year following the hike. That said, this simple calculation likely overstates the true aggregate response. First, our estimates apply to households with a minimum wage worker prior to an increase in the minimum wage. It is possible that raising the minimum wage reduces the odds that those without a job will find one. Second, we ignore most teenagers, who comprise 29 percent of all minimum wage workers. There is stronger evidence of disemployment effects for teenagers than adults. Finally, minimum wage hikes cause prices of goods produced by minimum wage workers to rise (Aaronson 2001; Aaronson, French, and MacDonald 2008). Thus, real income and spending by non-minimum wage workers will likely fall. For those adults who had a minimum wage job prior to a minimum wage hike, however, spending (particularly on vehicles), income, and debt rise afterward.

The rest of the paper is organized as follows. Section I provides a brief description of the CEX, SIPP, CPS, and administrative bank and credit bureau datasets used to estimate the spending, income, and debt responses. Section II describes the empirical results. Section III outlines a calibrated model of household spending responses to a minimum wage increase when borrowing constraints are present versus absent and links these results to the empirical findings. Section IV concludes.

I. Data

This section describes the data that we use to measure income, wages, spending, and debt. Online Appendix A and online Appendix Table A1 provide additional description of the data and sample selection criteria. All nominal values are reported in 2005 dollars.

Our empirical analysis draws heavily from the CEX, a representative sample of US consumer units providing detailed information on household spending.[2] The surveys span 1982 through 2008, a period in which six federal and numerous state minimum wage increases were enacted. The CEX interviews households up to five times, spaced three months apart. In each interview after the first, households are asked about detailed spending patterns for the previous three months. While this

[2]For ease of exposition, we refer to consumer units as households.

design provides monthly data, we follow Johnson, Parker, and Souleles (2006) and aggregate to the quarterly frequency.

In the second and fifth interviews, households are also asked about each member's income and hours worked over the previous year. This information is used to calculate the hourly wage of the first two adult (older than 18) members of the household, which is compared to the state's effective minimum wage to identify minimum wage workers and households. After sample restrictions described in online Appendix A, we are left with 200,549 household-survey observations on spending, of which 11 percent derive some income from minimum wage work.

Two additional datasets—the 1983 to 2007 SIPP and the 1980 to 2007 outgoing rotation files of the CPS—are used to measure income patterns following a minimum wage increase. We show these results because of the larger samples (809,631 and 474,758 observations for the CPS and SIPP, respectively) and because each are designed specifically to measure higher frequency earnings and wages. For the purpose of identifying minimum wage workers, it is particularly useful that both surveys report the hourly wage of those paid by the hour. SIPP and CPS variables are coded, and wage, self-employment, and family composition restrictions are introduced, to be as close as possible to the CEX sample.

Finally, to verify the spending patterns documented in the CEX, we use a proprietary dataset from a large, national financial institution that issues credit cards. This institution merges in quarterly credit bureau reports about each credit card holder's auto, home equity, mortgage, and credit card balance to her credit card account. We draw two samples from this data: a $2\frac{1}{2}$ year overlapping panel containing 4,610,497 observations from 1995 to 2008 and a separate sample of 644,037 observations that begins in January 2000 and runs for 4 years. This is not a random sample of households since an individual needs a credit card to be in this dataset: see online Appendix A.

We obtained state minimum wage histories from the January issues of the *Monthly Labor Review*. See online Appendix Table A2 for a list of minimum wage levels by year and state.[3]

II. Empirical Results

A. *Estimating Equations*

Our empirical strategy is standard. We estimate equations of the form

$$z_{it} = f_i + \sum_{k=-K}^{K} \phi_k w_{\min, it+k} + \boldsymbol{\omega}'\mathbf{x}_{it} + u_{it}, \tag{1}$$

where z_{it} is either income (estimated from the CEX, CPS, and SIPP), spending (estimated from the CEX), or change in debt (estimated from the credit bureau data), and $w_{\min, it+k}$ is the minimum wage rate for the state that individual i resides in at time $t + k$;[4] $\mathbf{x}_{it}$ includes year and quarter dummies or month dummies, and f_i is

[3] We do not account for within-state differences in the minimum wage (i.e., the living wage initiatives that sprung up in a few cities during the 2000s).

[4] When using quarterly CEX and debt data, $w_{\min, it+k}$ is the average value of the minimum wage over the quarter.

a household fixed effect.[5] The ϕ_k parameters are separately identified from the time dummies and household fixed effects because many states raise the minimum wage above the federal minimum. Thus, we can control for time effects, and in so doing, the possibility that both the minimum wage and household spending rise in response to strong aggregate income growth.

Equation (1) is estimated separately for minimum wage and nonminimum wage households. In particular, let S_i be the share of total household income that is derived from adults earning 60–120 percent of the minimum wage:

$$(2)\quad S_i = (E_{1i} \times I\{0.6w_{\min,i} \leq w_{1i} \leq 1.2w_{\min,i}\} + E_{2i} \times I\{0.6w_{\min,i} \leq w_{2i} \leq 1.2w_{\min,i}\})/F_i,$$

where E_{1i} and E_{2i} are the salary income for persons 1 and 2 (typically, the head and spouse), F_i is total pretax nonasset income, and $I\{0.6w_{\min,i} \leq w_{1i} \leq 1.2w_{\min,i}\}$ and $I\{0.6w_{\min,i} \leq w_{2i} \leq 1.2w_{\min,i}\}$ are indicators of whether persons 1 and 2 earn between 60 and 120 percent of the minimum wage, all measured in the first period the household is observed.[6]

We report estimates of ϕ_k for households with no initial minimum wage earnings $(S_i = 0)$, households with any adult minimum wage earnings $(S_i > 0)$, and households with at least 20 percent of total income from adult minimum wage earnings $(S_i \geq 0.2)$. The latter highlights those households that rely more extensively on minimum wage income.[7]

The credit bureau data contain the self-reported annual earnings of the account holder at the time of the credit card application but not hours worked necessary to construct S_i.[8] Therefore, the debt regressions weight the minimum wage variable $w_{\min,it+k}$ in equation (1) by the probability that the holder is a minimum wage worker, P_i. In other words, we assume spending is as in equation (1) with probability P_i and is equal to $f_i + \boldsymbol{\omega}'\mathbf{x}_{it} + u_{it}$ with probability $(1 - P_i)$, which gives rise to the following regression:

$$(3)\qquad z_{it} = f_i + \sum_{k=-K}^{K} P_i \phi_k w_{\min,it+k} + \boldsymbol{\omega}'\mathbf{x}_{it} + u_{it}.$$

To compute the weights, we use the CPS to estimate a probit model of whether a non–self-employed worker was within 120 percent of the minimum wage. Covariates are a quartic in annual earnings, a quartic in age, an age times annual earnings quartic, female, married, and female times married. The estimated probit model reveals that just under 60 percent of all individuals earning \$10,000 per year are minimum wage

[5] When available, we also condition on the number of adults and the number of kids in the household in order to be consistent with other research (e.g., Johnson, Parker, and Souleles 2006). Once the household fixed effect and time dummies are included, however, we find no observable covariates in the CEX or the debt data that substantively impact our coefficient of interest, ϕ_k.

[6] Previous research (e.g., Card and Krueger 1995, Wellington 1991, Lee 1999) has shown that minimum wage hikes increase the wages of workers that make slightly above the minimum wage. Thus, we assume that those earning up to 120 percent of the minimum wage are impacted by the minimum wage, but the results are not sensitive to other reasonable values.

[7] Results are not sensitive to other reasonable S_i thresholds, such as 10 and 30 percent.

[8] Technically, we only have information for individual card-holders, not the unit of interest, the household. We partially circumvent this limitation since debt contracts are typically written at the household level. Therefore, the credit bureau data are often, but not always, at the household level.

Table 1—Total Household Nonproperty Quarterly Income Response to Change in the Minimum Wage

Share of income from minimum wage jobs (S_i)	CEX (1)	CPS (2)	SIPP (3)	Weighted average[a] (4)	"Minimum wage" worker = 120 to 300% of minimum[b] CEX (5)	CPS (6)	SIPP (7)	Weighted average[a] (8)
0	−83	−29	118	14	−54	55	−12	28
	(233)	(42)	(63)	(35)	(432)	(98)	(130)	(77)
	92,810	688,356	420,634		37,997	153,340	112,022	
>0	247	276	178	242	−86	15	181	58
	(399)	(102)	(138)	(80)	(237)	(45)	(72)	(38)
	11,978	121,275	54,124		54,813	535,016	308,612	
≥0.2	−138	247	254	237	−170	8	200	50
	(450)	(105)	(129)	(80)	(222)	(44)	(76)	(38)
	8,511	93,846	39,472		50,102	501,925	276,213	
Time period	1983–2008	1980–2007	1986–2007		1983–2008	1980–2007	1986–2007	
Sample of workers[c]	All	Hourly wage workers	Hourly wage workers		All	Hourly wage workers	Hourly wage workers	

Notes: Each cell represents a separate regression; S_i is the share of pretax total household income from near minimum wage salaries earned by the top two adults in the household. See the text for additional details. All standard errors are cluster corrected by household (consumer unit in CEX).

[a] The weighted average estimate uses a GMM formula where weights are based on the precision of the individual estimates.

[b] Columns 5 to 8 show the "minimum wage effect" for workers that are between 120 and 300 percent of the minimum wage. These regressions drop households with workers that are 120 percent or less (i.e. $S_i > 0$ in columns 1 to 3) of the minimum wage.

[c] The CEX sample includes all workers and is based on a computed wage equal to annual earnings divided by annual hours worked. The SIPP and CPS samples consist of households with a worker who is paid by the hour.

workers, whereas only 6 percent of individuals earning over $20,000 per year are minimum wage workers. We therefore present the results separately for individuals whose earnings at credit card application are above and below $20,000.

B. *The Magnitude of the Income Response*

Table 1 begins by documenting the impact of a $1 increase in the minimum wage on household income. In these initial results, we ignore dynamics and set $K = 0$ in equation (1).[9] Each cell in the table represents a different regression. The top number is the point estimate, the second number is the standard error corrected for within-household serial correlation, and the third is the sample size. Rows are organized by S_i, the share of household head and spouse earnings that come from employment at minimum wage jobs as measured at the time the household enters the survey. Thus, the first row includes households with no initial minimum wage income ($S_i = 0$) and the next two include households where total household income includes any ($S_i > 0$) or at least 20 percent ($S_i \geq 0.2$) adult minimum wage earnings.

Column 1, based on the CEX, shows that a $1 increase in the minimum wage causes after-tax income to rise among $S_i > 0$ households.[10] In contrast, there is

[9] A handful of studies have estimated similar income equations. Recent examples include Draca, Machin, and Van Reenen (2011); Addison, Blackburn, and Cotti (2008); and Neumark, Schweitzer, and Wascher (2004, 2005). Each of these studies finds evidence that minimum wage hikes increase household income in the short run.

[10] The after-tax income measure is based on self-reported federal, state, and local, and other taxes paid. It does not include payroll taxes.

no income increase among households without minimum wage income. Precision is very low, however, and consequently the estimates among the minimum wage households are not stable across different S_i thresholds. Indeed, the point estimate on $S_i \geq 0.2$ households is negative, albeit with a standard error four times as large.[11]

Therefore, the next two columns provide estimates from the CPS and SIPP.[12] For households with at least 20 percent minimum wage income, we find that quarterly earnings rise by \$247 (\$105) and \$254 (\$129) in the CPS and SIPP immediately after a \$1 minimum wage increase. The final column reports a weighted average income response, where the weights are based on the precision of the three individual estimates. These calculations suggest that, in the near term, $S_i > 0$ and $S_i \geq 0.2$ household quarterly income rises by roughly \$240 with a standard error, calculated using standard generalized method of moments (GMM) formulas, of \$80.[13]

By comparison, the effect on nonminimum wage households is not statistically different from 0 (\$14 with a standard error of \$35), suggesting the impact of the minimum wage law is limited to households with workers very close to their state's effective minimum wage. That is also the case when, as a finer test, we look at households near the minimum wage but not necessarily directly impacted by the law. Columns 5 to 8 define S_i as the share of income earned by adult workers with a wage between 120 and 300 percent of the minimum wage.[14]

For households with such earners, we find no evidence of an income gain after a minimum wage increase in the CEX and CPS, although we observe a notable gain in the SIPP. A weighted average of the three datasets suggests the income gain is economically small and statistically indistinguishable both from zero and from the near zero gain among those with hourly wages more than triple the minimum (column 8, row 1). Moreover, the SIPP income gain is concentrated in households earning 120 to 200 percent of the minimum wage. Excluding these SIPP households that might plausibly be contaminated by the minimum wage law change (e.g., Card and Krueger 1995, Wellington 1991, Lee 1999), the estimated (but unreported) income gain among 200 to 300 percent households is \$28 (\$89) and the weighted average among the three datasets is \$7 (\$54).

It is important to note that household income need not rise among minimum wage workers if the legislated minimum wage increase leads to enough job loss. That does not appear to be the case, however. In online Appendix Table A3, we show that employment and hours do not fall after a minimum wage increase among our samples of adult CPS workers. Rather, wages rise among workers in minimum wage

[11] Reasonable alternative wage restrictions, such as dropping the top and bottom 1 percent, or not including a wage restriction results in positive point estimates.

[12] Unlike the CEX, these samples are restricted to households with hourly workers. As expected, when we use a computed wage, we find smaller earnings responses. The CPS and SIPP earnings measures are also pretax. In the CEX, we found the tax adjustment makes little difference to our estimates.

[13] An alternative way to compute the weighted average estimate is through a pooled regression with all three datasets with a full set of survey $\times$ covariate interactions. While there are important differences between the datasets (e.g., earnings refers to the previous year in the CEX but to the previous month in the CPS), we get similar results to column 4. For $S_i = 0$ households, the pooled estimate is \$60 (\$42). For $S_i \geq 0.2$ households, the pooled estimate is \$245 (\$90).

[14] These samples exclude households with an adult worker within 120 percent of the minimum. That is, they only include the $S_i = 0$ households from columns 1 to 3, thereby comparing households with workers paid 120 to 300 percent of the minimum to those households where the adult workers earn over 300 percent of the minimum.

households and not among nonminimum wage households, explaining the majority of the earnings pattern in Table 1.[15]

Beyond the first few quarters, the long-run effect of the minimum wage on income is more difficult to measure with existing data. Neumark, Schweitzer, and Wascher (2004, 2005) find that any income gain from a minimum wage increase dissipates substantially, perhaps even evaporates, within two years. This result is consistent with the empirical finding that many individuals who earn the minimum wage at a point in time will earn well above the minimum wage two years later (Smith and Vavrichek 1992; Carrington and Fallick 2001). Indeed, we find that only 64 percent (53 percent) of SIPP workers who make between 60 and 120 percent of their state's effective minimum wage are still within that range one (two) years later.

C. *The Magnitude of the Total Spending Response*

Table 2 reports the size of the spending response to a minimum wage increase. Like Table 1, each cell represents a separate regression and rows are stratified by S_i, the share of household income from minimum wage jobs.

Column 1 shows that total spending increases by an economically important and usually statistically significant amount for minimum wage households. Among households where minimum wage labor is the source of at least 20 percent of household income, total spending rises by \$815 (standard error of \$457) per quarter, representing 13 percent of an average quarter's spending (column 6).[16] In contrast, spending among households without minimum wage workers does not respond to a minimum wage change (−\$57 with a standard error of \$150). Moreover, the spending response, like the income response reported in Table 1, is not statistically different from 0 among households with workers that are 120 to 300 percent above the minimum wage (column 2, rows 2 and 3). This finding confirms that the spending effect is likely caused by the minimum wage and not by state-specific unobservable trends in consumption that are specific to low-wage families.

This basic pattern is robust to many perturbations of the sample and the statistical model. In column 3, we show that the spending response is large for households that might be particularly liquidity constrained. Liquidity constraints are proxied, as in Johnson, Parker, and Souleles (2006), by whether a household's balance in checking and savings accounts is below \$5,000. The results are also strongest in states that instituted substantial hikes (column 4 versus 5).[17] More generally, we find similar estimates when we remove data restrictions on family composition, age, wage levels, and wage changes, or control for other factors in the regressions, such as state-specific time trends, the age of the head, interview fixed effects, and changes to other

[15] Among $S_i > 0$ households, average wages rise by roughly \$0.47 per hour. Household hours worked per week average about 50. That implies roughly a \$300 increase in quarterly earnings ($0.47 \times 50 \times 13$ weeks). There is also a small, positive hours impact of about one hour per week, mostly driven by spouses that would add roughly \$50 in earnings per quarter at the average minimum wage over this period.

[16] We also estimated a version of equation (1) in first differences. For $S_i \geq 0.2$ households, total spending increases by \$658 (\$522) in the quarter of the minimum wage increase. For $S_i = 0$ households, the total spending effect is \$23 (\$180).

[17] We reestimated the model with a dummy for whether the minimum wage change was "small" and an interaction between this small indicator and the minimum wage. Small increases include years when a minimum wage increase was less than 25 cents or automated by CPI adjustments.

TABLE 2—TOTAL SPENDING RESPONSE TO CHANGE IN THE MINIMUM WAGE: CEX, 1983–2008

				Size of increase[c]			
Share of income from minimum wage jobs (S_i)	Baseline estimates (1)	120–300% of minimum wage[a] (2)	Liquid assets[b] <$5,000 (3)	Small (4)	Large (5)	Real average quarterly spending (6)	Implied marginal propensity to spend using average income[d] (7)
0	−57 (150) 178,075	67 (252) 73,569	77 (174) 77,790	−79 (456)	−55 (150)	10,938	
>0	499 (412) 22,474	−154 (174) 104,506	524 (369) 13,027	−290 (775)	530 (414)	7,640	2.1 (2.0)
≥0.2	815 (457) 15,834	−232 (175) 95,327	885 (404) 9,608	−60 (600)	874 (461)	6,462	3.4 (1.9)

Notes: Each cell represents a separate regression; S_i is the share of pretax total consumer unit income from near minimum wage salaries (<120% of the state minimum wage) earned by the top two adults in the consumer unit. See the text for details. All standard errors are cluster corrected by consumer unit.

[a] S_i is defined as the share of household income coming from workers making 120 to 300 percent of the minimum wage. The sample is all households with $S_i = 0$ in column 1.

[b] Liquid assets are defined as savings plus checking accounts, as in Johnson, Parker, and Souleles (2006).

[c] Small increases include years when a minimum wage increase was less than 25 cents or automated by CPI adjustments.

[d] Marginal propensity to spend is equal to the CEX spending response reported in Table 2, column 1 divided by the income response from Table 1, column 4.

relevant social policies—such as the EITC, welfare/Temporary Assistance for Needy Families, and unemployment insurance described in online Appendix A—that could conceivably be passed in tandem with a minimum wage increase.

Using the estimated spending effect in column 1 and the income estimates from Table 1, we report the marginal propensity to spend (MPS) in column 7. We find that $S_i \geq 0.2$ households spend 3.4 (standard error of 1.9, where standard errors are calculated using the formulas in the online Appendix) times the short-term increase in income that arises from minimum wage hikes. There is no impact among non-minimum wage households.

To help motivate our explanation for the high MPS and to further corroborate this result, we next use the detailed spending breakdown in the CEX and the debt data from the credit bureaus to show the composition, heterogeneity, and timing of spending and debt.

Composition of Spending Responses.—Table 3 displays the estimated durables and nondurables spending responses to minimum wage increases for households where $S_i = 0$, $S_i > 0$, and $S_i \geq 0.2$. We find that the majority of the large spending response reported in Table 2 is from spending on durable goods. For example, households with $S_i \geq 0.2$ increase durables spending by \$875 (\$391) per quarter following a \$1 increase in the minimum wage, an amount that, on average, doubles the typical household's quarterly spending on durables. Again, households with no minimum wage income report no additional durables spending after the minimum wage hike. By contrast, we cannot statistically reject that the impact on nondurables

Table 3—Decomposition of Spending Response: CEX, 1983–2008

Share of income from minimum wage jobs (S_i)	Non durables and services (1)	Durables (2)	Durables subcomponents: Furniture (3)	Floors and windows (4)	Misc. HH items (5)	Appliances and electronics (6)	Leisure activities (7)	Trans-portation (8)
0	21	−78	20	1	−12	11	−2	−97
	(78)	(124)	(18)	(7)	(9)	(14)	(8)	(119)
>0	116	383	9	12	47	37	−24	303
	(158)	(369)	(35)	(10)	(17)	(46)	(38)	(358)
≥0.2	−60	875	0	10	62	35	10	759
	(188)	(391)	(35)	(8)	(18)	(35)	(15)	(386)
Real average amount spent (2005$):								
0	9,120	1,818	164	35	153	275	108	1,083
>0	6,507	1,133	88	15	83	180	68	699
≥0.2	5,573	890	69	9	60	146	53	553
Conditional on purchase (2005$):								
0		1,943	607	340	248	357	172	11,754
>0		1,313	420	198	163	285	129	7,545
≥0.2		1,069	386	152	133	253	112	6,713

Notes: Each cell represents a separate regression. All standard errors are cluster-corrected by consumer unit.

and services is different from 0. The results are particularly striking considering that nondurables and services comprise 85 percent of total spending.

Since most of the spending response is in durables, the rest of the table decomposes this category more finely. In particular, we classify durable goods into six categories: furniture, floors and windows, appliances and electronics, leisure activities, miscellaneous household items, and net outlays on transportation (measured as the difference between the price of the vehicle purchased and the vehicle sold).[18]

For most categories, the impact is small and hard to distinguish from zero. The notable exception is transportation goods. Households with $S_i \geq 0.2$ spend an additional $759 ($386) on transportation durables, representing over 90 percent of the total spending response.

Not surprisingly, a small number of households are responsible for this durables spending. For households with $S_i \geq 0.2$, a fixed effects linear probability model shows that new vehicle purchases rise 2.7 percent (1 percent) per quarter (column 1 of table 4). Column 3 of Table 4 shows that those additional purchases lead to an extra $511 ($212) in quarterly expenditures, on average. There is little impact on used vehicles (columns 2 and 4) or other transportation items (not shown), possibly because they might be harder to debt-finance. Once again, $S_i = 0$ households show no additional spending on vehicles.

[18] Floors and windows include carpets, rugs, curtains, drapes, and blinds. Appliances and electronics include kitchen and laundry appliances, televisions, VCRs, DVDs, stereo and sound equipment, computers, telephones, PDAs, antennas, and satellite dishes. Leisure activities include musical instruments, sports equipment, bikes, camping equipment, toys, games, playground equipment, arts and crafts, CDs, and DVDs. Miscellaneous household items include clocks, lamps, linens, silverware, plates, glasses, decorative items, outdoor equipment, small appliances, smoke alarms, cleaning equipment, tools, lawn equipment, window air conditioners, and portable heaters and coolers. Transportation includes cars, trucks, vans, motorcycles, and boats. These purchases are net of trade-ins.

TABLE 4—DECOMPOSITION OF TRANSPORTATION SPENDING RESPONSE: CEX, 1983–2008

Share of income from minimum wage jobs (S_i)	Probability of purchase (1983–2008): New cars/trucks (1)	Probability of purchase (1983–2008): Used cars/trucks (2)	Expenditure (1983–2008): New cars/trucks (3)	Expenditure (1983–2008): Used cars/trucks (4)	Expenditures on new cars and trucks (1992–2008): Expenditure (5)	Expenditures on new cars and trucks (1992–2008): Net outlay, not financed (6)	Expenditures on new cars and trucks (1992–2008), Financed with loan: Down payment (7)	Expenditures on new cars and trucks (1992–2008), Financed with loan: Expenditure less down payment (8)
0	−0.003	0.006	−37	1	−115	−52	−15	−48
	(0.004)	(0.005)	(92)	(65)	(120)	(63)	(18)	(92)
>0	0.024	−0.005	440	−107	378	80	115	183
	(0.009)	(0.021)	(182)	(196)	(196)	(62)	(63)	(145)
≥0.2	0.027	0.004	511	19	431	45	121	265
	(0.010)	(0.026)	(212)	(204)	(233)	(71)	(75)	(174)
Average (2005$ for expenditures):								
0	0.027	0.058	556	458	554	80	58	416
>0	0.013	0.075	228	423	213	12	24	177
≥0.2	0.009	0.069	153	367	134	6	16	111
Conditional on positive number:								
0			20,643	7,938	22,477	22,468	4,345	19,764
>0			18,021	5,672	19,956	15,456	3,680	17,859
≥0.2			16,996	5,284	18,423	15,392	3,378	16,269

Notes: Probability of a purchase is estimated using a linear probability model with individual fixed effects. Each cell represents a separate regression. All standard errors are cluster-corrected by consumer unit.

Column 5 presents estimates of the spending response over the 1992 to 2008 period, where additional questions were asked about the financing of new vehicle purchases. Column 6 shows that only $45 of the $431 spending response comes from vehicle purchases that were not financed. Of the remaining $386, $121 is an increase in down payments (column 7) and the remainder comes from loans collateralized by the vehicle (column 8). Thus, most of the additional spending on new vehicles is debt-financed.

Distribution of the Spending Responses.—Since an additional 2.7 percent of minimum wage households purchase a new vehicle in the quarters immediately following a minimum wage increase, we would expect that the spending response is concentrated among a minority of households. This pattern is displayed in Figure 1, which graphs a set of quantile regressions of total spending, ranging from the 10th to 98th percentiles (quantiles shown on the x-axis), for households where either $S_i = 0$ (connected by the dashed line) or $S_i \geq 0.2$ (solid line).[19] The key insight is that, for minimum wage households, the mean response is much bigger than the median response, the latter of which is not statistically or economically different from zero. In particular, the average effect reported in earlier tables appears to be substantially driven by households beyond the 90th percentile of the distribution. We would not want to overemphasize these results given their precision. Indeed,

[19] In order to remove the household fixed effect, we first demeaned all variables, and then used standard quantile estimation techniques. Because a quantile estimator is not a linear model, demeaning the data will generate inconsistent estimates. When we performed our procedure on our simulated data, however, we found that this problem is very minor. Since we perform identical procedures on the simulated data, the estimates on actual and simulated data are comparable.

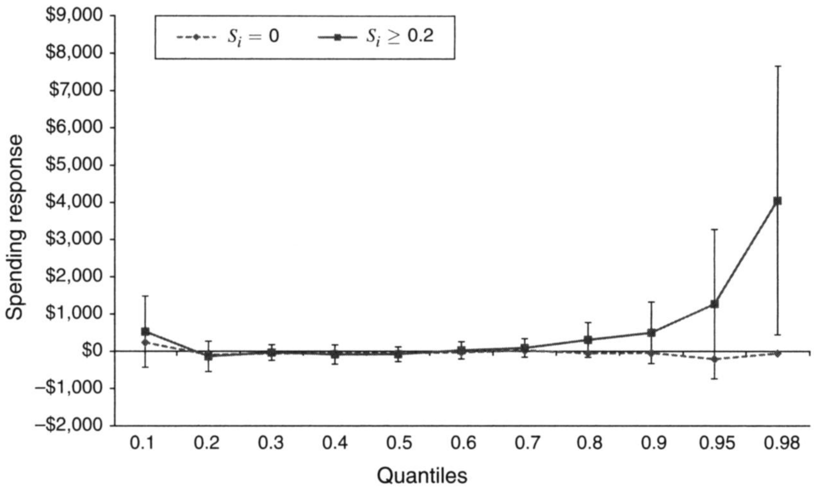

Figure 1. Spending Response to Change in Minimum Wage, CEX Quantile Regressions

90 percent error bands show that the estimates are not statistically distinguishable from zero. But the point estimates are broadly consistent with the heterogeneity in spending responses that we would expect given that average spending is driven by expensive durables purchases.

Timing of Spending.—Figure 2 panels A–D show the timing of the spending response for the $S \geq 0.2$ households. The plots are based on equation (1) where we allow for three quarters of lags and leads of the minimum wage ($K = 3$). The figures highlight three additional key facts.

First, the initial total spending increase (thick line in Figure 2, panel A) happens primarily in the quarter of the minimum wage change. There is little evidence that total spending increases prior to the minimum wage change, even though minimum wage hikes are typically passed into law 6 to 18 months prior to the time of the hike.[20]

Second, while total spending is flat prior to the minimum wage increase, this masks an offsetting increase in nondurables and services (dashed line, Figure 2, panel A) and a decline in durables spending (dotted line, Figure 2, panel A). When the hike occurs (defined as $t = 0$), durables spending spikes up. Though nondurables and service spending increases two quarters before the hike, it does not increase further during the quarter of the hike.

Third, spending does not immediately revert back to prehike levels after the initial increase. Rather, it bounces around $1,000 per quarter in the near term before starting to slowly decline.

For clarity, standard errors are presented in the other panels of Figure 2. Generally, we find that the patterns in nondurables spending (Figure 2, panel C) are not

[20]For example, of the 19 state minimum wage changes between 2000 and 2004 (excluding CPI adjustments), the median time between legislation and enactment date was 9 months. Only two increases (California in 2001 and Rhode Island in 2000) occurred less than five months after the bill's passage. Even among those exceptions, a public legislative debate began well before passage.

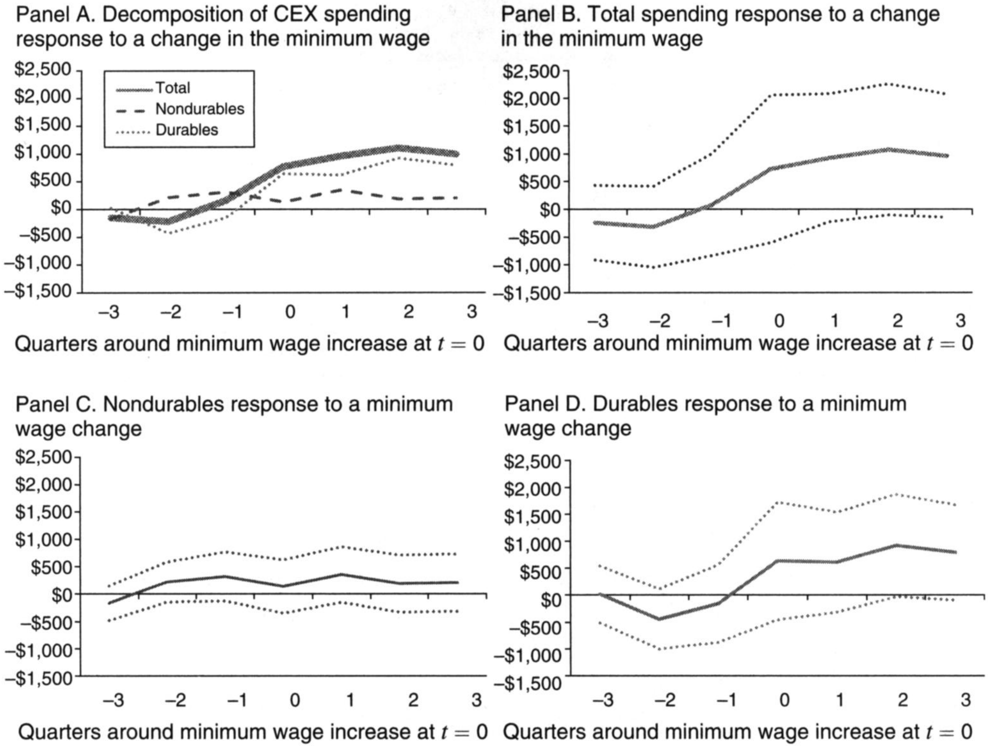

FIGURE 2

Notes: Dashed lines are 90 percent confidence intervals. Sample is $S_i \geq 0.2$. Plots are very similar for $S_i > 0$.

statistically different from zero, which is unsurprising given the nondurables results in Table 3. In contrast, durables spending (Figure 2, panel D) tends to be statistically and economically significant and, as we argue later, broadly consistent with the borrowing constraint model we introduce in Section III.

D. *Debt*

If spending rises more than income after a minimum wage increase, it follows that net financial assets decline. Although we do not have panel data on assets, we have panel data on debt. Table 5 shows quarterly changes in debt, as measured by the credit bureaus, after a minimum wage hike, broken into subcategories: vehicle loans, home equity loans, mortgages, and credit card debt. The results are reported separately for individuals reporting annual income above and below $20,000 at the time of credit card application.[21]

In each category, debt increases after a minimum wage increase, but particularly in collateralized loans tied to vehicles. We estimate that a $1 minimum wage

[21] Recall, we do not have wages for this sample and therefore cannot compute S_i. All observations are weighted based on the estimated relationship, described in Section IIA, between annual earnings and an indicator for whether the hourly wage is at or below 120 percent of the minimum wage.

Table 5—Debt Response to Change in the Minimum Wage Credit Bureau and Credit Card Data, 1995–2008

Income at credit card application	Auto debt (1)	Home equity debt (2)	Mortgage debt (3)	Credit card debt (4)	Total debt (5)	Total minus mortgage debt (6)
≥$20,000	17	10	7	12	47	38
	(99)	(85)	(136)	(7)	(134)	(75)
<$20,000	205	130	155	106	603	440
	(86)	(86)	(371)	(96)	(338)	(148)

Notes: Data on collateralized debt (auto, home equity, and mortgage) are from the credit bureaus. Data on credit card debt is based on cards from our institution. All observations are weighted by P_i, the probability that an individual account holder is a minimum wage worker. See text for details. Sample sizes are 4 million and 582,000 for account holders with incomes of at least $20,000 and incomes less than $20,000, respectively. Each cell represents a separate regression. All standard errors are cluster-corrected by account holder.

increase causes auto loan balances to increase by $205 ($86) per quarter, similar to the increase in debt collateralized by vehicles estimated from the CEX and shown in column 10 of Table 4.[22] Furthermore, home equity lines, which can be used to purchase vehicles,[23] rise by $130 ($86). Auto loans, home equity, and credit card debt combined increase by $440 ($148).[24] There is no increase in debt among higher income (≥ $20,000) individuals.

These numbers are consistent with the income and spending results presented thus far. Assuming that financial assets do not change after a minimum wage hike, rearranging a standard asset accumulation equation (like equation 5 below) shows that spending is equal to the sum of the debt and income responses. Taking the mean income response of $S_i > 0$ and $S_i \geq 0.2$ minimum wage households to be $241 and $238 and the debt response to be $440 (this cannot be estimated by specific levels of S_i), we impute a spending response of $682 and $677, close to what we observe in the CEX, with standard errors of $168 and $168. This result is shown in Table 6, column 2. A weighted average of the imputed and estimated spending effects is $655 ($155) and $694 ($158) for $S_i > 0$ and $S_i \geq 0.2$ households. Such a spending response implies a marginal propensity to spend of roughly three with a *t*-statistic of just over three.[25]

Figure 3 displays the dynamics of household debt (auto, home equity, and credit card) in the nine quarters that follow a minimum wage increase. To provide a longer panel, this figure is based on the sole cohort of accounts that are followed for four years starting in January 2000 rather than the series of two-year panels used in Table 5. The figure clearly shows total debt rising in the first year after a minimum

[22] Likewise, we find that new loans increase by 2.8 percent (with a standard error of 0.8 percent) in the first quarter after a minimum wage increase. Roughly three-quarters are automobile loans and the remainder are home equity loans. Again, these figures are comparable to the estimated increase in automobile purchases in the CEX (column 1 of Table 4).

[23] According to CNW Research, home equity lines were used in 12 to 14 percent of vehicle purchases made between 2003 and 2007. These data were generously provided to us by CNW. They are based on monthly phone and mail interviews of more than 14,000 households.

[24] The estimated credit card debt response of $105 ($95) is based only on our institution. If we use accounts where the balance ratio is high, however, and therefore the individual relies primarily on only our card, the change in debt following a minimum wage increase is similar, albeit less precisely estimated. Our total debt also excludes loans not recorded by the credit bureau, including educational debt.

[25] Standard error derivations are shown in online Appendix B.

Table 6—Alternative Estimates of Spending Response

Share of income from minimum wage jobs (S_i)	Spending			Weighted average marginal propensity to spend[c]
	CEX[a] (1)	Imputed from income/debt[b] (2)	Weighted average (3)	(4)
>0	499	682	655	2.8
	(412)	(168)	(156)	(0.9)
≥0.2	815	677	694	2.9
	(457)	(168)	(158)	(0.9)

[a]From Table 2, column 1.
[b]Table 1, column 4 plus Table 5, column 6. See text.
[c]Column 3 of this table divided by column 4 of Table 1. See online Appendix B for details on the standard error calculations.

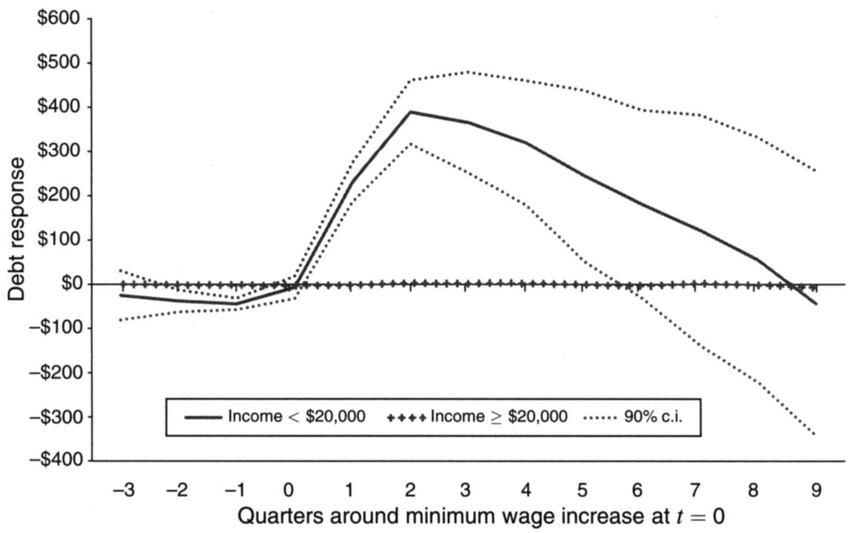

Figure 3. Debt (Auto, Home Equity, and Credit Card) Response to a Change in the Minimum Wage Credit Card/Credit Bureau Data

wage increase for households with income below $20,000 (solid line) but not for higher income households (crossed line). In subsequent quarters, debt rises by less, to the point that by the end of the second year, we cannot reject that debt among low-income households is beginning to fall. This pattern provides direct evidence that much of the early consumption response is in fact debt-financed, and corroborates the independent CEX measures of debt-financed vehicle spending and the large MPS estimates arising from the income and spending regressions.

Finally, Figure 4 plots a set of quantile debt regressions, ranging from 0.10 to 0.98, for households with $< \$20,000$ and $\geq \$20,000$ in income. We again find that the median and mean effects are quite different. The average effect reported in Table 5 is driven by the upper tails of the debt response distribution, consistent with the heterogeneity in spending responses that we would expect given that spending is driven by expensive durables purchases.

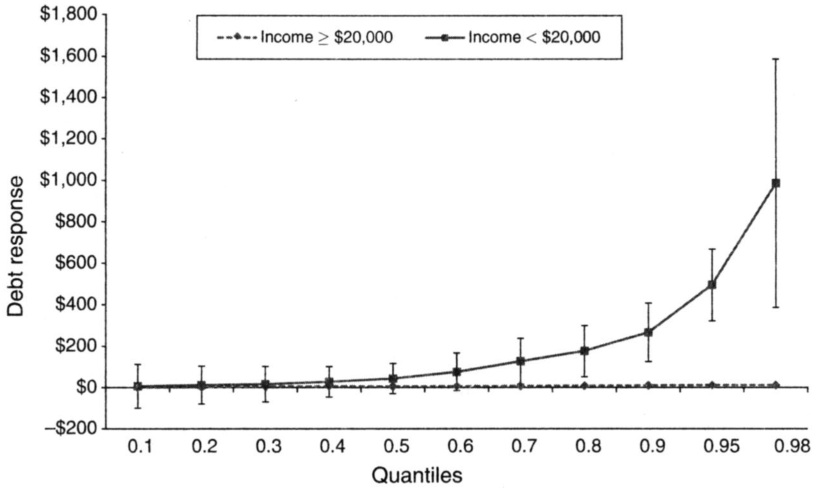

Figure 4. Debt Response to Change in Minimum Wage Credit Bureau Quantile Regressions

Despite the rise in debt, we find little evidence of an increase in defaults in the near term. The probability that an account is 60 days past due actually falls slightly from 5.6 to 5.45 percent (with a standard error of 0.14 percent) six months after a minimum wage increase. This result is again based on a single cohort of credit bureau accounts, but the cohort is large and followed for four years, and the linear probability models include controls for account holder fixed effects and time dummies.

E. *Summary of Empirical Results*

We identify several stylized facts about income, spending, and debt following a minimum wage increase.

First, spending and income increase approximately $700 and $250 per quarter immediately following a minimum wage hike among households that derive income from minimum wage jobs. Consequently, we should see debt rising dramatically, a pattern that we document with the CEX and credit bureau data.

Second, the majority of the spending response occurs in the form of durable goods and, in particular, new vehicles that are debt-financed. Consequently, the spending response is concentrated among a small number of households.

Third, total spending begins to rise within one quarter of a minimum wage increase rather than at the legislation's passage, which typically occurs 6 to 18 months prior. Moreover, there are some compositional differences in the timing. Prior to the minimum wage hike, durables spending falls and nondurables spending rises by roughly equal amounts, so the total spending response is almost zero. After the minimum wage hike, nondurables spending barely increases further, but durables spending immediately spikes upward.

Finally, high levels of durables spending and debt accumulation persist for several quarters after a minimum wage hike.

III. A Model with Durable Goods and Borrowing Limits

In this section, we describe a model that can explain many of these key empirical findings. Define C_t as consumption of nondurable goods at time t and D_t as the durables stock at time t (where time is measured in quarters). The household maximizes

$$E_{t_0} \sum_{t=t_0}^{T} \beta^t (C_t^{1-\theta} D_t^{\theta})^{1-\gamma}/(1-\gamma) \tag{4}$$

subject to the constraints below. Within-period preferences are Cobb-Douglas between durables and nondurables. Thus, consistent with the evidence, expenditure shares are assumed constant.[26] We model individuals for 188 quarters, from age 18 to 65.

The asset accumulation equation is

$$A_{t+1} = (1+r)A_t + Y_t - C_t - I_t, \qquad A_{T+1} \geq 0, \tag{5}$$

where A_t denotes net financial assets (i.e., financial assets less debt), r the interest rate, I_t investment in consumer durables, and Y_t income. The law of motion for durables is

$$D_{t+1} = (1-\delta)D_t + I_t, \tag{6}$$

where δ is the depreciation rate.

In contrast to much of the literature, but often observed in practice, we allow individuals to borrow against durable goods. Assets must satisfy the borrowing constraint

$$-A_t \leq (1-\pi)D_t, \tag{7}$$

where π is the down payment rate, or the fraction of the value of newly purchased durable goods that does not serve as collateral. Such a constraint may exist because of limited enforcement, where collateral guards against the temptation to default (e.g., Kiyotaki and Moore 1997). Rewriting equation (7) shows that "voluntary equity," defined as

$$\text{voluntary equity}_t \equiv A_t + (1-\pi)D_t,$$

must always be greater than 0.

Finally, the income process is

$$\ln Y_t = \alpha_t + P_t + u_t, \tag{8}$$

[26] For example, durables share of expenditures is 17 and 15 percent for CEX households with and without adult minimum wage earners, respectively. Fernandez-Villaverde and Krueger (2011) review the evidence on the substitutability of durables and nondurables and conclude that Cobb-Douglas is consistent with the evidence.

where α_t is the life-cycle profile of income. We assume that $\alpha_t = \alpha_{t_0} + \alpha_1 t$ for the first 80 quarters of an individual's life, and is constant at $\alpha_t = \alpha_{t_0} + \alpha_1 \times 80$ afterward, which is consistent with estimates showing that income growth tapers off after 20 years in the labor force (e.g., Gourinchas and Parker 2002) for low-skill workers. Because we found virtually no change in employment or hours worked following minimum wage hikes, we do not allow for an hours choice.

The stochastic components of income are the white noise term u_t and the AR(1) term P_t

$$P_{t+1} = \rho P_t + \epsilon_{t+1}, \tag{9}$$

where $\epsilon_t \sim N(0, \sigma_\epsilon^2)$ and $u_t \sim N(0, \sigma_u^2)$.

The model is complex and thus we solve it numerically using the solution techniques described in the online Appendix.

A. *Calibration of the Model*

To calibrate the model, parameters are set to the values listed in Table 7. In this section, we highlight those that are less standard.

First, we pick θ to match the CEX's estimate of nonresidential durables' share of total nonresidential expenditure, $I_t/(I_t + C_t)$. Second, for δ, we use the Campbell and Hercowitz (2003) estimate of quarterly depreciation rates for nonresidential durable goods, which is similar to Adda and Cooper (2000). Third, we choose $1 + r = \sqrt[4]{1.03}$ to correspond to a 3 percent real annual rate of interest, a standard in the literature.

Fourth, we assume the down payment rate, π, is 0.4. The Federal Reserve's G19 Consumer Credit release reports that the loan-to-value ratio, $(1 - \pi)$, on new cars averaged 90 percent between 1982 and 2005, covering most of the years in our CEX sample. Only 58 percent of our estimated durables spending response came from new vehicles, however.[27] The rest of durables spending likely requires larger down payments, including some products for which collateralized financing may not be readily available (e.g., small appliances).

Fifth, we choose β to match the share of households that are liquidity-constrained. Using data from the 1989 to 2007 waves of the Survey of Consumer Finances (SCF), the 25th and 50th percentiles of voluntary equity $(A_t + (1 - \pi)D_t)$ at ages 22, 34, and 50 (which are the midpoints of the age tertiles of CEX minimum wage workers) are $-\$70$ and \$452.[28] We choose $\beta = \sqrt[4]{0.93}$, or 0.93 at an annual rate. This value of β minimizes the sum of squared deviations between model-predicted and empirical values of voluntary equity at the 25th and 50th percentiles.

[27] For example, Tables 3 and 4 show that for $S_i \geq 0.2$, the durables response is \$875 and the new vehicle response is \$511.

[28] The 75th percentile of voluntary equity is \$7,563, and thus the 75th percentile of individuals do not appear liquidity constrained. The statistics above were calculated for ages 21, 33, and 49, which is one year before the age of the minimum wage hike. We do the calculation one year before the hike so that the model predictions are unaffected by savings behavior in response to the minimum wage hike. The 25th, 50th, and 75th percentiles of "voluntary equity" for the full SCF at all ages are \$204, \$3,118, and \$12,034, which shows that the distribution is somewhat sensitive to the sample used.

TABLE 7—PARAMETERS USED FOR CALIBRATION

Parameter	Quarterly value	Definition
β	$\sqrt[4]{0.93}$	Discount factor
γ	2	Coefficient of relative risk aversion
θ	0.15	Utility weight on durables
$T - t_0$	188	Number of time periods
r	$\sqrt[4]{1.03} - 1$	Quarterly interest rate
δ	0.034	Durables depreciation rate
π	0.4	Down payment rate
$E(Y)$	\$4,500	Average income of minimum wage households
α_1	0.0108	Income growth
ρ	0.995	Autocorrelation of income
σ_ϵ^2	0.005	Variance of AR(1) innovations
σ_u^2	0.05	Variance of transitory innovations

Lastly, we estimate the parameters of the income process using the SIPP. We estimate $\alpha_1 = 0.0108$ using a household fixed effects regression of log income on age for households with minimum wage workers and heads younger than 40.[29] We choose α_{t_0} such that average income across ages 22, 34, and 50, is \$4,500, roughly the average of all minimum wage households in the SIPP, CEX, and SCF samples.[30] We assume $\rho = 0.995$ (or 0.98 at an annual rate), $\sigma_u^2 = 0.05$, and $\sigma_\epsilon^2 = 0.005$, similar to Gourinchas and Parker (2002), Meghir and Pistaferri (2004), and Kaplan and Violante (2010).

B. *Initial Joint Distribution of the State Variables*

Each simulated individual begins her life with a vector of state variables: the permanent component of income, net financial assets,[31] and the stock of durable goods. We generate the state vector by taking random draws of minimum wage households headed by an individual aged 18 to 25 in the SCF. Online Appendix Table A4 present key descriptive statistics.

C. *Modeling Minimum Wage Hikes*

In order to assess the impact of the minimum wage on spending, we simulate the model with and without a minimum wage hike. The hike is modeled as an innovation to the deterministic component of income, α_t. Given our estimates in Section IIB, we assume that income increases by \$250 immediately following the hike. We assume that the size of income gain does not vary with age. That initial gain is assumed to

[29] This translates into 4 percent average annual income growth, close to estimates for early career low-skill workers (e.g., French, Mazumder, and Taber 2006).

[30] For example, SCF mean income of minimum wage workers is \$4,748 at all ages, and \$4,252 when averaging over ages 21, 33, and 49.

[31] More precisely, the state variable is cash-on-hand, which is the sum of net financial assets and current income.

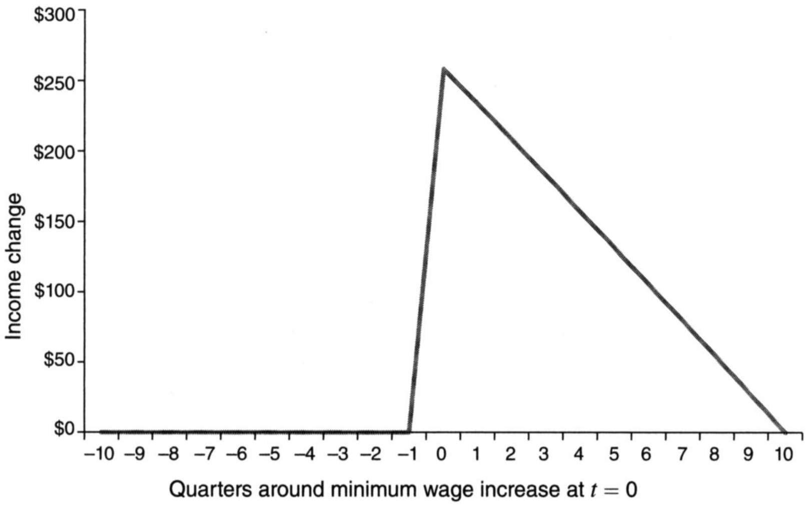

Figure 5. Simulated Income Change around a Minimum Wage Increase

dissipate over the next 10 quarters.[32] After 10 quarters, income once again grows by 1.08 percent per quarter for younger households and 0 percent for older households.

We simulate the model, with and without the minimum wage–induced income gain, at ages 22, 34, and 50. Figure 5 plots the difference in income profiles between simulated individuals who received a minimum wage hike and those who did not, averaged over the ages surrounding the three minimum wage hikes. In total, a 10 percent minimum wage hike increases total discounted lifetime income by just over $1,250.

Finally, we assume that households learn about the minimum wage hike three quarters before it occurs. This is consistent with the observation that minimum wage legislation is typically passed into law at least three quarters before the minimum wage hike is implemented.

D. *Model Results without Uncertainty and Borrowing Constraints*

We first describe the calibration results for the case when households face neither borrowing constraints (so π is unimportant) nor income uncertainty $(\sigma_u^2 = \sigma_\epsilon^2 = 0)$ in order to clarify the dimensions on which this model succeeds in describing the empirical facts. We use the parameters in Table 7,[33] with the exception that the time discount factor β is set to 1.01 to allow the model to generate a more plausible asset

[32] At age 22 this means that rather than grow at 1.08 percent per quarter, income only grows by 0.3 percent in the nine quarters after the hike for households receiving a minimum wage increase. This allows any income gain from the minimum wage to be eroded after 10 quarters.

[33] We continue to make the model predicted mean income $E(Y_t) = \$4,500$ and income jump after a minimum wage hike be \$250. Because $E(Y_t) = \exp(\alpha_{t_0} + (\sigma_{P_t}^2 + \sigma_u^2)/2)$ (where $\sigma_{P_t}^2$ is the variance of the permanent component of income) and earnings variance varies across specifications, we adjust α_{t_0} and how α_t changes after minimum wage hikes across specifications to hold $E(Y_t) = \$4,500$ and the size of the income jump constant across specifications.

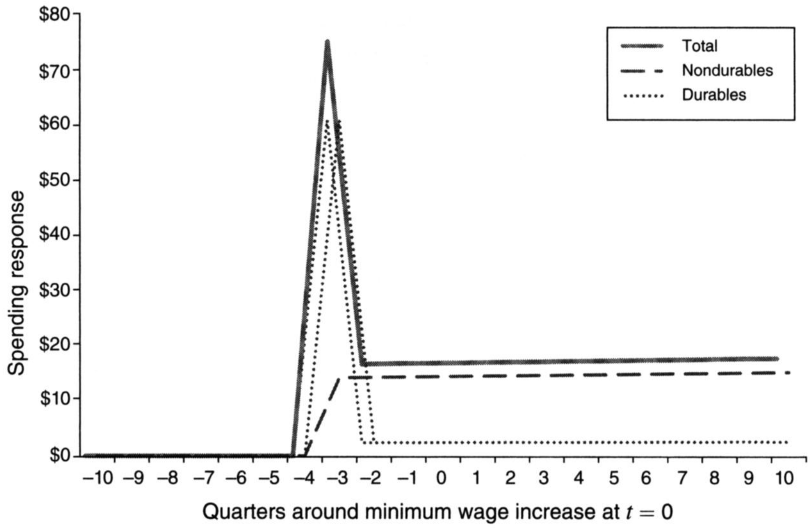

Figure 6. Spending Change around a Minimum Wage Increase Simulation without Borrowing Constraints

distribution. When $\beta = \sqrt[4]{0.93}$, median net financial assets at the time of the minimum wage hike are implausibly low.[34]

Figure 6 shows the predicted spending response to a minimum wage hike (averaged over ages 22, 34, and 50); i.e., the difference between predicted spending of those who received a minimum wage hike and those who did not. Three key features of the figure are worth highlighting.

First, the initial spending increase is \$75, followed by \$17 spending per quarter thereafter. The present value of this stream of spending is roughly \$1,250, the lifetime income gain from the minimum wage hike. These estimates are substantially smaller in the near term than what we observe in the spending data. To better understand the size of the spending responses, we use the parameter values in Table 7 and formulas in the online Appendix to show that if T is large or there is a resale market for durables, the marginal propensity to spend on nondurables and durables is well below 1:

$$(10) \qquad \left.\frac{\partial C_0}{\partial A_0}\right|_{D_0} = (1-\theta)\left[\frac{1-\dfrac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}}{1-\left(\dfrac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}\right)^{T+1}}\right] = 0.01,$$

$$(11) \qquad \left.\frac{\partial I_0}{\partial A_0}\right|_{D_0} = (\beta(1+r))^{\frac{1}{\gamma}}\left(\frac{\theta}{r+\delta}\right)\left[\frac{1-\dfrac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}}{1-\left(\dfrac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}\right)^{T+1}}\right] = 0.04,$$

[34] When $\beta = \sqrt[4]{0.93}$, households are more impatient, and spend more in the short run. For example, the short-run spending response increases from \$75 when $\beta = \sqrt[4]{1.01}$ to \$118 when $\beta = \sqrt[4]{0.93}$.

where θ and $1-\theta$ are the shares of lifetime expenditure devoted to nondurables and durables, respectively. The term $r+\delta$ is a user cost, or the per-period price of durables relative to nondurables, and $\left[\frac{1-\frac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}}{1-\left(\frac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}\right)^{T+1}}\right]$ is an annuitization factor.

Second, the household purchases large quantities of durables and more modest quantities of nondurables upon learning about the minimum wage hike. The reason for the durables increase is that if the household wishes to permanently increase the *service flow* of durables by a small amount, it must increase durables *spending* by a larger amount. After an initial jump, durables spending can decline again as the household only spends to maintain the new higher durables stock (Mankiw 1982).

Third, the spending response occurs when the household learns about a minimum wage hike in quarter -3, not when the hike occurs in quarter 0.

The magnitude, composition, and timing of these predictions are inconsistent with the empirical findings described in Section II.

E. *Model Results with Borrowing Constraints and Income Uncertainty*

Next, we introduce collateral constraints and income uncertainty to the model. Figure 7 plots the spending response to a minimum wage hike that emerges from this model. It illustrates several noteworthy, and ultimately testable, implications.

The first is the sheer magnitude of the spending increase. Total spending increases by over \$300 per quarter in the year after the minimum wage hike. This increase in spending is larger than the gain in income in the first year.

The second finding relates to timing. Spending increases when the minimum wage increases, not when the household learns about the impending hike in quarter -3. Because households are unable to borrow against future income in order to finance current spending, their spending does not rise until the minimum wage increases. Between quarters -1 and 0, the total spending response increases from \$$-89$ to \$468.

The third finding has to do with the composition of spending before and after the minimum wage increase. Prior to its implementation but after its legislative enactment (quarters -3 to -1), total spending is largely unchanged. Nondurables spending rises while durables spending falls. Once the minimum wage increases in quarter 0, however, durables spending soars by \$512 relative to the previous quarter, while nondurables spending continues along a relatively stable path that began at quarter -3. In the face of borrowing constraints, fluctuation in durables spending is optimal because a short-run decline in durables spending has a small effect on the durables stock and its corresponding service flow. Put simply, it is easier to postpone buying a car than food (see Browning and Crossley 2000 for a proof).

That leads us to our final notable result—the persistence of durables spending. The minimum wage hike increases durables spending by \$363, \$227, and \$135 during quarters 0, 1, and 2. The increase in durables spending is still larger than the increase in nondurables two quarters after the minimum wage hike.

One of the striking aspects of this model is that spending exceeds income in the near term. To see the intuition behind this result, and why spending may be

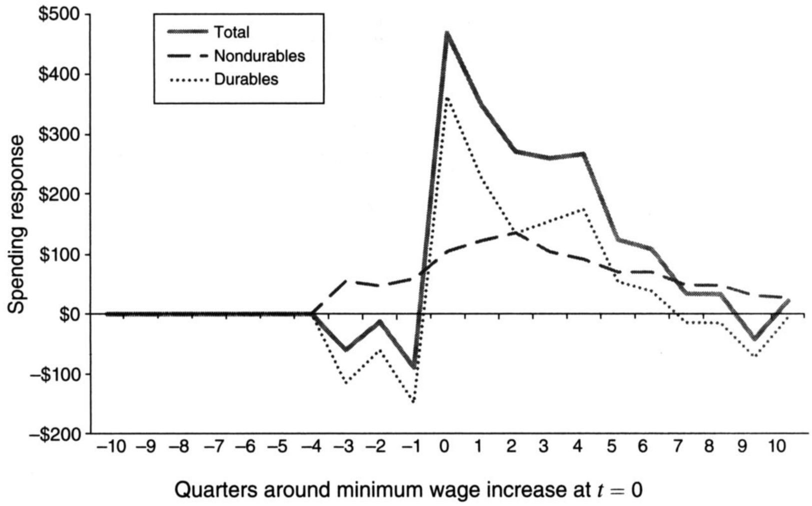

FIGURE 7. SPENDING CHANGE AROUND A MINIMUM WAGE INCREASE
SIMULATION WTH BORROWING CONSTRAINTS

concentrated in durables expenditures, assume that the borrowing constraint (7) always binds; i.e., $A_t = -(1-\pi)D_t$. Combining equation (7) with the asset accumulation equation (5) and the law of motion for durables, equation (6), it can be shown that

$$\pi I_t + C_t + (1-\pi)(r+\delta)D_t = Y_t. \tag{12}$$

Households spend income on durables I_t, nondurables C_t, and interest payments on durables D_t. Since the household only needs $\$\pi$ in income to purchase $1 worth of durables, spending gains can temporarily exceed income gains.

The model with borrowing constraints and income uncertainty better matches the magnitude, timing, composition, and persistence of the CEX spending response than the model without these features. Figure 8, panels A–D plot our estimates (solid lines) against the predictions of the model without borrowing constraints (dotted lines) and with borrowing constraints (dashed lines). Figure 8, panel A displays the response of total spending; Figure 8, panel B nondurables; Figure 8, panel C durables; and Figure 8, panel D debt.[35] The figure emphasizes that the predicted spending response of the model with borrowing constraints is smaller than that estimated in the data, but is much larger than the response predicted by the model with no borrowing constraints. Furthermore, the timing of the model with borrowing constraints matches up well with what is observed in the data.

[35] As above, we assume there is no change in financial assets around minimum wage hikes, so the debt change is $-\Delta A_t$.

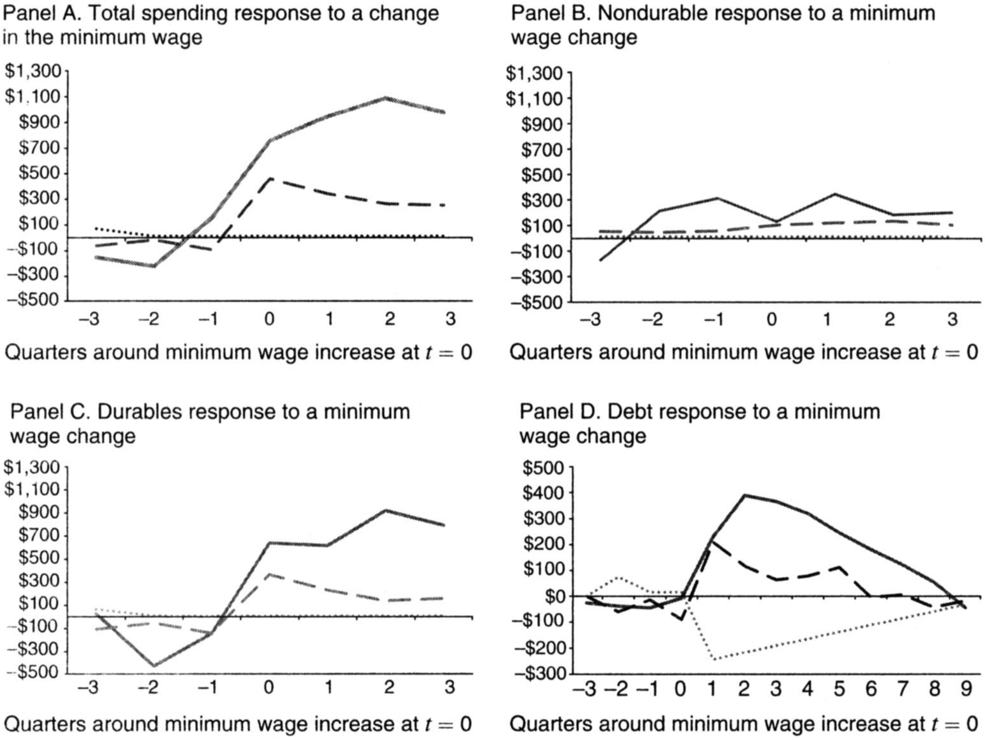

FIGURE 8

Notes: Solid lines are data (see Figures 2 and 3). Dashed and dotted lines are model predictions with and without borrowing constraints. See text.

TABLE 8—ROBUSTNESS CHECKS

Parameters	Nondurables spending	Durables spending	Total spending	25th percentile voluntary equity[c]	Median voluntary equity[c]
Estimates[a]	−60	875	815	−70	452
Baseline[b]	57	411	468	0	73
$\pi = 1.0$	28	193	221	0	47
$\pi = 1.0, \beta = \sqrt[4]{0.95}$	18	196	214	0	106
$\sigma_\epsilon^2 = 0$, $\beta = \sqrt[4]{0.95}$	4	616	620	0	0
$\sigma_\epsilon^2 = 0.002$, $\sigma_u^2 = 0.0, \beta = \sqrt[4]{0.95}$	34	415	449	0	67
Adjustment cost = 0.05	−16	225	209	173	494
Adjustment cost = 0.05, $\beta = \sqrt[4]{0.91}$	−13	213	201	138	280
$\beta = 1.01, \sigma_\epsilon^2 = 0$, no borrowing constraints	3	50	53	NA	NA
$\beta = 1.01, \sigma_\epsilon^2 = 0$, adjustment cost = 0.05, no borrowing constraints	−5	26	21	NA	NA

[a]Spending estimates from Table 3, voluntary equity from online Appendix Table A4.
[b]Baseline parameters shown in Table 7. All parameters are set to baseline values unless otherwise indicated.
[c]Voluntary equity defined as $A_{it} + (1 - \pi)D_{it}$.

F. *Robustness Checks*

Table 8 describes the robustness of our model predictions to changes in down payment rate and the income process. The particular way parameters are adjusted for each of these tests is explained in the first column. The next three columns report nondurables, durables, and total spending responses to minimum wage hikes given the new parameter values. The fifth and sixth columns report the 25th and 50th percentiles of voluntary equity, $A_{it} + (1 - \pi)D_{it}$, which is a measure of how borrowing constrained the agent is.

The first row reviews our estimated spending response from the CEX and the 25th and 50th percentiles of voluntary equity in the SCF. The second row reviews our baseline borrowing constraint model, as described in Section IIIE and Figure 7.[36] Model predicted total spending rises by \$468 in total per quarter after a minimum wage hike.

The next row increases the down payment rate to 100 percent, as in the standard buffer stock model with durable goods. The spending response in this case is \$221 when $\beta = \sqrt[4]{0.93}$, and the response falls slightly to \$214 when we increase β to $\sqrt[4]{0.95}$ to better match the observed distribution of voluntary equity. Higher down payment rates mean fewer durable goods can be purchased with a given level of income. Thus, spending is less sensitive to income when the down payment is higher.

The next two rows explore the sensitivity of the results to differences in the income process. Given that some of the income heterogeneity estimated in Meghir and Pistaferri (2004) or Gourinchas and Parker (2002) may not reflect uncertainty so much as income changes known to individuals, we explore lower levels of income risk than in the benchmark specification.

The spending response is sensitive to the level of income risk. Income risk causes agents to hold precautionary wealth, which in turn affects whether borrowing constraints bind. When borrowing constraints bind, the spending response is larger. For example, when $\sigma_\epsilon^2 = \sigma_u^2 = 0.0$ and $\beta = \sqrt[4]{0.95}$ (no income uncertainty and households are impatient), the key saving motive is removed. Median voluntary equity is \$0. Because agents are borrowing constrained in this scenario, the total spending response rises to \$620 per quarter. Consistent with the empirical evidence, this response is driven almost entirely by durables. That is, we can replicate the estimated spending responses in the data when we reduce the amount of voluntary equity held by minimum wage households. Although this calibration of the model better matches the spending responses than the baseline specification, it produces lower voluntary equity and thus tighter borrowing constraints than what the SCF data suggest. For this reason, we view our baseline specification where not all minimum wage households are borrowing-constrained as more plausible.

When reducing income uncertainty but holding the distribution of voluntary equity fixed, spending responses are similar to the baseline estimates. Eliminating

[36] These are estimated on the simulated data using a household fixed effects regression similar to equation (1). In order to be consistent with the empirical methods and CEX data, we use simulated spending data two quarters before to two quarters after the minimum wage hike. To further match the empirical methodology, we assume the share of minimum wage households that receive minimum wage hikes is similar to that in the data.

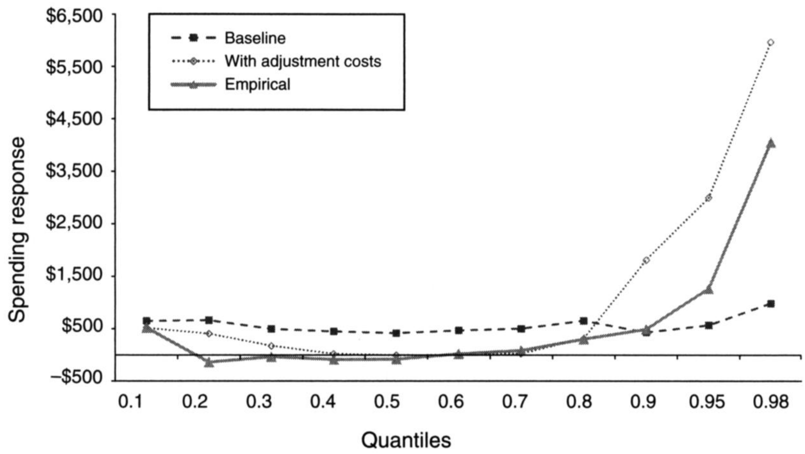

Figure 9. Model Predicted Spending Response to a Change in Minimum Wage with and without Adjustment Costs: Quantile Regressions

the variance of transitory income shocks and reducing the variance of persistent shocks so that $\sigma_\epsilon^2 = 0.002$ and $\sigma_u^2 = 0.0$, but setting $\beta = \sqrt[4]{0.95}$ to keep voluntary equity roughly fixed, leads to a spending response of $449. This is similar to the response from the baseline specification.

The next row reports spending responses when there are adjustment costs, which we discuss in greater detail in Section IIIG. For completeness, the final two rows report spending responses in the model without borrowing constraints, as in Section IIID.[37] As before, spending barely responds under this version of the model.

G. *Adjustment Costs and the Distribution of Spending Responses*

Because much of the spending increase comes from vehicles, there is considerable heterogeneity in spending after a minimum wage increase. Figure 9 compares the estimated distribution of the spending response, as shown in Figure 1 and replotted with the solid green line, to that predicted by our baseline model (the dashed blue line), as well as the baseline model augmented for adjustment costs (the dotted red line). The baseline model predicts roughly the same-sized effect throughout the spending distribution and thus underpredicts the spending response at the right tail relative to what is seen in the data.

Now, consider the possibility that households face a cost of adjusting their durables stock, as in Carroll and Dunn (1997) and Kaboski and Townsend (2011). Households might face transactions costs of adjusting their durables stock if the trade-in value of a used car is less than the price of buying the same car from a used car lot. We follow Grossman and LaRoque (1990) and Eberly (1994) by assuming that in order to increase the durables stock, 5 percent of the previous stock would be lost.[38] Given this assumption, the model predicts that purchases occur every

[37] As in Section IIID, we set $\beta = \sqrt[4]{1.01}$ to generate a plausible wealth level.

[38] See also Attanasio (2000) and Bertola, Guiso, and Pistaferri (2005) for more evidence.

12 quarters, which is similar to the frequency of vehicle expenditures in the CEX. This adjustment cost transforms equation (5) into

$$A_{t+1} = (1 + r)A_t + Y_t - C_t - I_t - 0.05D_t \times I\{I_t \neq 0\}, \tag{13}$$

where $I\{I_t \neq 0\}$ is an indicator for whether the individual purchases or sells a durable good.

When we make this modification, but leave other parameters at the baseline, the average total spending response moves from \$468 to \$209 per quarter (see Table 8) when we hold β at its baseline level and \$201 when we reduce β to $\sqrt[4]{0.91}$ to better match the distribution of voluntary equity. Thus, the model with adjustment costs does worse at explaining large mean spending responses in the data.

That said, adjustment costs, combined with the borrowing constraint, better explain the skewness of spending responses. This is displayed in the red dotted line in Figure 9 for the case where $\beta = \sqrt[4]{0.91}$. The model with adjustment costs displays a significant spike in spending at the right tail of the spending distribution. In particular, for those at the 98th percentile, the spending response is \$5,966 per quarter, larger than the \$4,053 observed in the data.

This higher response comes about because households upgrade their durables stock periodically in the adjustment cost model. Thus, for the majority of households, the durables spending response is zero in any given quarter. Conditional on a minimum wage increase, the probability of a durables purchase, as well as the amount spent conditional on a purchase, rises. This causes the spending response to be very large at the 95th and 98th percentiles but small below that. Consequently, the model with a 5 percent adjustment cost overstates the right tail of the spending distribution, whereas the model without adjustment costs understates it.

IV. Discussion

In this paper, we estimate the magnitude, timing, composition, and distribution of the income, spending, and debt responses to minimum wage hikes among households with adult minimum wage workers. We present four key empirical findings.

First, a \$1 minimum wage hike increases total spending by approximately \$700 per quarter in the near term. This exceeds the roughly \$250 per-quarter increase in family income following a minimum wage hike of similar size. These patterns are corroborated by independent data showing that debt rises substantially after a minimum wage increase. Second, the majority of this additional spending goes toward durable goods, in particular vehicles. Consequently, the spending response is concentrated among a small number of households. Third, total spending increases within one quarter of a minimum wage increase and not prior, despite legislation typically passing 6 to 18 months before enactment. Finally, high levels of durables spending and debt accumulation persist for several quarters after a minimum wage hike.

We find that the model that best matches these facts is an augmented buffer stock model in which households can borrow against part, but not all, of the value of their durable goods. If households face collateral constraints, small income increases can generate small down payments, which in turn can be used for large durable goods purchases. With a 20 percent down payment, each additional dollar of income can be used to purchase $5 of durable goods. Consistent with this model, we find that most of the debt increase following a minimum wage hike is in collateralized debt, such as auto loans. Adjustment costs (representing, say, the trade-in cost of a vehicle) can help to reproduce the fact that the spending response is skewed.

While our model goes a good ways toward explaining the spending patterns in the data, it still falls short. One explanation is that borrowing constraints are more widespread than we assume based on observed asset holdings. Indeed, our model can reproduce the estimated spending responses if we assume near-universal borrowing constraints among minimum wage households.[39] A better understanding of this and other alternative explanations is left for future work.

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[39] Alternatively, our model might miss an important incentive that people face. For example, minimum wage hikes cause the wage, and thus the price of time, to rise. Although we find no evidence that the minimum wage affects adult hours or employment, a higher minimum wage may cause workers to purchase cars so that they can ensure that they hold on to their job. See Gurley and Bruce (2005), who cite evidence on the importance of access to cars on the probability of work among low-income households.

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Seattle's Minimum Wage Experience 2015-16

By Michael Reich, Sylvia Allegretto, and Anna Godoey

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Michael Reich is a Professor at UC Berkeley and Co-Chair of the Center on Wage and Employment Dynamics (CWED) at UC Berkeley's Institute for Research on Labor and Employment. Dr. Sylvia Allegretto is a Research Economist and Co-Chair of CWED. Dr. Anna Godoey is a post-doctoral researcher at CWED. *We acknowledge support from the University of California and the Ford Foundation and we are grateful to Arindrajit Dube and Ben Zipperer for their sage advice.*

ABSTRACT

This brief on Seattle's minimum wage experience represents the first in a series that CWED will be issuing on the effects of the current wave of minimum wage policies—those that range from $12 to $15. Upcoming CWED reports will present similar studies of Chicago, Oakland, San Francisco, San Jose and New York City, among others. The timing of these reports will depend in part upon when quality data become available. We focus here on Seattle because it was one of the early movers.

Seattle implemented the first phase of its minimum wage law on April 1, 2015, raising minimum wages from the statewide $9.47 to $10 or $11, depending upon business size, presence of tipped workers and employer provision of health insurance. The second phase began on January 1, 2016, further raising the minimum to four different levels, ranging from $10.50 to $13, again depending upon employer size, presence of tipped workers and provision of health insurance. The tip credit provision was introduced into a previously no tip credit environment. Any assessment of the impact of Seattle's minimum wage policy is complicated by this complex array of minimum wage rates. This complexity continues in 2017, when the range of the four Seattle minimum wages widened, from $11 to $15, and the state minimum wage increased to $11.

We analyze county and city-level data for 2009 to 2016 on all employees counted in the Quarterly Census of Employment and Wages and use the "synthetic control" method to rigorously identify the causal effects of Seattle's minimum wage policy upon wages and employment. Our study focuses on the Seattle food services industry. This industry is an intense user of minimum wage workers; if wage and employment effects occur, they should be detectable in this industry. We use county level data from other areas in Washington State and the rest of the U.S. to construct a synthetic control group that matches Seattle for a nearly six year period before the minimum wage policy was implemented. Our methods ensure that our synthetic control group meets accepted statistical standards, including not being contaminated by wage spillovers from Seattle. We scale our outcome measures so that they apply to all sectors, not just food services.

Our results show that wages in food services did increase—indicating the policy achieved its goal—and our estimates of the wage increases are in line with the lion's share of results in previous credible minimum wage studies. Wages increased much less among full-service restaurants, indicating that employers made use of the tip credit component of the law. Employment in food service, however, was not affected, even among the limited-service restaurants, many of them franchisees, for whom the policy was most binding. These findings extend our knowledge of minimum wage effects to policies as high as $13.

PART 1 INTRODUCTION

Minimum wage policy in the U.S. has entered a new wave of state and local activity, in response to over a decade of inaction at the federal level. As of June 2017, nine large cities and eight states have enacted minimum wage policies in the $12 to $15 range. San Francisco's minimum wage will increase to $14 on July 1, 2017 and to $15 on July 1, 2018. Seattle's 2017 minimum wage ranges from $11 to $15 and will reach $15 for all employers in 2021. Dozens of smaller cities and counties have also enacted wage standards in this range. These higher standards, which will be gradually phased in, already cover well over 20 percent of the U.S. workforce. And a substantial number of additional cities and states are poised to soon enact similar policies.

These minimum wage levels substantially exceed the previous peak in the federal minimum wages, which reached just under $10 (in today's dollars) in the late 1960s. These new policies will also raise pay substantially for a large share of the workforce—roughly 30 percent in most areas and as much as 40 to 50 percent of the workforce in some jurisdictions. By contrast, individual minimum wage increases in the period 1984-2014 increased pay for less than 10 percent of the workforce.[1]

Although minimum wage effects on employment have been much studied—and debated, this new wave of policy initiatives reaches levels that lie well beyond the reach of previous studies. To better inform public discussion, CWED is studying and will report on the effects of the new wave of minimum wage policies in as close to real time as is possible.

This brief represents the first of a number of reports that CWED plans to issue on this topic. Their timing and coverage will be determined by the phase-in schedules of each jurisdiction and the availability of sufficient post-policy data to make credible assessments. We begin with Seattle because it was one of the first movers in this new wave of minimum wage policies.

We begin by reviewing briefly how economists have studied minimum wage effects. Part 2 describes the Seattle policies; Part 3 describes our methods and findings. Appendix A provides our conceptual framework of how minimum wages affect an economy; Appendix B lists the counties that we use for our comparisons with Seattle.

Background: How economists study minimum wage effects on employment

Ever since George Stigler's pioneering 1946 essay, "The Economics of Minimum Wage Legislation," economists have used the familiar downward-sloping labor demand curve of Econ 101 as the conceptual framework to analyze the expected employment effects of minimum wages. In this framework, a higher wage floor implies that a smaller amount of labor will be demanded. The size of

[1] Nonetheless, $15 is insufficient, anywhere in the U.S., to allow a livable wage for households with children—even when supplemented by safety net programs such as food stamps or the Earned Income Tax Credit.

the disemployment effect depends upon how elastic labor demand is to wages. This elasticity is determined both by the slope of the demand curve and the relevant point on the line, since each point on a given labor demand curve represents a different elasticity. On a given curve, demand elasticities are smaller at lower wages and higher at higher wages. Stigler's framework thus leaves open the possibility that the wage gains of those receiving increases could be greater or smaller than the wage losses of those losing their jobs. Further, Stigler recognized that higher minimum wages could generate positive employment effects when employers possessed some power to set wages. Yet Stigler's analysis provided only a partial analysis based upon the effects of a minimum wage increase in a single industry. A more expanded analysis, which adds the effects of higher minimum wages upon worker purchasing power and consumer demand, finds that minimum wage effects upon employment can be positive or negative.[2]

Given these ambiguities in the theory's predictions, labor economists turned their attention to empirical studies to estimate the actual employment effects of minimum wages. Since the 1990s alone, economists have conducted hundreds of such studies (Bellman and Wolfson 2016). Some find a very small negative employment effect, while others find an effect that is difficult to distinguish from zero.

Almost all of these studies utilize a "difference-in differences" framework that has become standard in empirical economics (Angrist and Pischke 2009). This phrase refers to two sets of differences, each measuring changes in an outcome before and after a policy intervention, but in different areas, one that received the policy treatment and one that did not. The policy intervention in our case is a minimum wage change; the outcomes of interest are actual pay levels and employment among low-wage workers.

A key challenge in these studies is to identify a comparable area—or group—that did not experience the policy. We want to avoid control groups that are influenced by other changes, such as local economic conditions, that might be correlated with but not caused by minimum wage changes. Ideally, we would split the population randomly into two parts—a treatment group that would be given minimum wage increases, and a control group that would not. We could then be assured that differences in the outcomes between these two groups reflected only the causal effects of the treatment.

Of course, randomization is not feasible in the real world of minimum wage policies. Economists have therefore devised different strategies to ensure that our findings reflect causation and not correlation. The outcomes of differing minimum wage studies often vary simply because they use different methods and standards to define their comparison group.

In the past decade, the field of econometrics has made major advances—often known as the "credibility revolution"—that codify the best methodological practices in such studies (Angrist and

[2] We present a revised and expanded conceptual framework for analyzing minimum wages effects in Appendix A.

Pischke 2009). In particular, econometricians emphasize that a treatment and control study should pass three simple but very important tests:

1. The treatment and control groups should behave similarly in the pre-treatment period. This principle is often referred to as the parallel trends assumption. It is important to pass this test to rule out confounding factors that produce a biased causal estimate. The test is stronger when the pre-trend study period is much longer than the period of the post-trend time period.

2. The treatment should have a detectable effect on the treated group but not on the control group. That is, the minimum wage should have increased pay on the treated group by a detectable amount. Otherwise, there should be no expectation of a detectable effect on employment.

3. Groups that did not get a treatment should not exhibit any treatment effects. That is, minimum wages should not have any effects on high-paid groups or on areas that did not experience a minimum wage change. This principle is often examined by administering a "placebo" treatment to the control group.

CWED researchers and affiliates—and others—have reviewed many of the recent studies that obtain negative minimum wage effects. We find that these studies do not conform to one or more of the above three principles. When we deploy methods that do meet these principles—such as by comparing contiguous border county pairs that straddle a state line with a minimum wage difference, we find substantial wage effects but only very small or nonexistent negative employment effects.[3]

Some labor economists nonetheless continue to dispute whether adjoining areas make good comparison groups (Neumark, Salas and Wascher 2014). In response, we and other researchers have used a relatively new method to analyze minimum wage policies, called synthetic controls (Dube and Zipperer 2015; Allegretto, Dube, Reich and Zipperer 2017). This method, when properly deployed, is designed to generate the best control group possible by using an objective data-generated algorithm. We describe further and then use the synthetic control method in Part 3 of this report. Synthetic control methods, when not properly used, may not meet all of the three basic principles above. Under such conditions, they can give misleading results.

[3] See Allegretto, Dube, Reich and Zipper 2017 as well as Zipperer 2016 for examples.

PART 2 SEATTLE'S POLICY TIMETABLE AND COVERAGE

Table 1 displays Seattle's effective minimum wages from 2010 to 2022. We include the years from 2010 on as our study period begins then.

The citywide minimum wage law was enacted on June 20, 2014 and first implemented on April 1, 2015. As Table 1 shows, Seattle adopted a long phase-in policy, with a complex schedule. Two different minimum wages applied in 2015—$10 and $11, depending on size of employer, provision of medical benefits for employees and, for firms with 500 or fewer employees, whether employees receive tips. The law measures employer size using the firm's national employment, not employment just in Seattle, and it defined franchises as part of larger business entities for this purpose. These 2015 rate increases amount to increases of 5.6 percent and 16.2 percent, respectively, from the 2015 state minimum wage of $9.47.

Table 1 Seattle minimum wage timeline

	Large firms (500+)		Small firms (500 or fewer)	
Date	No health insurance	Health insurance	No health insurance, no tips	Health insurance /tips
January 1, 2010[a]	$8.55	$8.55	$8.55	$8.55
January 1, 2011[a]	$8.67	$8.67	$8.67	$8.67
January 1, 2012[a]	$9.04	$9.04	$9.04	$9.04
January 1, 2013[a]	$9.19	$9.19	$9.19	$9.19
January 1, 2014[a]	$9.32	$9.32	$9.32	$9.32
January 1, 2015[a]	$9.47	$9.47	$9.47	$9.47
April 1, 2015[b]	$11.00	$11.00	$11.00	$10.00
January 1, 2016	$13.00	$12.50	$12.00	$10.50
January 1, 2017	$15.00	$13.50	$13.00	$11.00
January 1, 2018	Indexed	$15.00	$14.00	$11.50
January 1, 2019	Indexed	Indexed	$15.00	$12.00
January 1, 2020	Indexed	Indexed	Indexed	$13.50
January 1, 2021	Indexed	Indexed	Indexed	$15.00
January 1, 2022	Indexed	Indexed	Indexed	Indexed

Notes: a.Seattle followed Washington State's minimum wage, which was indexed each year. b.Initiative 1433 went into effect on April 1, 2015. Employers of tipped workers receive a $1 tip credit in 2015 and a $2 tip credit in 2016. After the minimum wage reaches $15, it will be adjusted each year on January 1, based on the CPI for the Seattle-Tacoma-Bremerton Area.

Four different mandated wage standards were introduced on January 1, 2016, varying from $10.50 to $13, again depending upon employer size, provision of medical benefits and, for firms with fewer than 500 employees, whether the employees received tips. These increases ranged from 5 percent to 22 percent. The state minimum wage did not increase in 2016, even though it is indexed each year, as the CPI was unchanged. All Seattle employers will face at least a $15 minimum wage in 2021.

On January 1, 2017, the minimum wage range among Seattle employers became even wider, extending from $11 to $15. Meanwhile, a statewide November 2016 ballot initiative raised the state minimum wage to $11 in 2017, to be increasing further to $13.50 by 2020.

Seattle's complex schedule, which does not appear in other $15 citywide minimum wage ordinances, makes it difficult to compute an average minimum wage effect for each year, as we lack data on how many employees fall under each of the four categories. Our data also do not permit us to discern whether individual employers actually adopted the minimum that applied to them, nor whether employees responded to these differences by moving to employers that had to pay higher minimums.

These are important issues, in part because Seattle's franchise businesses, which employ about six percent of all private sector workers, according to the International Franchise Association (IFA), contested their inclusion in the large employer category. Many of the franchises are limited-service restaurants (think fast food chains) and many of the franchisees own multiple stores. The IFA sued the city, arguing that it was unfair to include these businesses among large employers just because their franchisor employed 500 employees or more throughout the U.S. Despite losing in lower courts, the franchises' minimum wage requirements remained uncertain until May 2016, when the U.S. Supreme Court refused to hear the case (Reuters May 2, 2016).

The Seattle policy instituted an allowable subminimum wage (lower than the regular minimum wage) to be paid to workers who customarily and regularly receive tips—such as wait staff and bartenders. The sub-wage hinges on a tip credit provision—the amount of the wage bill that an employer can pass on to customers in the form of tips. This provision effectively limited the minimum cash wage for restaurant servers to $10 in 2015 and 2016, giving employers a tip credit of $1 in 2015 and $2 in 2016.

This introduction of a tip credit for employers, aka a subminimum wage for tipped workers, into a previously non-tip credit policy environment in Seattle is extremely rare, perhaps unique. Previous research using panel data has shown that cash wages are indeed lower in states with greater tip credits without creating more employment (Allegretto and Nadler 2015). Our data permits us to distinguish differences in wage and employment effects between limited- and full-service restaurants. Since limited-service restaurants by definition rarely employ tipped servers, we may be able to observe the effects of introducing a tip credit on employer-provided pay in Seattle.

PART 3 SYNTHETIC CONTROL ANALYSES

Data and Methods

Data

We use the Bureau of Labor Statistics' Quarterly Census on Employment and Wages (QCEW) administrative data for our analysis. The QCEW tabulates employment and wages of all business establishments that belong to the Unemployment Insurance (UI) system. The UI system covers about 97 percent of all wage and salary civilian employment. We obtained QCEW data from 2009q4 through 2016q1, for all counties in the U.S., from the website of the U.S. Bureau of Labor Statistics. We obtained Seattle city-level QCEW tabulations from Seattle's Office of Economic and Financial Analysis.

The coverage of the QCEW is thus much more complete than household or employer surveys. But like all datasets, it is not perfect. QCEW data can be noisy for areas smaller than a county, insofar as businesses change location or their name. Moreover, some multi-site businesses report payroll and head counts separately for each of their locations, while others consolidate their data and provide information as if their business operated only at a single location. Moreover, the Bureau of Labor Statistics recently began to organize data spatially by geocodes (exact addresses), rather than by zip codes. Postal zip codes do not exactly match city boundaries. In some cities these changes affected both how multi-unit businesses report their results and whether some businesses were located in the city. Our tests find that the statistical noise level in the city-level Seattle QCEW data was very low.

Finally, QCEW data do not include independent contractors, such as Uber and Lyft drivers. The number of such workers has grown in Seattle in recent years, and faster than in other areas of the U.S. (Seattle Minimum Wage Team 2016b). This growth is unrelated to minimum wage policy and thus should not affect our analysis.

Outcomes

Our main outcomes of interest are average weekly wages (reported quarterly) and employment (reported monthly).[4] We construct the average weekly wage variable using the ratio of total industry payroll to employment; it thus reflects both the hourly wage paid to workers and the number of hours worked every week. Employers who react to the minimum wage increase by reducing employee hours will thus impart a negative effect on our wage measure. In the presence of negative effects on hours, our estimated effects on wages represent a lower bound on the true wage effect. However, studies that have hours data (including Seattle Minimum Wage Team 2016a, b), find a very small hours effect.

[4] We obtain the average weekly wage by dividing total payroll by average employment and then dividing by 13 weeks for a quarterly measure. Monthly employment counts only filled jobs, whether full or part-time, temporary or permanent, by place of work on the twelfth of the month.

We focus our analysis on the food service/restaurant industry because it is the most intensive employer of the minimum wage workforce. We examine wages both to determine if there is a treatment effect (which assures us we are analyzing an affected industry) and to quantitatively estimate the increase in worker pay. We report employment and wage outcomes for the major industry category of Food Services and Drinking Places, the combined subsectors of Full Service (FSR) and Limited Service Restaurants (LSR), and separately for the two latter industries.[5]

Wage increases and employment effects in food services are likely to be larger than in other industries, precisely because it has the highest proportion of low-wage workers affected by the minimum wage policy. Therefore, as is standard in minimum wage research, we express our outcome measures as elasticities rather than as absolute changes. Minimum wage elasticities measure the percent change in an outcome, such as actual wages or employment, for a one percent change in the minimum wage. We also report the labor demand elasticity, which is the ratio of the employment elasticity to the wage elasticity. With these scaling, that results from the food services industry are comparable to results for all minimum wage jobs.

Methods

We evaluate the causal effects of minimum wages on wages and employment by using synthetic control estimation. While we can observe wages and employment directly in Seattle, we cannot observe how wages and employment would have evolved if Seattle had not implemented its minimum wage policies. To evaluate the policy empirically, we estimate a counterfactual—what would have happened in a counterfactual or "Synthetic" Seattle, made up of a weighted average of donor counties that did not raise their minimum wage standards. More precisely, the synthetic control method estimates the counterfactual outcomes by constructing an optimally-weighted average of counties in non-treated areas that track pay and employment trends in pre-treatment Seattle.[6] The data-driven nature of this procedure reduces the role of subjective judgment by the researchers in determining the appropriate control region.

We specify a pool of potential donor counties that have similar population size, and which come only from states that, like Washington, index their minimum wages each year, but did not experience any other changes to the minimum wage during the study period. We are thus careful to ensure (unlike Neumark, Salas and Wascher 2014) that our pool of synthetic donor counties is not contaminated by minimum wage increases.

As Appendix B shows, the synthetic control algorithm picks mainly donor counties that are outside Washington State. This result contrasts with previous studies (Dube and Zipperer 2015), which may reflect idiosyncrasies of the Seattle area. In particular, other areas of Washington (outside of King

[5] Food Services and drinking places (NAICS 722), Full Service Restaurants (NAICS 722110 pre-2011, 722511 in 2011+) and Limited Service Restaurants (NAICS 722211 pre-2011, 722513 in 2011+).
[6] A more formal discussion of the synthetic control methods used in these studies will be available in a forthcoming working paper. For insight and intuition regarding this method, see Abadie et al. 2010.

County) are quite dissimilar to Seattle itself. In any case, the large distance between Seattle and the most highly-weighted donors ensures that wage spillovers from Seattle do not contaminate our synthetic control. We are also careful to construct independent synthetic controls for each outcome.

We use as long a period as possible to construct the synthetic control for the time period that runs up close to, but not right at, the minimum wage increase (the "learning" period). We then test to ensure that we can actually obtain a good synthetic Seattle by a) examining the goodness of fit for the outcomes during the learning period and b) testing the goodness of fit for quarters that fall between the learning period and when the treatment is introduced.

We then estimate minimum wage effects by comparing post-treatment outcomes in Seattle with post-treatment outcomes in our Synthetic Seattle. For each outcome, we calculate point estimates as the difference between the outcome in Seattle and Synthetic Seattle, averaged over the post-treatment period and relative to the average outcome in Synthetic Seattle. We then calculate elasticities by scaling the point estimates using the corresponding minimum wage changes.

To assess the statistical significance of these effects, we follow the usual approach in the literature, estimating a series of placebo models for untreated donors. By construction, there have been no changes in minimum wage policies in the donor counties, so any apparent effect on wages or employment are caused by random variation. By looking at the share of donor counties that show apparent wage or employment effects greater than that in Seattle, we obtain an indication of the statistical significance of the estimated effects. For each estimate, we construct the percentile rank statistic as the rank of the estimated treatment effect divided by the number of donors +1. If $p<0.025$ or $p>0.975$, the estimated effect is significant at the 5 percent level.

Key findings

Wage effects

Figure 1 below presents our synthetic control results for the wage effect of the Seattle minimum wage law. Our data begin in 2009q4 and end in 2016q1. The dashed vertical line represents the time of implementation of the first phase of the policy—in April 2015. The second phase began in January 2016. The data have been seasonally corrected using standard procedures.

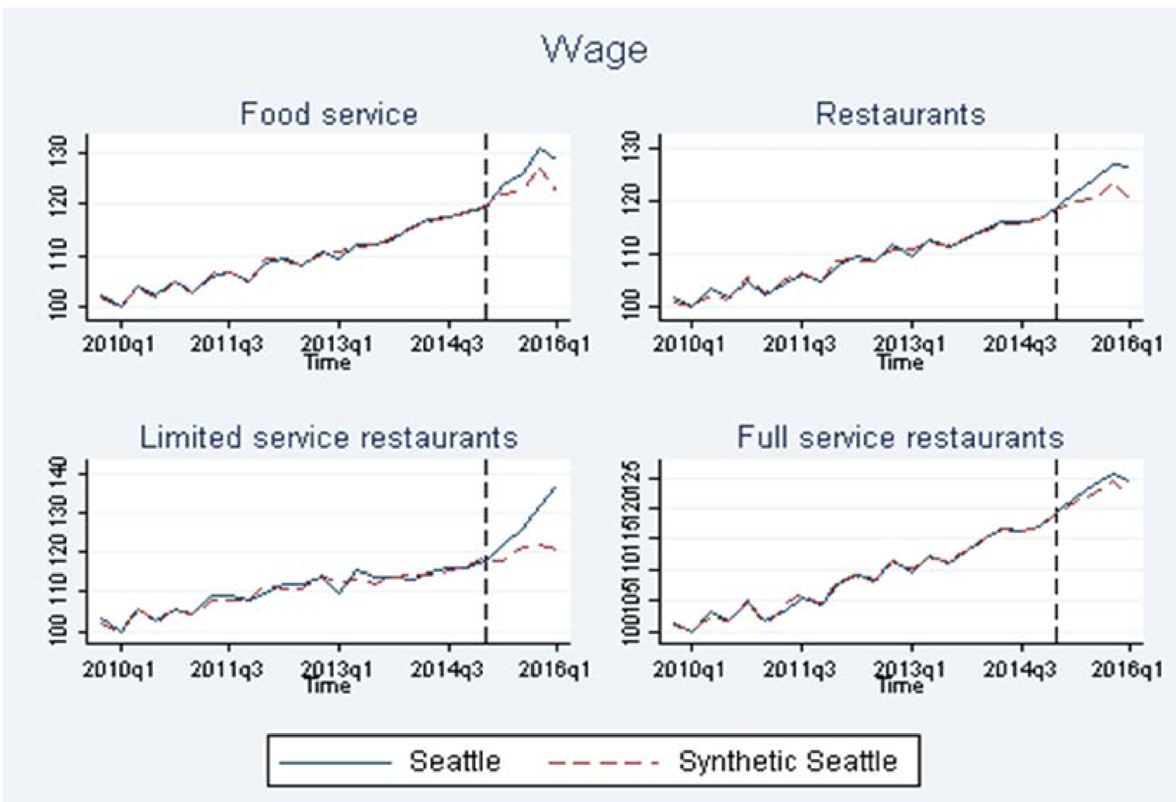
As the figure shows, wages in Synthetic Seattle track wages in Seattle remarkably well, and over the entire pre-treatment period.[7] This finding indicates that our application of the synthetic control method strongly passes the parallel trends requirement. These results thereby satisfy the first of the three credible causal identification conditions we laid out in the beginning of this brief.

[7] The synthetic control method is not appropriate if the researcher cannot obtain close fits in the pre-treatment period. This is often the case. For copious such examples, see Donohue, Aneja and Weber 2017. Researchers who do not display these time paths raise questions about their ability to come up with a synthetic cohort with a good fit.

After the treatment begins, wages in each of the industry groupings increase faster in Seattle than in Synthetic Seattle. This result supports the presence of a wage effect, indicating that the treatment did what it was supposed to do. This finding satisfies the second condition for a credible causal identification.

Importantly, wages increase substantially more in limited service restaurants than in the overall food service industry. And wages in full-service restaurants barely increase relative to Synthetic Seattle. The larger wage increase among limited-service restaurants, many of which are part of franchise chains, suggests widespread compliance with the law, despite the opposition of the International Franchise Association. On the other hand, the very small wage increase among full-service restaurants suggests that these employers made great use of the tipped wage credit.

Figure 1 Wage outcomes, Seattle and Synthetic Seattle

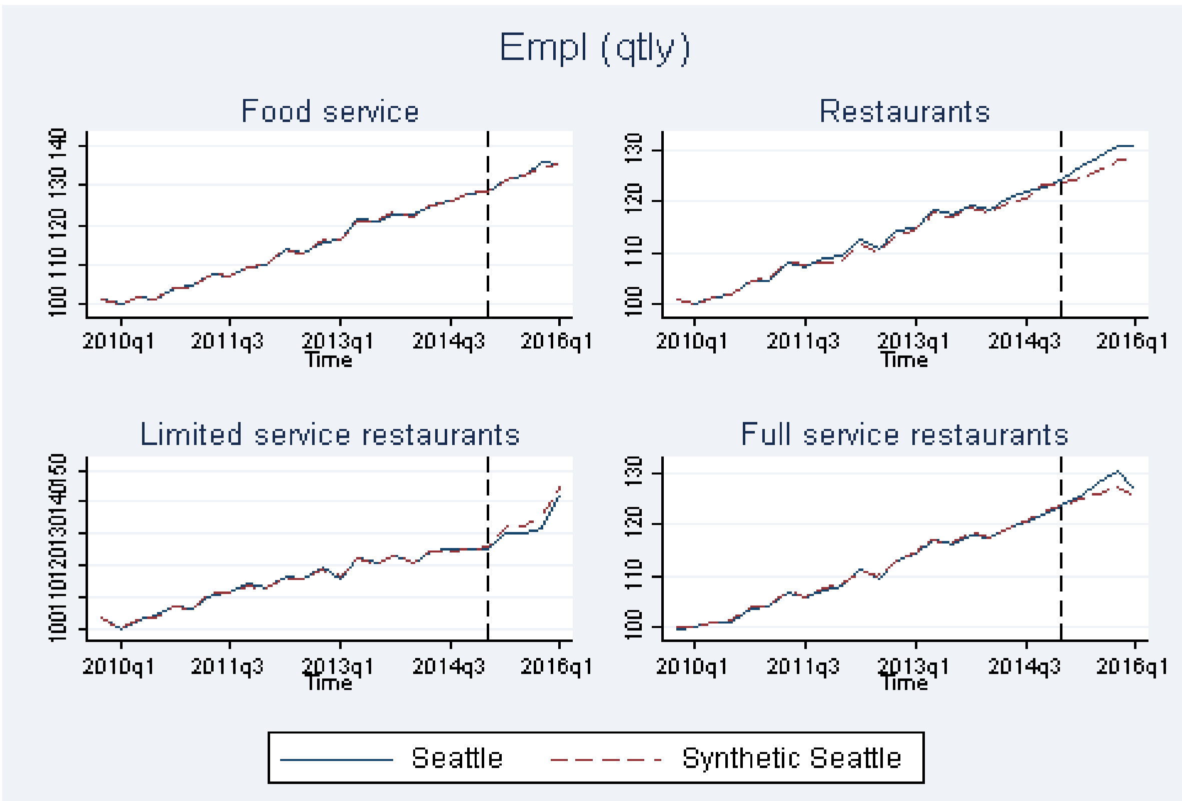


Notes: City-level QCEW data for Seattle. County-level QCEW data for the donors that make up Synthetic Seattle. See Appendix B for a list of donors. The vertical dashed line refers to April 1, 2015, the implementation date of the first phase. The second increase occurred on January 1, 2016.

Employment effects

Figure 2 displays our synthetic control results for employment. Once again, each of the four industry groupings show a close fit between employment in Seattle and employment in Synthetic Seattle over the entire pre-treatment period. Post-treatment employment gains are slightly greater in Seattle than in Synthetic Seattle for all restaurants and among full-service restaurants, and slightly smaller among limited-service restaurants.

Figure 2 Employment trends, Seattle and Synthetic Seattle



Notes: City-level QCEW data for Seattle. County-level QCEW data for the donors that make up Synthetic Seattle. See Appendix B for a list of donors. The vertical dashed line refers to April 1, 2015, the implementation date of the first phase. The second increase occurred on January 1, 2016.

Wage and employment elasticities

Table 2 presents our estimated wage and employment elasticities for each of the four industry groups. The percentile rank statistic in the last column provides a measure of the statistical significance of the estimate. Percentile ranks above .975 and below .025 indicate conventional statistical significance—at the ten percent level. Percentile ranks between these two progressively indicate lower levels of statistical significance.

The estimated wage elasticities in the top panel of Table 2 for food services, all restaurants and limited service restaurants all fall within the range of previous studies and all are highly significant. The wage elasticity of 0.229 for limited service restaurants is nearly identical to our findings in Allegretto et al. (2017). The 0.036 wage elasticity for full-service restaurants is very small and less precisely estimated. These results suggest that full-service restaurants made use of the tip credit to limit the wage increases they would otherwise have paid.

These estimated wage results are subject to a standard caveat. Wages in Seattle may have diverged from Synthetic Seattle just when the minimum wage was implemented for reasons that have little to do with the minimum wage. For example, Seattle's economy may have entered an especially boom period at that time (Tu, Lerman and Gates 2017). We will be able to test this issue by including additional controls in our regressions in future years, as additional quarters of data become available.

The bottom panel of Table 2 displays the employment elasticities. Three of the elasticities are positive, implying a positive effect on employment and one is negative. All are very small and none are precisely estimated, implying that they are not significantly different from zero. All of them are similar to employment elasticities in previous research (such as Allegretto et al. (2017).

Table 2 Estimated wage and employment elasticities

Dependent variable	Industry	Elasticity	Percentile rank statistic
Wage	Food services & drinking places	.098**	.985
	Restaurants (all)	.098**	.984
	Limited service restaurants	.229**	.987
	Full service restaurants	.036	.946
Employment	Food services & drinking places	.010	.538
	Restaurants (all)	.058	.739
	Limited service restaurants	-.060	.333
	Full service restaurants	.045	.704

Notes: Statistical significance levels: ***1 percent, **5 percent, *10 percent. To calculate elasticities, we use the fastest phase-in schedule in Table 1 (employees of large firms who are not covered by employer-sponsored health insurance).

Labor demand elasticities

Although our estimated employment elasticities are not statistically significant from zero, for completeness we present here their equivalents when scaled as labor demand elasticities. Estimated labor demand elasticities in low-wage labor markets in other studies generally center on -0.3. Should they be any different for Seattle? The industries most affected by minimum wages provide local services (in economists' terms, they are not tradeables). Moreover, Seattle is large enough that most of the consumption by Seattle residents occurs within the city's boundaries.

We compute labor demand elasticities for each of our four industry groupings by taking the ratio of the employment elasticity to the wage elasticity, using the results in Table 2. The labor demand elasticities are 0.102 for food services and drinking places, 0.592 for all restaurants, -0.262 for limited-service restaurants, and 1.25 for full-service restaurants. These results vary in part because our estimated wage increases vary by industry and in part because our employment effects vary by industry. However, we do not place much weight on these results as they are measured very imprecisely.

Placebo tests

We turn next to examining how our donor counties, which did not receive the minimum wage treatment, respond when they are given a "placebo" minimum wage treatment. The synthetic control algorithm conducts this test separately for each donor county.[8] Recall that the purpose of these tests is to validate the statistical significance of the results reported in Figures 1 and 2 and Table 2.

Figure 3 displays the placebo results with thin gray lines, one for each donor county. (The vertical lines in Figure 3 are located one quarter after the first minimum wage implementation; we will correct this in a future version.) The gray lines trace the difference between the outcomes of interest for each donor, relative to its "synthetic area." Since these donor counties did not actually receive a minimum wage treatment, we expect considerable random variation in the large post-treatment outcomes. If the post-treatment individual gray lines diverge considerably from each other, we are observing random variation—the absence of a treatment effect.

Figure 3 also displays the results for Seattle (using the thicker orange line), relative to Synthetic Seattle. The orange lines that lie well within the envelope of the numerous gray lines indicate that the orange line could just reflect random variation. If an orange line hugs or reaches outside the envelope

[8] The starting point for these placebo graphs consists of all the potential donors with data available for all periods for the industry subcategory. The potential donors were counties in states that indexed minimum wages but had no other minimum wage events. We estimated two versions: (1) ranking the Seattle result relative to all potential donors; (2) ranking the Seattle results against donors with a "good" pre- intervention fit (RMSPE<2 times that of Seattle). This second criterion excludes potential donors for whom we were unable to construct a good-fitting synthetic control. The placebo graphs illustrate the second approach. Although the second approach excludes some potential donors, potentially reducing significance levels, the actual significance levels are not materially different.

of gray lines, we have additional support that the Seattle results reflect a statistically significant treatment.

In the upper panel of Figure 3, the gray lines diverge during the placebo treatment period, consistent with random variation and no observed treatment effect. For all food services and for all restaurants, this panel also shows a substantial difference between the Seattle results (the thick orange line) and the set of individual donor placebo results (the thin gray lines), indicating that the wage effect is not likely the result of random variation. These results satisfy the three basic principles articulated by the credibility revolution in econometrics.

The upper panel of Figure 3 shows a particularly large and significant effect on wages in limited-service restaurants (note the compression of the vertical axis in this industry's figure). This result is consistent with lower initial pay in limited-service restaurants than in the rest of the industry and with substantial compliance among fast-food restaurants, whether franchises or company-owned.[9] The orange line in the full-service sector is not so steep, indicating smaller and statistically insignificant pay increases, consistent with the results in Table 2. These results are also consistent with the establishment of a tip credit for employers in this industry.

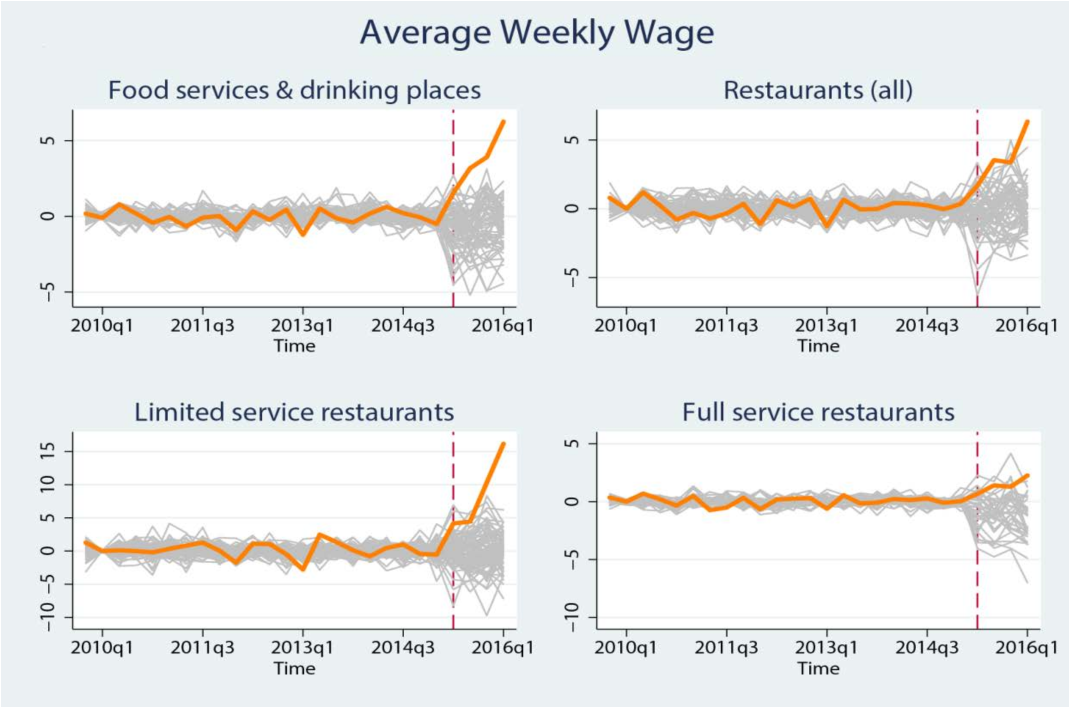
The lower panel of Figure 3 displays the equivalent results for the employment outcomes. Again, the placebo test lines diverge considerably in the post-placebo treatment period, indicating the absence of a treatment effect on employment when there was no treatment. The thick orange line now falls within the enveloped of individual gray lines for food services and for all restaurants.

The orange line is closer to the bottom envelope of the placebo results for limited-service restaurants in the first treatment phase and then bounces back in the second phase.[10] In both periods, it remains within the envelope, indicating that the observed outcome could reflect random variation. The orange line for full-service restaurant employment rises within the top of the placebo envelope in the first phase and bounces back toward zero in the second phase. These results confirm the finding in Table 2: the employment effects in limited- and full-service restaurants are not statistically different from zero.

[9] Ji and Weil (2015) find that franchised outlets of fast food restaurants exhibit much lower compliance rates with minimum wages than do company-owned outlets.

[10] This effect looks larger than it is because the vertical axis is elongated, relative to the other outcomes.

Figure 3 Placebo graphs for wages and employment



Note: The vertical dashed line in this Figure refers to one quarter after the implementation of the first phase. The vertical axis in the limited services figure is elongated relative to those in the other three figures, exaggerating the actual deviations from zero. Placebos where RMSPE<2 times that of Seattle are reported.

SUMMARY

The evidence collected here suggests that minimum wages in Seattle up to $13 per hour raised wages for low-paid workers without causing disemployment. Each ten percent minimum wage increase in Seattle raised pay by nearly one percent in food services overall and by 2.3 percent in limited-service restaurants. The pay increase in full-serve restaurants was much smaller and not statistically significant, consistent in part with higher pay in full-service restaurants and the establishment of a tip credit policy. Employment effects in food services, in restaurants, in limited-service restaurants and in full-service restaurants were not statistically distinguishable from zero. These results are all consistent with previous studies that credibly examine the causal effects of minimum wages.

These findings of no significant disemployment effect of minimum wages up to $13 significantly extend the minimum wage range studied in the previous literature. Of course, unobserved factors, such as Seattle's hot labor market compared to that in Synthetic Seattle (Tu, Lerman and Gates 2017), may have positively affected Seattle's low-wage employment during this period. We will monitor this possibility as the city's $15 policy continues to phase in. And Seattle makes up just one case study; examination of a wider set of cities may lead to different conclusions. Our future reports will throw further light on this possibility.

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APPENDIX A

Why minimum wage increases produce little to no employment effects

CWED researchers and other labor economists have challenged the Stigler downwardly-sloping labor demand framework and developed an alternative framework that considers how minimum wages affect an entire economy (Reich, Allegretto and Montialoux 2017). We refer to this alternative framework as the CWED minimum wage model. It contains five components:

1. Building upon Stigler's insight that employers may possess some wage-setting power, we recognize that employers can choose whether to set low wages and experience high turnover costs or set higher wages and face lower turnover costs. This formulation follows modern search theories of the labor market. Wage rates are indeed inversely related to employee turnover rates, often exceeding 100 percent per year in low-wage industries. Wage-setting power in low-wage labor markets then becomes the norm and not the exception (as Stigler had expected). Our previous empirical work confirms that raising minimum wages does significantly reduce the high rate of employee turnover in low-wage industries (Dube, Lester and Reich 2016). We estimate that the reduced costs of recruiting and retaining workers absorb about 15 percent of the increased payroll costs.

2. Raising wages directly increases worker productivity somewhat, even in low-skilled jobs. A recent study by Burda, Genadek and Hamermesh (2016) confirms this relationship. Increased productivity may arise directly because workers are more experienced or motivated or more likely to receive employer-based training.

3. Higher minimum wages can lead to increased substitution of technology for labor. However, the magnitude of this effect is smaller than is commonly recognized—especially in low-paid service occupations that remain difficult to routinize, such as restaurant food preparation, childcare and eldercare, driving emergency vehicles and janitorial work. Technology has transformed more routinized work mainly because the cost of technology has fallen so sharply, while wages have remained stagnant.

4. Higher costs due to minimum wages will be passed on in higher prices and reduce the scale of output, thereby reducing labor demand. This effect is also much smaller than is usually recognized, for five reasons. First, some workers in affected industries are already well-paid and will not get increases. Second, the pay of workers getting increases does not bunch entirely at the old minimum wage—it ranges across the entire range to just above the new minimum wage. As a result, actual wage increases are about 20-25 percent of the statutory increase. Third, labor consists of only about 30 percent of operating costs in the affected industries. Fourth, prices increases are limited to the industries that most employ minimum wage workers. Fifth, consumer demand in these industries is relatively inelastic to changes in

prices, so the effect on sales and on demand for workers is even smaller than the effects on prices.

5. Minimum wage increases raise take-home pay primarily among workers who have high propensities to spend on consumer goods. This increased consumption increases the demand for labor in the entire consumer goods sector. When larger numbers of workers will get pay increases, the magnitude of this effect grows in relative importance to the others above.

Each of these components affects employment, some in a negative direction and others in a positive direction. Adding them together generates the net effect on employment. Our CWED team has used parameters from various literatures and the Implan Input-Output model to calibrate our model. We have already estimated the model for $15 minimum wage policies in New York State, California, San Jose and Fresno County. We have in progress a study of the effects of a federal $15 policy on the U.S. and on Mississippi. All of these enacted or proposed policies would phase in over five to seven years. $15 in 2024 is the equivalent of $12.50 to $13 today.

These studies all suggest that a $15 minimum wage policy would substantially raise pay for millions of workers and their families with only negligible net effects on employment. Of course, much bigger increases, such a $50 minimum wage, would not have the same effects and indeed would require building an entirely different model.

APPENDIX B: DONOR COUNTIES AND WEIGHTS

Appendix Table B1: Wages

Food service	Boulder County, Colorado	.537
	Pickaway County, Ohio	.105
	Charlotte County, Florida	.100
	Carroll County, Ohio	.062
	Coconino County, Arizona	.061
	Clear Creek County, Colorado	.041
	Park County, Colorado	.031
	St. Louis County, Missouri	.023
	Lafayette County, Missouri	.016
	Pend Oreille County, Washington	.008
	Larimer County, Colorado	.007
	Trumbull County, Ohio	.006
	Stevens County, Washington	.004
Restaurants	Larimer County, Colorado	.310
	Kitsap County, Washington	.157
	Missoula County, Montana	.132
	Charlotte County, Florida	.128
	St. Johns County, Florida	.071
	Medina County, Ohio	.061
	Trumbull County, Ohio	.056
	Union County, Ohio	.036
	Jefferson County, Colorado	.025
	Sarasota County, Florida	.024
Limited service	Walla Walla County, Washington	.165
	Jefferson County, Colorado	.165
	Stevens County, Washington	.147
	Union County, Ohio	.125
	Cochise County, Arizona	.094
	Douglas County, Colorado	.073
	Missoula County, Montana	.066
	Delaware County, Ohio	.059
	Benton County, Washington	.055
	Charlotte County, Florida	.025
	Chelan County, Washington	.024
	Clay County, Florida	.002
Full service restaurants	Skagit County, Washington	.276
	Platte County, Missouri	.147
	Spokane County, Washington	.133
	Yavapai County, Arizona	.119
	Larimer County, Colorado	.100
	Pinal County, Arizona	.080
	Whatcom County, Washington	.051
	Portage County, Ohio	.037
	Lafayette County, Missouri	.020
	Teller County, Colorado	.011
	Santa Rosa County, Florida	.010
	Cass County, Missouri	.008
	Park County, Colorado	.008

Appendix Table B2: Employment

Food service	Lee County, Florida	.257
	Delaware County, Ohio	.143
	Nassau County, Florida	.081
	Denver County, Colorado	.075
	Jefferson County, Ohio	.074
	Flagler County, Florida	.069
	El Paso County, Colorado	.060
	Osceola County, Florida	.059
	Walla Walla County, Washington	.033
	Allen County, Ohio	.032
	Newton County, Missouri	.032
	Carbon County, Montana	.029
	Collier County, Florida	.029
	Buchanan County, Missouri	.017
	Highlands County, Florida	.006
	DeKalb County, Missouri	.003
	Park County, Colorado	.001
Restaurants	Lee County, Florida	.225
	Lorain County, Ohio	.193
	Newton County, Missouri	.148
	Platte County, Missouri	.109
	Jasper County, Missouri	.079
	Brevard County, Florida	.076
	Carbon County, Montana	.051
	Gulf County, Florida	.020
	Hernando County, Florida	.020
	Asotin County, Washington	.015
	Lafayette County, Missouri	.013
	Gadsden County, Florida	.012
	Teller County, Colorado	.010
	Sumter County, Florida	.009
	Park County, Colorado	.009
	Cochise County, Arizona	.006
	Clear Creek County, Colorado	.002
	Carroll County, Ohio	.002
	Pickaway County, Ohio	.001
Limited service	Pinal County, Arizona	.295
	Jasper County, Missouri	.161
	Bay County, Florida	.088
	Polk County, Florida	.058
	Sumter County, Florida	.052
	Snohomish County, Washington	.046
	Fulton County, Ohio	.044
	Santa Rosa County, Florida	.043
	Walton County, Florida	.04
	Geauga County, Ohio	.038
	Flagler County, Florida	.024
	St. Johns County, Florida	.023
	Citrus County, Florida	.021
	Collier County, Florida	.013
	Asotin County, Washington	.013
	Franklin County, Washington	.011

	Charlotte County, Florida	.011
	Brevard County, Florida	.011
	Yavapai County, Arizona	.008
Full service restaurants	Denver County, Colorado	.156
	Lee County, Florida	.133
	Allen County, Ohio	.110

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ABSTRACT

This paper evaluates the wage, employment, and hours effects of the first and second phase-in of the Seattle Minimum Wage Ordinance, which raised the minimum wage from $9.47 to as much as $11 per hour in 2015 and to as much as $13 per hour in 2016. Using a variety of methods to analyze employment in all sectors paying below a specified real hourly rate, we conclude that the second wage increase to $13 reduced hours worked in low-wage jobs by around 9 percent, while hourly wages in such jobs increased by around 3 percent. Consequently, total payroll fell for such jobs, implying that the minimum wage ordinance lowered low-wage employees' earnings by an average of $125 per month in 2016. Evidence attributes more modest effects to the first wage increase. We estimate an effect of zero when analyzing employment in the restaurant industry at all wage levels, comparable to many prior studies.

Ekaterina Jardim
Daniel J. Evans School of Public Policy
and Governance
University of Washington
Box 353055
Seattle, WA 98195
erosh@uw.edu

Mark C. Long
Daniel J. Evans School of Public Policy
and Governance
University of Washington
Box 353055
Seattle, WA 98195
marklong@uw.edu

Robert Plotnick
Daniel J. Evans School of Public Policy
and Governance
University of Washington
Box 353055
Seattle, WA 98195
plotnick@uw.edu

Emma van Inwegen
Daniel J. Evans School of Public Policy
and Governance
University of Washington
Box 353055
Seattle, WA 98195
emmavani@uw.edu

Jacob Vigdor
Daniel J. Evans School of Public Policy
and Governance
University of Washington
Box 353055
Seattle, WA 98195
and NBER
jvigdor@uw.edu

Hilary Wething
Daniel J. Evans School of Public Policy
and Governance
University of Washington
Box 353055
Seattle, WA 98195
hwething@uw.edu

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Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence from Seattle

1. Introduction

Economic theory suggests that binding price floor policies, including minimum wages, should lead to a disequilibrium marked by excess supply and diminished demand. Previous empirical studies have questioned the extent to which this prediction holds in the labor market, with many estimates suggesting a negligible impact of higher minimum wages on employment. This paper, using rich administrative data on employment, earnings and hours in Washington state, re-examines this prediction in the context of Seattle's minimum wage increases from \$9.47 to as much as \$11 per hour in April 2015 and as much as \$13 per hour in January 2016. It reaches a markedly different conclusion: employment losses associated with Seattle's mandated wage increases are in fact large enough to have resulted in net reductions in payroll expenses – and total employee earnings – in the city's low-wage job market. The contrast between this conclusion and previous literature can be explained largely, if not entirely, by data limitations that we are able to circumvent. Most importantly, much of the literature examines the impact of minimum wage policies in datasets that do not actually reveal wages, and thus can neither focus precisely on low-wage employment nor examine impacts of policies on wages themselves.

Theory drastically oversimplifies the low-skilled labor market, often supposing that all participants possess homogeneous skill levels generating equivalent productivity on the job. In reality, minimum wages might be binding for the least-skilled, least-productive workers, but not for more experienced workers at the same firm. Empirically, it becomes challenging to identify the relevant market for which the prediction of reduced employment should apply, particularly when data do not permit direct observation of wages. Previous literature, discussed below, has typically defined the relevant market by focusing on lower-wage industries, such as the restaurant sector, or on lower-productivity employees such as teenagers.

This paper examines the impact of a minimum wage increase for employment across *all* categories of low-wage employees, spanning *all* industries and worker demographics. We do so by utilizing data collected for purposes of administering unemployment insurance by Washington's Employment Security Department (ESD). Washington is one of four states that collect quarterly hours data in addition to earnings, enabling the computation of realized hourly wages for the entire workforce. As we have the capacity to replicate earlier studies' focus on the restaurant industry, we can examine the extent to which use of a proxy variable for low-wage status, rather than actual low-wage jobs, biases effect estimates.

We further examine the impact of other methodological choices on our estimates. Prior studies have typically drawn "control" cases from geographic regions immediately adjoining the "treatment" region. This could yield biased effect estimates to the extent that control regions alter wages in response to the policy change in the treatment region. Indeed, in our analysis simple geographic difference-in-differences estimators fail a simple falsification test. We report results from synthetic control and interactive fixed effects methods that fare better on this test. We can also compare estimated employment effects to estimated wage effects, more accurately pinpointing the elasticity of employment with regard to wage increases occasioned by a rising price floor.

Our analysis of restaurant employment at all wage levels, analogous to many prior studies, yields minimum wage employment impact estimates near zero. Estimated employment effects are higher when examining only low-wage jobs in the restaurant industry, and when examining total hours worked rather than employee headcount. Even when analyzing low-wage employment across all sectors, employment elasticities as conventionally calculated lie within the range established in prior literature, if somewhat on the high side.

Our analysis reveals a major limitation of conventional elasticity computation methods, however. When comparing percent changes in employment to percent changes in wage, conventional methods must arrive at the percent change in wage by assumption rather than estimation, in some cases assuming that the percent change in wage equals the percent change in the statutory minimum. This is often a necessity, as analysis is performed using datasets that do not permit the estimation of policy impacts on wages themselves. We show that the impact of Seattle's minimum wage increase on wage levels is *much smaller* than the statutory increase, reflecting the fact that most affected low-wage workers were already earning more than the statutory minimum at baseline. Our estimates imply, then, that elasticities calculated using the statutory wage increase as a denominator are *substantially* underestimated. Our preferred estimates suggest that the rise from $9.47 to $11 produced disemployment effects that approximately offset wage effects, with elasticity point estimates around -1. The subsequent increase to as much as $13 yielded more substantial disemployment effects, with net elasticity point estimates closer to -3.[1]

While these findings imply that Seattle's minimum wage policy served to decrease total payroll expenses on low-wage employees, and by extension those employees' earnings, several caveats are in order. These estimates pertain to a minimum wage increase from what had been the nation's highest state minimum wage to an even higher level, and might not indicate the effects of more modest changes from lower initial levels. In fact, our finding of larger impacts of the rise from $11 to $13 per hour than the rise from $9.47 to $11 per hour suggests non-linearity in the response. Second, our data do not capture earnings in the informal sector, or by contractors, and minimum wage policies could conceivably lead employers and workers to shift towards these labor market arrangements. Some employers may have shifted jobs out of Seattle but kept them within the metropolitan area, in which case the job losses in Seattle overstate losses in the local labor market. Even without mobility responses by firms, reductions in payroll per employee may significantly exceed reductions in worker income to the extent that workers were able to find alternate employment in Seattle's rapidly growing suburbs.

Our analysis focuses on a subset of Washington State employers, those that definitively report workplace location for each of their employees. Because of this restriction, smaller single-site employers are over-represented in our sample; we include 89% of all business entities employing 63% of Washington's workforce. We discuss the ramifications of this restriction extensively below. While there may be concerns that larger businesses might exhibit significantly different responses to the minimum wage, survey evidence indicates no differential response and tracking workers longitudinally we find no evidence of an exodus of workers from the sector included in our analysis to the excluded sector.

Finally, the mechanisms activated by a local minimum wage ordinance might differ from those associated with a state or federal increase. It is reasonable to expect that policies implemented at a broader geographic scale offer fewer opportunities to reallocate employment in response.

[1]Because we calculate elasticity by taking the ratio of the estimated effect on employment to estimated effect on hourly wages, these estimates are imprecise. For instance, the 95% confidence intervals for the elasticities associated with a $13 minimum wage range from -5.9 to -0.3.

We emphasize that any analysis of the welfare implications of a minimum wage increase must consider how income gains and losses distribute across the low-wage workforce. Some low-wage workers are household heads responsible for maintaining a family's standard of living. Others are secondary or tertiary earners whose income is less necessary for basic survival. Our study does not address which workers are better or worse off as a consequence of the minimum wage ordinance. Future analysis will combine employment records with other administrative data from Washington State to more fully address critical distributional questions.

2. Challenges in estimating the impact of minimum wage increases

Traditional competitive models of the labor market suggest that an increase in a binding minimum wage will cause reductions in employment. Any number of modifications to the standard model can raise doubts about this prediction. These include the presence of monopsony power (Bhaskar and To, 1999), the possibility that higher wages intensify job search and thus improve employee-employer match quality (Flinn, 2006), "efficiency wage" models that endogenize worker productivity (Rebitzer and Taylor, 1995), and the possibility that some low-wage workers exhibit symptoms of a "backward-bending" supply curve associated with a need to earn a subsistence income (Dessing, 2002). Even in the absence of these theoretical modifications, there has long been debate regarding the empirical magnitude of the theorized effect.

Over the course of the past 25 years, a robust literature has developed with researchers using a variety of strategies to estimate the effect of minimum wages on employment and other outcomes. While this literature has often generated significant debate over econometric specifications and data sources, the heavy reliance on proxies for low-wage employment in the absence of actual wage data has figured less prominently.[2]

2.1 What is the relevant labor market?

[2] One notable exception is the work of Belman and Wolfson (2015). They note: "Focusing on low-wage/low-income groups offers the advantage of providing more focused estimates of the effect of changes in minimum wage policies; employment and wage effects are less likely to be difficult to detect due to the inclusion of individuals unlikely to be affected by the minimum wage. Use of proxies for low wage/low income such as age, gender, and education are a step in this direction, but still potentially dilute the impact by the inclusion of unaffected individuals (p. 608)."

Previous literature has not examined the entire low-wage labor market but has focused instead on lower-wage industries such as the restaurant sector, or on stereotypically lower-productivity employees such as teenagers. Studies of the restaurant industry harken back to Card and Krueger (1994), which utilized a case study approach to estimate the employment effects of New Jersey's increase in its state minimum wage. The authors argue that fast-food restaurants are not just a leading employer of low-wage workers, but also display high rates of compliance with minimum-wage regulations. Many authors have subsequently chosen the restaurant and fast food industry to study federal and state level minimum wages (Addison, Blackburn and Cotti, 2012, 2014; Dube, Lester and Reich, 2010; Dube, Lester and Reich, 2016; Neumark, Salas and Wascher, 2014; Totty, 2015; Allegretto, Dube, Reich, and Zipperer 2016). Other authors have focused on retail (Kim and Taylor, 1995; Addison, Blackburn and Cotti, 2008).

Another strand of studies estimates the effect of minimum wages on teenagers. These studies argue that teenagers are typically at the bottom of the wage and earnings distribution and make up a large share of the low-wage workforce. Studies of minimum wage effects on teenagers have occurred at the federal and state level (Card, 1992; Allegretto, Dube, and Reich, 2011; Neumark and Wascher, 1994, 1995, 2004, 2008, 2011; Neumark, Salas, and Wascher, 2014).

Using restaurant or retail employees or teenagers as proxies for the entire low-wage labor market might lead to biased minimum wage effects. Intuitively, a sample mixing jobs directly affected by the minimum wage with others for which the price floor is irrelevant would generally skew estimated impacts towards zero. Isolating one industry, such as the fast food industry, may lead to downwardly biased wage and employment effects due to heterogeneity in wages in the industry (i.e., some workers whose wages are above the minimum wage will be misclassified as belonging to the "treatment" group). The estimates capture the minimum wage's net effects on all restaurant employees, not the effects on low-wage employees, which would likely be stronger. Similarly, using teenagers may lead to artificially large employment estimates as this group omits other low-wage workers, particularly those that have a stronger attachment to the labor force and are full-time full-year workers, for whom the wage-elasticity of demand may be smaller. On the other hand, since some teens earn wages well above the minimum, including them in the sample would lead to artificially low estimates of the impacts for that demographic group.

This discussion begs the question of what, exactly, should count as a low-wage job. An intuitive approach – and the one pursued in this analysis – focuses on jobs that pay below a certain (inflation-adjusted) hourly wage.[3] Analysis of employment at or below a specified wage threshold may overstate disemployment effects to the extent that minimum wage policy may cause some employers to raise wages of workers from below to above the threshold. A more purist approach would focus on jobs that entail any of a variety of tasks for which there are no specialized skill requirements, which any able-bodied person might perform. Practically, few if any employment datasets contain such information.

In theory, analysis of employment at or below a specific real wage level will be unproblematic if the wage distribution can be effectively partitioned into a component affected by minimum wage policy and an unaffected counterpart. Imagining a reaction function relating pre-policy to post-policy wages, the partition would be associated with a fixed point. It is not clear that any such fixed point exists. Our analyses below are informed by efforts to estimate reaction functions, which reveal little evidence of significant responses to the minimum wage above relatively low thresholds. We also report the results of sensitivity analyses that vary the threshold substantially.

2.2 Debates over methodology

While much of the previous literature has elided the difficult problem of identifying the relevant labor market by using simple industry or demographic proxies, there has been no shortage of debate over causal estimation strategy. The traditional approach uses variation in state-based minimum wages and estimates minimum wage-employment elasticities using a two-way fixed effect OLS regression (Neumark and Wascher, 2008). This approach assumes parallel pre-trends across treatment and control states and estimates the overall impact of minimum wages on wage and employment of multiple minimum wages over time. The two-way fixed effect approach has come under criticism in recent years because there are spatial patterns in minimum wage adoption (Allegretto, Dube, Lester and Reich, 2016). States with higher minimum wages are concentrated in the Northeast and West coast, regions that have different

[3] This approach bears a strong resemblance to Cengiz et al., (2017) who use pooled Current Population Survey data to study the impact of state-level minimum wage increases on employment at wages just above and below the newly imposed minimum between 1979 and 2016.

employment patterns from states in the South and parts of the Midwest. If this underlying regional pattern affects state employment trends differentially, then the parallel trends assumption of the two-way fixed effects model does not hold. Subsequently, difference-in-differences estimation strategies, which weight all states without a higher minimum wage equally as their control region, may negatively bias employment elasticity estimations.

To account for this issue, researchers have argued for a variety of specifications. These include: the use of local area controls, such as division-period fixed effects or a border discontinuity approach, (Allegretto, Dube and Reich, 2011; Dube, Lester and Reich, 2010; 2016; Allegretto, Dube, Lester, Reich, 2016), the use and order of region-specific time trends (Addison, Blackburn, Cotti, 2012, 2014), the use of a synthetic control to identify control regions with pre-trend employment levels similar to the treatment region (Neumark, Salas, and Wascher; 2014), and linear factor estimation (Totty, 2015).[4]

Local area control designs assume that neighboring counties or states within a census division region are more similar in trends and levels than regions further away. Researchers using local-area controls (Dube, Lester and Reich 2010, 2016; Allegretto, Dube, Reich, 2011) show strong and significant earnings elasticity estimates but insignificant employment elasticities near zero. While it is reasonable to think that nearby regions share many background characteristics with the treated region, a local area control design will yield biased estimates when policies have spillover effects in nearby areas, such as when businesses raise wages in response to a wage increase in a nearby jurisdiction.

The notion that nearby regions offer the best match on background characteristics is itself a matter of debate. Using a synthetic matching estimator approach, Neumark, Salas, and Wascher (2014) show that local areas are not picked as donors in the synthetic estimator of panel national data, and thus should not be used as the control region. Allegetto, Dube, Lester and Reich (2016) rebut this claim noting a recent paper found statistically significant larger mean absolute differences in covariates not related to the minimum wage for noncontiguous counties compared to contiguous counties (Dube, Lester and Reich, 2016).[5]

[4] In this study we do not replicate region-specific time trends due to the limited time-frame of our treatment group. However, this specification has become popular; see Dube, Lester and Reich (2010, 2016) and Addison, Blackburn and Cotti (2014) for use of linear and polynomial time trends in minimum wage estimation strategies.

[5] Covariates included log of overall private sector employment, log population, private-sector employment-to-population ratio, log of average private sector earnings, overall turnover rate and teen share of population.

A final strand of estimation has used linear factor estimation and interactive fixed effects, which relaxes the assumption of parallel trends in control and treatment regions by explicitly modelling unobserved regional trends. Totty (2015) utilizes Pesaran's (2006) common correlated effects estimators as a linear factor estimation. Pesaran's common correlated effects estimators do not estimate common factor and common factor loadings, like the interactive fixed effects estimator, but rather use cross-sectional averages of the dependent and independent variables as a proxy for factors. Totty also uses an interactive fixed effects estimator, identical to ours, which involves estimating the common factors and factor loadings across space and over time and finds insignificant and null employment effects of minimum wages.

3. Policy Context

In June 2014, the City of Seattle passed a minimum wage ordinance, which gradually increases the minimum wage within Seattle City boundaries to $15 an hour. The phase-in rate differs by employer size, and offers some differentiation for employers who pay tips or health benefits. The minimum wage rose from the state's $9.47 minimum to as high as $11 on April 1, 2015. The second phase-in period started on January 1, 2016, when the minimum wage reached $13 for large employers (see Table 1 for details). In this paper, we study the first and second phase-in periods of the Seattle Minimum Wage Ordinance (hereafter, the Ordinance) during which the minimum wage rose from $9.47 to $13 for large businesses – a 37.3% increase.[6] This ordinance, which at the time would have raised Seattle's minimum wage to the highest in the country, came toward the beginning of a wave of state and local minimum wage laws passed in 2012-2016.[7, 8]

[6] As of 2016, employers with fewer than 501 employees worldwide that provide health benefits or pay tips could pay a minimum wage of $10.50 if they contribute at least $1.50 towards tips and health benefits. Our data do not allow us to observe if a worker gets health benefits, but we do observe total compensation, which includes tips. We come back to this issue in greater detail when we discuss the data.

[7] Most prior research has, by necessity, focused on increases at the federal (Card 1992, Katz and Krueger 1992, Belman and Wolfson 2010) or state (Dube, Lester, Reich 2010; 2016, Card and Krueger 1994, Neumark and Wascher 1995, Meer and West 2016) level. This ordinance provides an opportunity to study the minimum wage on a smaller geographic area with an integrated labor market that could allow businesses and workers flexibility to relocate. Prior research on local minimum wage changes (Dube, Naidu, Reich 2007, Potter 2006, Schmitt and Rosnick 2011) have found small or no employment effects of the local wage policies, results consistent with the bulk of the minimum wage literature.

[8] During the years we study (2005 to 2016), the State of Washington had a state-specific minimum wage that was indexed to CPI-W (growing at an average annual rate of 2%) and was, on average, 30% higher than the federal Minimum Wage. As a result, none of the increases in federal minimum wage over this time period have been binding in Washington.

For most of the phase-in period, the minimum wage ordinance mandates higher wages for larger businesses, defined as those with more than 500 employees worldwide. For purposes of the ordinance, a franchised business – independently owned, but operated under contract with a parent company and reflecting the parent company brand – are considered large businesses so long as the sum of employment at all franchises worldwide exceeds 500.

Seattle's groundbreaking minimum wage was implemented in the context of a robust local economic boom. As the figures in Table 3 below indicate, overall employment expanded rapidly in Seattle over the two years following the ordinance's passage. Our methods will endeavor to separate this background trend from the impact of the ordinance itself.

4. Data

4.1 Basic description

We study the impact of the 2015 and 2016 minimum wage increases in Seattle using administrative employment data from Washington State covering the period 2005 through the third quarter of 2016. Washington's Employment Security Department collects quarterly payroll records for all workers who received wages in Washington and are covered by Unemployment Insurance (UI).[9,] Employers are required to report actual hours worked for employees whose hours are tracked (i.e. hourly workers), and report either actual hours worked or total number of hours, assuming a 40 hour work week for employees whose hours are not tracked (i.e. salaried workers).[10, 11]

[9] Most studies that analyze employment responses to minimum wage hikes in the US rely on data from the Quarterly Census of Employment and Wages, which in turn relies on information from the same data source as we do – payroll data on jobs covered by the UI program. As a result, our estimates will be comparable to many results in the literature.

[10] The Employment Security Department collects this information because eligibility for unemployment benefits in Washington is determined in part by an hours worked test. Comparison of the distribution of hours worked in the ESD data with the distribution of self-reported hours worked in the past week among Washington respondents to the CPS reveals some points of departure. In particular, self-reported data show more pronounced "spikes" at even numbers such as 40 hours per week. In general, given the statutory reporting requirement driven by benefits determination provisions, ESD considers the hours data reliable.

[11] Minnesota, Oregon, and Rhode Island are the other three states that collect data on hours.

This unique dataset allows us to measure the average hourly wage paid to each worker in each quarter by dividing total quarterly earnings by quarterly hours worked.[12, 13, 14] As such, we can identify jobs more likely affected by an increase in the minimum wage, and track trends in both employment counts and calculated average hourly wages.[15] Unlike the prior literature, we can plausibly identify low-wage jobs across industries and in all demographic groups, obviating the need for proxies based on those factors. As a result, we can estimate effects solely for low-wage jobs within all industries.

The ESD data contain industry (NAICS) codes, which permit us to estimate results using the restaurant industry proxy used in much of the prior literature (Addison, Blackburn and Cotti, 2012, 2014; Dube, Lester and Reich, 2010; Dube, Lester and Reich, 2016; Neumark, Salas and Wascher, 2014; Totty, 2015; Allegretto, Dube, Lester and Reich, 2016).[16]

We measure employment both as the number of jobs (headcount) and the number of hours worked during the quarter. Because the data provide information on all jobs that were on payroll during a quarter, including jobs which lasted only for a few weeks or even days, we follow prior studies in focusing on the number of beginning-of-quarter jobs, defined as a person-employer match which existed both in the current and previous quarter.[17] The hours worked measure includes all employment, regardless of whether a person-employer match persists for more than one quarter. Because the hours measure captures shifts in staffing on both the intensive and extensive margins, we focus on it in our preferred specifications.

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[12] We convert nominal quarterly earnings into real quarterly earnings by dividing by the Consumer Price Index for Urban Wage Earners and Clerical Workers (CPI-W). All wage rates and earnings should thus be considered to be in 2nd quarter of 2015 dollars.

[13] The average wage may differ from the actual wage rate for workers who earn overtime pay, or have other forms of nonlinear compensation including commissions or tips. Workers may occasionally be paid in one quarter for work performed in another. In analysis below, we exclude observations with calculated wages below $9 or above $500 in 2015 dollars. We also exclude observations reporting under 10 or over 1,000 hours worked in a calendar quarter. These restrictions exclude 6.7% of all job/quarter observations.

[14] ESD requires employers to include all forms of monetary compensation paid to a worker, including tips, bonuses and severance payments. As such, for tipped employees we will observe total hourly compensation after adding tips, as long as employers have reported tipped income in full. Because of this data feature, appropriate minimum wage schedule for tipped workers employed by small businesses should include tip credit.

[15] The average hourly wage construct used here is not directly comparable to, say, the self-reported hourly wage in the CPS – in which respondents are instructed to exclude overtime, commissions, or tips. Results obtained through analysis of this average hourly wage measure may differ from those gleaned from self-reported wage studies to the extent that employers alter the use of overtime, tips, or commissions in response to the wage increase.

[16] Specifically, we examine employment and wages in the 3-digit NAICS code 722 "Food and Drinking Places".

[17] This definition is used by the Quarterly Workforce Indicators, based on the Longitudinal Employer Household Data (LEHD), and produces the total number of jobs comparable to the employment counts in the Quarterly Census of Employment and Wages.

The ESD data exclude jobs not covered by the UI program, such as contract employment generating IRS 1099 forms instead of W-2s, or jobs in the informal economy paid with cash. Our estimates may overstate actual reductions in employment opportunities if employers respond to the minimum wage by shifting some jobs under the table or outsourcing workers on payroll to contractor positions.

4.2 Limitation to geographically locatable employment

The data identify business entities as UI account holders. Firms with multiple locations have the option of establishing a separate account for each location, or a common account. Geographic identification in the data is at the account level. As such, we can uniquely identify business location only for single-site firms and those multi-site firms opting for separate accounts by location.[18, 19] We therefore exclude multi-site single-account businesses from the analysis, referring henceforth to the remaining firms as "locatable" businesses. As shown in Table 2, in Washington State as a whole, locatable businesses comprise 89% of firms, employ 62% of the entire workforce (which includes 2.7 million employees in an average quarter), and 63% of all employees paid under $19 per hour.[20]

Multi-site single-account or "non-locatable" firms may respond differently to local minimum wage laws for several reasons. These larger employers may be more likely to face higher mandated minimum wages under the Seattle ordinance. It is not possible to precisely determine which employers are subject to the large business phase-in schedule, as Washington data identify global employment only for those firms with no operations outside the state, do not identify which entities have operations outside the state, and do not indicate whether a business operates under a franchise agreement let alone the number of employees at all same-branded

[18] To determine the exact location of each business, we geocode mailing addresses to exact latitude and longitude coordinates. We then use these data to determine if a business is located within Seattle, and to place businesses into Public Use Microdata Areas within Washington State. A small number of employers use a post office box as a mailing address or have not reported a valid address; these are excluded from the analysis.

[19] Note that our analysis sample includes both independently-owned businesses and franchises where the owner owns a single location, but excludes corporations and restaurant and retail chains which own their branches and franchises whose owner owns multiple locations, unless these entities opt to establish separate UI accounts by location.

[20] Appendix Table 1 shows that the proportion of low-paid (under $19 per hour) employees included in the analysis falls close to the 63% benchmark in the accommodation and food service industry and the health care and social assistance industry. It exceeds the benchmark in manufacturing, educational services, and arts, entertainment and recreation. It falls short of the benchmark in the retail industry.

franchises. While it is reasonable to assume that multi-site employers are more likely to be large and thus subject to the higher wage mandate, it is by no means a perfect indicator.[21]

If it were a perfect indicator, basic economic theory suggests that excluded businesses should reduce employment faster than included businesses, as they face a higher mandated wage increase. Individual employees may exhibit some incentive to switch into employment at an excluded firm, but these job changes will be tempered by any adverse impact on labor demand.

This basic prediction could be tempered to the extent that excluded businesses exhibit a different labor demand elasticity relative to included businesses. On the one hand, firms with establishments inside and outside of the affected jurisdiction might more easily absorb the added labor costs from their affected locations, implying a less elastic response to a local wage mandate. On the other hand, such firms might have an easier time relocating work to their existing sites outside of the affected jurisdiction, implying a greater elasticity.

Survey evidence collected in Seattle at the time of the first minimum wage increase, and again one year later, suggests that multi-location firms were in fact more likely to plan and implement staff reductions.[22] Moreover, the ESD data can be used to track workers longitudinally, to check whether minimum wage increases are associated with an increased flow of workers from locatable jobs to non-locatable jobs. If the minimum wage ordinance were to cause an expansion of labor demand in the non-locatable sector, we might expect increased worker flows into this sector. As Figure 1 illustrates, we find that the rate of transition from locatable to non-locatable employment – tracking individual workers from one year to the next – shows no significant change in either Seattle or nearby regions as the city's minimum wage increased, suggesting no impact of the ordinance on gross flows into the non-locatable sector.[23]

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[21] In addition, larger firms are more likely to provide health benefits to their workers, and Seattle's minimum wage ordinance establishes a lower minimum wage for employers who contribute towards health benefits.

[22] The Seattle Minimum Wage Study conducted a stratified random-sample survey of over 500 Seattle business owners immediately before and a year after the Ordinance went into effect. In April 2015, multi-site employers were more likely to report intentions to reduce hours of their minimum wage employees (34% versus 24%) and more likely to report intentions to reduce employment (33% versus 26%). A one-year follow-up survey revealed that multi-location employers were more likely to report an actual reduction in full-time and part-time employees, with over half of multi-site respondents reporting a reduction in full-time employment (52%, against 45% for single-site firms). See Romich et al. (2017) for details on employer survey methodology.

[23] The basic impression conveyed by this figure is confirmed by synthetic control regression analysis, which finds no significant impact of the minimum wage ordinance on the probability that a low-wage individual employed at a locatable Seattle business in a baseline quarter is employed in the non-locatable sector anywhere in Washington State one year later.

Our best inference, in summary, is that our data restriction to geographically locatable employment likely biases our employment results towards zero.

4.3 Basic plots of the hourly wage distribution

Figure 2 shows the distribution of quarterly hours worked across one-dollar-wide wage bins, up to the \$39-40 per hour level, in the 2nd quarter of 2014, when the minimum wage ordinance was passed, compared to the 2nd quarter of 2015, the quarter when \$11 per hour minimum wage was implemented, and the 2nd quarter of 2016, one quarter after implementation of the \$13 per hour minimum wage. After both minimum wage step-ups, we see strong declines in the share of Seattle's workers earning low wages, as well as increases in the hours worked in Seattle at higher wage levels. This change in the distribution could be due to the Ordinance, but might also reflect labor demand growth outpacing supply, which would prompt a similar rightward shift in the wage distribution. Indeed, the Seattle metropolitan area enjoyed a strong labor market during this time period, with unemployment rates well below the national average. As shown in Appendix Figure 1 for outlying King County and for surrounding Snohomish, Kitsap, and Pierce Counties, we see somewhat similar changes in the distributions of hours.[24] Our methods seek to differentiate the impacts of the ordinance from background labor market trends.

5. Methodology

5.1 Determining a threshold for low-wage employment analysis

As indicated in section 2 above, we focus our analysis on jobs with calculated hourly wages below a fixed (inflation-adjusted) threshold. This proxy for low-skilled employment will produce accurate estimates of the impact of minimum wage increases to the extent that a wage threshold accurately partitions the labor market into affected and unaffected components. It will overstate employment reductions if the threshold is set low enough that the minimum wage increase causes pay for some work to rise above it. This concern is particularly relevant given previous evidence of "cascading" impacts of minimum wage increases on slightly higher-paying

[24] Outlying King County is defined as the area of King County excluding the cities of Seattle and SeaTac. SeaTac lies between Seattle and Tacoma with an area of 10 square miles mostly containing the Seattle-Tacoma International Airport. In 2013, SeaTac passed a law raising its minimum wage to \$15 per hour. We therefore exclude it from our analysis.

jobs (Neumark, Schwizer, and Wascher, 2004). It may understate proportional employment and wage effects if set too high, as effects on relevant jobs will be diluted by the inclusion of irrelevant positions in the analysis. Imagining a reaction function linking initial wages to post-increase wages, we aim to identify a fixed point above which there does not appear to be any impact.

To do this, we exploit the longitudinal links in ESD data to examine the pattern of wage increases experienced by individual workers at the discrete points when Seattle's minimum wage increased. To consider which workers' experiences are potentially relevant for this exercise, we select a preliminary threshold of $19 per hour, almost exactly twice the baseline minimum, a level beyond which cascading effects are less likely to occur (Neumark, Schwizer, and Wascher, 2004).[25] For employees in this category in a baseline quarter, we examine the full distribution of their hourly wages conditional on continued employment in a locatable Seattle firm one year later. We repeat this analysis with end quarters just before and after minimum wage increases to infer the impact of the minimum wage.[26]

Figure 3 presents four cumulative density functions, representing the results of this exercise for the periods ending just before and after Seattle's first and second minimum wage increases. The top panel shows densities which correspond to the time of the first minimum wage increase. Direct comparison of these densities reveals an expected consequence of the minimum wage increase: the cumulative density function visibly shifts to the right at the lowest wage levels, indicating that fewer tracked workers had wages below $11 after the first minimum wage increase, compared to workers tracked to a point just before the implementation date. Above $11 the two cumulative density functions quickly converge, indicating that the first minimum wage increase had little to no impact on the probability that a longitudinally tracked worker earned a wage greater than any threshold over $12. This is not to say that longitudinally tracked workers enjoyed no wage increases; indeed the cumulative density function shows that roughly 20% of the workers in this longitudinal sample moved from below $19 to above $19

[25] In the years before the minimum wage increase, a median Seattle worker earning the minimum wage worked about 1,040 hours per year (Klawitter, Long, and Plotnick, 2014). Using this figure, a family of two adults and one child with one adult working 1,040 hours at a wage of $19 per hour, would have a family income of $19,760, which is right above the official poverty threshold for such a family.

[26] This analytical strategy could be problematic to the extent there are significant anticipatory effects of minimum wage increases. Results below will indicate little to no evidence of anticipation effects associated with the Seattle minimum wage increases.

over one year. However, this probability appears equal before and after the minimum wage increase.

The bottom panel plots the pair of cumulative density functions which reveal the experiences of workers tracked just before and after the second minimum wage increase. Here, there is once again evidence of a rightward shift at the low end of the distribution, with the share of workers earning under $12, $13, or even $15 per hour dropping noticeably. The two cumulative densities overlap one another closely towards the right side of the chart. Once again, we infer that the minimum wage increase had no discernable impact on the probability that a longitudinally tracked worker earned a wage over any threshold higher than about $17.

Although the pairs of cumulative density functions plotted in Figure 3 overlap closely with one another above relatively modest thresholds, across-pair comparisons clearly show some rightward drift in the inflation-adjusted distribution, consistent with Seattle's overall pattern of robust employment growth. This rightward drift may be of little consequence to our analysis if it is also present in data for control regions. If it is not, this evidence shows that our best opportunity to cleanly identify minimum wage effects pertains to immediately apparent impacts.[27]

While the preponderance of evidence suggests that a low-wage threshold slightly above the statutory minimum poses little risk of miscoding jobs as lost when they have really been promoted to higher wage levels, in our preferred specifications we report findings based on a relatively conservative $19 threshold. In the analysis below, we evaluate impacts going up to a $25 threshold. As shown below, consistent with the results in Figure 3, we do not find evidence of gains in hours between $19 and $25 per hour caused by the Ordinance.

5.2 Causal identification strategy

We estimate the effect of the Ordinance on changes in employment and wages in Seattle relative to the 2nd quarter of 2014, when the Ordinance was passed. From this baseline period, we analyze effects over the next nine calendar quarters. The first three correspond to the period after

[27] Alternately, one could record the fact that over the period between early 2015 and early 2016 the probability of a worker earning under $19 remaining under $19 declined by about 2 percentage points, and consider this the result either of the minimum wage or exogenous increases in labor demand relative to supply. Under the assumption that 100% of the apparent drift can be attributed to the minimum wage, in spite of the fact that it occurs entirely across quarters where the minimum wage did not increase, this suggests our methods may overstate employment losses by about 2 percentage points.

the Ordinance was passed but before the first phase-in; this period is considered “post-treatment” in our analysis so that we can assess whether anticipatory effects ensued.[28] The minimum wage reached as high as $11 per hour in the fourth through sixth quarters after baseline and as high as $13 per hour in the remaining quarters. The “pre-treatment” period includes quarterly observations beginning in 2005.

Though we are interested in the cumulative effect of the minimum wage, we analyze variation in year-over-year changes in each outcome. This approach differences out seasonal fluctuations, and conforms to a standard time-series approach used in the prior literature. We define the year-over-year change in outcome Y as follows:

(1) $$\Delta Y_{rt} = Y_{rt}/\,Y_{r,t-4} - 1$$

where r denotes region (e.g. Seattle or comparison region), and t denotes quarter (with t ranging from -33 to 9, and $t = 0$ corresponding to the quarter during which the Ordinance was passed).

We begin with three candidate causal identification strategies. We will subject these strategies to a basic falsification test utilizing pre-treatment data before proceeding to the main analysis.

First, we consider a simple difference-in-differences specification, in which the outcomes of the treated region (Seattle in our case) are compared to the outcomes of a neighboring control region. We consider two different control regions. Comparison of Seattle to immediately surrounding King County can be thought of as equivalent to the contiguous county specification used by Dube, Lester and Reich (2010). Next, we compare growth rates in employment in Seattle to Snohomish, Kitsap, and Pierce Counties (SKP), which surround King County but do not share a border with Seattle (see Figure 4). Since a higher minimum wage might have a spillover effect on the parts of King County immediately adjacent to Seattle, we chose the counties which have similar local economic climates to Seattle’s, but are not immediately adjacent to Seattle, as a candidate control region. We expect SKP to experience a smaller (if any) spillover effect of the Ordinance compared to King County, and thus yield a less biased estimate of its impact.[29]

[28] Alternatively, if one assumes that anticipatory effects are unlikely, then these three months can be considered policy leads and used to evaluate whether there is divergence in pre-implementation trends. As we show below, we do not find significant evidence of anticipation effects, which could, alternatively, be interpreted as lack of divergence in pre-implementation trends.

[29] Our companion paper (Jardim et al., 2017) examines this possibility of spillover and mechanisms for estimating spillovers in greater detail.

In both cases, we estimate the following difference-in-differences specification:

$$\Delta Y_{rt} = \alpha_r + \psi_t + \sum_{q=1}^{9} \beta_q T_{rt} + \varepsilon_{rt}, \qquad (2)$$

where α_r is a region fixed effect, ψ_t is a period fixed effect, β_q is the treatment effect of the Ordinance in quarter $t = q$ (corresponding to the nine quarters after the Ordinance was passed), T_{rt} is an indicator that equals one for the treated region during which $t = q$, and ε_{rt} is an idiosyncratic shock.

In equation (2), $q = 1$ corresponds to the third quarter of 2014, the first quarter after the Ordinance had been passed; $q = 4$ corresponds to the second quarter of 2015, when the first phase-in of the Ordinance occurred; $q = 7$ corresponds to the first quarter of 2016, when the second phase-in occurred; and $q = 9$ corresponds to the third quarter of 2016, the last period of data currently available. Since our interest is in the cumulative effect of the Ordinance on each outcome, we convert these coefficients into cumulative changes, using the following rules. For quarters one to three $\beta_q^{cum} = \beta_q$; for quarters four to eight, $\beta_q^{cum} = (1 + \beta_q)\left(1 + \beta_{q-4}\right) - 1$; and for quarter nine $\beta_9^{cum} = (1 + \beta_9)(1 + \beta_5)(1 + \beta_1) - 1$. We present all results in terms of cumulative changes, and adjust the standard errors accordingly using the delta method.

The model in Equation 2 is a standard two-way fixed effect specification used in the literature (Neumark and Wascher, 2008). As pointed out in Bertrand, Duflo, and Mullainathan (2004), local economic outcomes in this model are not independent from each other, because they come from the same region. We account for this correlation by calculating two-way clustered standard errors at the region and year level.

Difference-in-differences specifications assume that the treated and control region have the same trends in the absence of the policy (parallel trends assumption), and will generally fail to produce consistent treatment effect estimates if this assumption is not true. It is prudent to be especially cautious about the parallel trends assumption given that the greater Seattle region experienced rapid economic growth coming out of the Great Recession, and the pace of recovery could have varied in different sub-regions. As we show below, our two difference-in-differences specifications fail a falsification test, which suggests divergent trends between Seattle and Outlying King County and between Seattle and SKP.

To overcome this concern, we estimate the impact of the minimum wage using two methods which allow for flexible pre-policy trends in control and treated regions: the synthetic control estimator (Abadie and Gardeazabal, 2003) and the interactive fixed effects estimator (Bai, 2009). Both methods have been used in the regional policy evaluation literature and applied to the minimum wage as well (see Allegretto, Dube, Reich, and Zipperer (2013) for an application of synthetic control, and Totty (2015) for an application of interactive fixed effects).

Both methods assume that changes in employment in each region can be represented as a function of K unobserved linear factors plus the treatment effect:

(3) $$\Delta Y_{rt} = \sum_{k=1}^{K} \lambda_{rk}\mu_{tk} + \sum_{q=1}^{9} \beta_q T_{rt} + \varepsilon_{rt},$$

where μ_{tk} is an unobserved factor, common across all regions in each year-quarter, and λ_{rk} is a region-specific factor loading, constant across time.

The unobserved factors can be thought of as common economic shocks which affect all regions at the same time, such as an exchange rate shock, common demand shock, or changes in weather. Because the regions are allowed to have different sensitivity in response to these shocks, the treated and control regions are no longer required to have parallel trends.

Though both the synthetic control and interactive fixed effects estimators have the same underlying model, their implementation is quite different. The synthetic control estimator does not explicitly estimate the factors or factor loading, and uses pre-policy observations to find an optimal set of (weighted) control regions, which collectively match the pre-policy trend in the treated region. Denote Seattle by $r = 1$ and denote $r = 2, \ldots, R$ all potential control regions. Then the weights for synthetic control can be found by minimizing forecasting error in the pre-policy period:

(4) $$\min_{w_r} \sum_{t=-33}^{0} \left(\Delta Y_{r=1,t} - \sum_{r=2}^{R} w_r \Delta Y_{rt}\right)^2,$$

subject to the constraints $\sum_r w_r = 1$ and $\forall r\ w_r \geq 0$.[30] Given a set of weights $\widehat{w_r}$, the impact of the Ordinance in quarter q is estimated as follows:

(5) $$\beta_q^{Synth} = \Delta Y_{r=1,q} - \sum_{r=2}^{R} \widehat{w}_r\, \Delta Y_{rq}\ .$$

We allow weights across regions to be different for each outcome to improve the quality of the match in 2005-2014. Appendix Figure 2 shows that the set of regions in Washington,

[30] We implement synthetic control estimator using the R programs provided by Gobillon and Magnac (2016).

which receive a positive weight in synthetic control estimator is very similar for employment outcomes and payroll, but somewhat different for wage rates.[31]

The interactive fixed effects approach estimates the factors and factor loadings in Equation 3 explicitly, by imposing normalization on the sum of the factors. Since the number of unobserved factors is not known, we estimate the model allowing for up to 30 unobserved factors, and pick the model with the optimal number of factors using the criterion developed in Bai and Ng (2002).[32] We implement the interactive fixed effects estimator following Gobillon and Magnac (2016) who have developed a publicly-available program to estimate the treatment effects in the regional policy evaluation context. Appendix Figure 3 shows the sensitivity of the interactive fixed effects estimates as a function of the number of factors used, as well as showing the choice of the optimal number of factors.We implement the synthetic control and interactive fixed effects estimators by approximating Seattle's economy using data on employment trends across Public Use Microdata Areas (PUMAs) in Washington State. A PUMA is a geographic unit defined by the U.S. Census Bureau with a population of approximately 100,000 people, designed to stay within county boundaries when possible.[33] We exclude King County PUMAs from analysis because of potential spillover effects. The remainder of Washington includes 40 PUMAs (see Figure 5), while Seattle is composed of five PUMAs.[34]

[31] Pairwise correlations between synthetic control weights chosen for hours worked, number of jobs, and payroll are each larger than 0.85, while the correlations of the synthetic control weights chosen for wages with weights chosen for the other three outcomes is positive, but smaller (0.21, 0.22, and 0.22). Examination of the weights, depicted in Appendix Figure 2, suggest a basic intuitive story: the strong growth in employment in Seattle finds its closest parallels in outer suburban or exurban portions of the state, where rapid population growth drives expansion of local economies. The strongest resemblance to Seattle in terms of wages, by contrast, tends to be in closer-in suburban areas, including the satellite centers of Tacoma and Everett.

[32] The coefficients, β_q, can be identified if the number of factors is smaller than the number of periods in the data minus the number of coefficients to be estimated minus one. In our case, we cannot have more than 32 factors in the model (43 periods – 9 coefficients – 1). We use a global criterion IC2 developed by Bai and Ng (2002) to pick the optimal number of factors, and the optimal number of factors is always smaller than the maximum number of factors allowed by the model. We choose the optimal number of factors using criterion IC2 suggested in Bai and Ng (2002), as it was shown to have good performance in small samples.

[33] Twenty-seven of Washington's thirty-nine counties have fewer than 100,000 inhabitants, implying that they must share a PUMA with territory in at least one other county.

[34] Given Seattle's unique status as a city experiencing a tech-driven economic boom, there may be some concern that our restriction to Washington State forces us to use comparison regions that match poorly to the City's labor market dynamics. We present evidence on the quality of fit between treatment and control region below. Intuitively, we seek regions that match Seattle's dynamics in the low-wage labor market, and Appendix Figure 2 reveals that the high quality matches tend to be found in suburban or exurban regions of the state that are themselves experiencing growth, often associated with new construction and expansion of the residential population.

Though the synthetic control and interactive fixed effects estimators generally perform similarly in Monte Carlo simulations (Gobillon and Magnac, 2016), analytic standard errors for interactive fixed effects estimator have been established, while standard errors for the synthetic control estimator are usually obtained using placebo estimates. We provide the baseline standard errors for the synthetic control estimates using an approach of "placebo in space," suggested by Abadie, Diamond, and Hainmueller (2014). We implement it by randomly selecting 5 PUMAs in Washington State as "treated" and estimate the placebo impact for these PUMAs.[35] As in Gobillon and Magnac (2016), we implement 10,000 draws to obtain the standard errors. The standard deviation of these estimated placebo impacts is our estimate of the standard error.[36, 37]

6. Results

6.1 Simple first-difference analysis

Table 3 presents summary statistics on the number of jobs, total hours worked, average wages, and total payroll in Seattle's single-location establishments for all industries and for food and drinking places by wage level for the quarter the Ordinance was passed (t = 0, including June 2014), the first three quarters after the law was passed (t = 1, 2, or 3, July 2014-March 2015), and the first six quarters after the law was in force (t = 4, 5, 6, 7, 8, or 9, April 2015-September 2016). These statistics portray a general image of the Seattle labor force over this time period and should not be interpreted as estimates of the causal impact of the Ordinance.

As shown in Panel A of Table 3, comparing the baseline second quarter of 2014 to the second quarter of 2016, the number of jobs paying less than $13 per hour in all industries declined from 39,807 to 24,420 (a decline of 15,387 or 39%).[38] The decline is consistent with

[35] Note that Seattle spans 5 PUMAs, thus our placebo treatment region replicates Seattle's size.

[36] We have also estimated the standard errors based on a "placebo in time" approach. It is implemented by randomly picking a period when the Ordinance is implemented using the data before the actual Ordinance went in effect, and estimating a placebo effect for this period. We then take the standard deviation of these estimated placebo effects as estimate of the standard error. Standard errors using the "placebo in space" approach prove to be more conservative (i.e. larger) than the standard errors using a "placebo in time", so we report the former standard errors in our baseline estimate.

[37] Computing standard deviation of the placebo impact as a standard error of the estimated impact assumes that the distribution of placebo impacts converges to normal distribution as the number of permutations increases. We have compared inference based on this normality assumption with the inference based on 95% confidence intervals derived from the distribution of placebo impacts. The conclusions about the statistical significance based on these two procedures are very similar, and as such we report the standard errors in our estimation tables.

[38] Note that we are using the second quarter of 2016 to avoid issues with seasonality. Seattle's low-wage labor force tends to peak in the third quarter of each year during the summertime tourist season, and exhibits a trough in the winter months.

legislative intent, and the persistence of employment at wages below $13 can be explained by the fact that lower minima applied to small businesses and those offering health benefits.[39]

The reduction in employment at wages under $13 could reflect either movement of wage rates above this threshold or the elimination of jobs. Table 3 panel A shows that over the same two-year time period, the number of jobs paying less than $19 per hour fell from 92,959 to 88,431 (a decline of 4,528 or 4.8%).[40] Measuring hours worked at low wages rather than employee headcount, the table shows a 5.8 million hour reduction at wage rates under $13, and a 1.7 million hour (4.5%) reduction at wages under $19.

Over this same period, overall employment in Seattle expanded dramatically, by over 13% in headcount and 15% in hours. Table 3 makes clear that the entirety of this employment growth occurred in jobs paying over $19 per hour.[41] The impression of skewed growth – driven in part by rapid growth in the technology sector – extends to wage data.[42] Average hourly wages at jobs paying less than $19 rose from $14.14 to $15.01 (a 6.1% increase), while average hourly wages at all jobs surged from $36.93 to $44.04 (a 19.2% increase).[43]

Table 3 documents that payroll reductions attributable to declines in hours worked very nearly offset the observed wage increases for jobs paying under $19. Comparing "peak" third quarter statistics in 2014 and 2016, the sum total of wages paid at rates under $19 actually declines by over $6 million.[44] Similar comparisons of second quarter statistics reveal a comparably-sized increase.

Panel B of Table 3 restricts attention to Food and Drinking Places (NAICS industry 722), which, respectively, comprised 27%, 20%, and 10% of jobs in Seattle's locatable establishments

[39] Low-wage employment could also reflect overestimation of hours by the employer, underreporting of tips, hours worked for wages paid in a different calendar quarter, or a subminimum wage set equal to 85% of the minimum for workers under 16 years old.

[40] Appendix Table 2 breaks down the changes in employment into more wage categories. The largest gains in employment occurred for jobs paying more than $40 per hour, which grew 32% between 2014.2 and 2016.2.

[41] The more detailed statistics in Appendix Table 2 show that net job growth in Seattle was positive for jobs paying over $25/hour but negative for jobs paying under $25. About 80% of net job growth can be attributed to jobs paying over $40/hour, and 95% to jobs paying over $30/hour.

[42] Quarterly Census of Employment and Wage (QCEW) data for King County indicate that between 2014 and the third quarter of 2016, the county added 94,000 jobs. The majority of these job gains can be attributed to four industries: non-store retail, information, professional/technical services, and construction. The food service industry added more than 10,000 jobs countywide over this same time period.

[43] The average hourly wage statistic at all wage levels includes a large number of salaried jobs in which hours may be imputed at 40 per week rather than tracked.

[44] At the same time, total quarterly wages paid at rates above $19 increased by $1.7 billion – implying a dramatic increase in inequality of earnings between low- and high-wage workers in Seattle.

paying less than $13, less than $19, and overall during the quarter the Ordinance was passed. Although this industry accounts for a minority of all low-wage employment, we highlight it for purposes of comparison with existing literature.

As in the full economy, growth in hours at restaurant jobs paying above $19 per hour exceeded growth in lower-paying restaurant jobs. At all wages, hours within this industry expanded by 12.9% while hours worked by low-wage employees in the restaurant industry was nearly unchanged, down 0.2% between the second quarter of 2014 and the second quarter of 2016. Wages in the restaurant sector grew comparably in the low-wage market and the full market: 12.1% growth in wages in jobs paying less than $19 per hour, and 13.6% growth in wages in all jobs.

6.2 Falsification tests

Previous analyses have raised concerns regarding the applicability of the parallel trends assumption in minimum wage evaluation. As noted above, the short duration of our post-treatment panel makes it infeasible to employ the traditional linear time-trend correction. For this reason, and to assess the performance of our proposed estimators, we conduct a simple falsification test by estimating the effects of a "placebo" law as if it were passed two years earlier (second quarter of 2012). We restrict this analysis to data spanning from the first quarter of 2005 to the third quarter of 2014. Table 4 presents the results.

We find strong evidence that total hours worked in jobs paying less than $19 per hour in Seattle diverged from both surrounding King County and SKP after second quarter 2012, as shown in columns 2 and 4. In both columns, all of the estimated pseudo-effects on hours are negative and significant, and would falsely suggest the placebo law caused a reduction in hours of 4.1% or 5.0%, respectively, in the average quarter following the second quarter of 2012. Given this divergent trend, we consider the two difference-in-differences estimators to have failed the falsification test and dispense with them henceforth.

In contrast, the synthetic control results shown in columns 5 and 6 behave well. In the average quarter following the placebo law, we find a 0.4% increase in wages and 0.1% increase in total hours. The pseudo-effects on wages, which are all positive, but mostly insignificant, are somewhat concerning – if these same positive pseudo-effects persist into the period that we study, we would be modestly overstating the effect of Seattle's minimum wage on wages, and thus understating elasticities of hours with respect to changes in wages.[45] The pseudo-effects on hours flip back-and-forth between positive and negative.

Finally, columns 7 and 8 show the estimates of the pseudo-effects using the interactive fixed effects specification. This specification finds no pseudo-effect on wages, while the pseudo-effects on hours are all negative, yet insignificant (with larger standard errors), and average -1.9%. If these same negative pseudo-effects on hours persist into the period that we study, we would be moderately overstating the negative effect of Seattle's minimum wage on hours. Consequently, we conclude that the synthetic control method is the most trustworthy, but include interactive fixed effect models below with the caveat that they may be prone to overstating negative employment impacts.

6.3 Examining the synthetic control match

Figure 6 plots the time series of year-over-year percentage changes in average wages, jobs, hours worked, and payroll for low-wage jobs in Seattle and the weighted average of PUMAs outside King County identified using the synthetic control algorithm.[46] In each panel, there is a very strong pre-policy match in trends between Seattle and the control region. As shown in Panel A, wage growth patterns in Seattle and control regions match closely, with growth rates matching to within a 0.5 percentage point tolerance except around 2009, where wage trends in the control region appear to anticipate those in the city.

Employment trends (panels B and C for jobs and hours, respectively) likewise match closely, with discrepancies below a 2-percentage point threshold except in the period around the Great Recession, where the control regions appear to enter and exit the slump slightly before the city itself. Total payroll growth also matches closely throughout the pre-policy period.

These graphs anticipate our causal effect estimates: in all cases, the post-ordinance period is marked by treatment-control divergences well outside the range observed in the pre-treatment period.

6.4 Causal effect estimates

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[45] These positive wage effects are consistent with other evidence indicating robust labor demand in Seattle, including the cumulative density functions in Figure 2 above.

[46] Appendix Figure 4 shows a parallel analysis of the time series for Seattle compared to Outlying King County and SKP.

Table 5 presents our first estimates of the causal impact of the Ordinance for workers earning less than $19 per hour. Looking at both sets of results, we associate the first minimum wage increase, to $11, with wage effects of 1.4% to 1.9% (averaging 1.7%). The second increase, to $13, associates with a larger 2.8% to 3.6% wage effect (averaging 3.1%). A 3.1% increase in the wage of these workers corresponds to $0.44 per hour relative to the base average wage of $14.14.[47] We do not find strong evidence that wages rose in anticipation of enforcement during the three quarters following passage of the law. The small coefficients range from 0.3% to 0.7% and most are statistically insignificant.

These wage effect estimates appear modest in comparison to much of the existing literature. We note that the first-difference results presented in Table 3 themselves indicate modest increases in wages at the low end of the scale (under $19), about 4.5% during the first phase-in and 6.0% during the second. These estimates suggest that wages increased in the control region as well.[48] We further note that Table 3 indicates that the majority of low-wage jobs observed at baseline – 62% when defined as jobs paying under $19 per hour and weighted by hours – were not directly impacted by the minimum wage increase to $13. Any impacts on wages paid for jobs between $13 and $19 per hour at baseline would be "cascading" effects expected to be much smaller than the impact on lowest earners. Figure 3 above confirms that very little impact on the cumulative wage distribution of longitudinally tracked workers can be observed above relatively low thresholds. If we were to presume that our estimate reflects some sizable impact on jobs directly impacted by the increase and no cascading effects on other jobs under $19, the impact works out to a 7.9% wage increase, a level in line with existing literature.[49] Finally, we note that the measure of wages used here – average hourly wages – would by construction capture employer responses such as a reduction in the use of overtime. These would not be captured in, for example, self-reported CPS wage data.

Table 6 shows employment impacts for jobs paying less than $19 per hour. As shown in columns 1 and 2, relative to the baseline quarter (2014.2), we estimate statistically insignificant

[47] Estimated wage impacts are larger when the low-wage threshold is lowered from $19. This is consistent with the minimum wage ordinance having sizable effects on the lowest-paid workers and smaller cascading impacts on workers with initial wages closer to $19.

[48] Data from the Bureau of Labor Statistics' Current Employment Statistics indicate that seasonally adjusted average hourly earnings for all employees increased about 5.5% nationwide from June 2014 to September 2016.

[49] Belman and Wolfson (2014) point to elasticities of wages paid to statutory minimum wage increases in the range of 0.2 to 0.5. An effect of 7.9% on a minimum wage increase of 37% would imply an elasticity just over 0.2. We note, moreover, that the full $13 minimum did not apply to small business or businesses providing health benefits.

hours reductions between 0.9% and 3.4% (averaging 1.9%) during the three quarters when the minimum wage was $11 per hour. By contrast, the subsequent minimum wage increase to $13 associates with larger, significant hours reductions between 7.9% and 10.6% (averaging 9.4%). Columns 3 and 4 present a parallel analysis for jobs, with qualitatively similar results: statistically weak evidence of reductions in the first phase-in period followed by larger significant impacts in the second. The adverse effects on hours in the final three quarters are proportionately greater than the effects on jobs, suggesting that employers are not only reducing the number of low-wage jobs, but also reducing the hours of retained employees. Multiplying the -6.8% average job estimate by the 92,959 jobs paying less than $19 per hour at baseline suggests that the Ordinance caused the elimination of 6,317 low-wage jobs at locatable firms.[50] Scaled up linearly to account for multi-site single-account firms, job losses would amount to roughly 10,000.[51]

As noted above, there is some concern that our methodology might yield negative estimates in scenarios where increasing labor demand is leading to a rightward shift in the overall wage distribution, pushing a growing number of jobs above any given threshold. We note that the results in Table 6 are consistent with this “rightward shift” hypothesis only under a specific and unusual set of circumstances. In the synthetic control estimates for hours, for example, we observe no significant negative coefficients through the end of 2015 – in fact, the point estimates for the first and last quarters of 2015 are nearly identical. The point estimate exhibits a sudden change in the first quarter of 2016 and then remains at this more negative level without exhibiting any further trend. A confounding rightward shift would have had to occur precisely at the beginning of 2016 – in the winter, the trough period of Seattle’s seasonal economy. Figure 3 shows no evidence of such a precisely-timed rightward shift among continuously employed workers tracked longitudinally.

To probe this issue further, Figure 7 illustrates the sensitivity of the estimated effect on hours using different thresholds ranging from jobs paying less than $11 to jobs paying less than $25. For the effect of raising the minimum wage to $11 per hour, shown in the top panel, the

[50] If we base this calculation on just the synthetic control estimates, we would conclude that the Ordinance led to 5,133 fewer jobs paying less than $19 per hour.

[51] We cannot ascertain whether the effect on locatable firms should extrapolate to multi-site single-account firms. As noted above, survey evidence suggests that multi-location firms were more likely to have reported reducing staffing in the wake of minimum wage increases.

estimated impacts become insignificant once the threshold rises to around $17. It appears that any "loss" in hours at lower thresholds likely reflects a cascade of workers to higher wage levels. In contrast, as shown in the bottom panel, the negative estimated effects of the second phase-in to $13 are significant as we raise the threshold all of the way to $25 per hour. Thus, there is no evidence to suggest that the estimated employment losses associated with the second phase-in reflect a similar cascading phenomenon.

Figure 8 illustrates these same results, but multiplies the estimated coefficients by the baseline number of hours worked in jobs paying below the threshold. These results show the estimated absolute change in total hours. We find that during the second phase-in period low-wage hours fell by 3.5 million hours per quarter when the threshold is set at $19 per hour, and this result remains as we increase the threshold to $25 per hour.[52]

Because the estimated magnitude of employment losses exceeds the magnitude of wage gains in the second phase-in period, we would expect a decline in total payroll for jobs paying under $13 per hour relative to baseline. Indeed, we observe this decline in first-differences when comparing "peak" calendar quarters, as shown in Table 3 above. Table 7 confirms this inference in regression specifications examining the impact on payroll for jobs paying less than $19 per hour. Although results are not consistently significant, point estimates suggest payroll declines of 4.0% to 7.6% (averaging 5.8%) during the second phase-in period. This implies that the minimum wage increase to $13 from the baseline level of $9.47 reduced income paid to low-wage employees of locatable Seattle businesses by roughly $120 million on an annual basis.[53]

Note that the largest and only statistically significant payroll estimate corresponds to the first quarter of 2016. This result is notable, as the first quarter tends to be a time of slack demand for low-wage labor (after Christmas and before the summer tourist season) – in effect, Seattle suffers a mini recession every winter. This result could be a harbinger of the effects of the minimum wage in a full recession, or in a less robust local economy, as wages will have less ability to decrease to equilibrate the low-wage labor market.[54]

[52] Confidence intervals widen as we increase the threshold – we are, in essence, looking for the same needle (i.e., the same 3.5-million-hour decline) in a larger haystack as we increase the threshold.
[53] Simple calculations based on preceding results suggest an effect of comparable magnitude. Wage results suggest a 3% boost to earnings, which on a base of about $530 million paid in the baseline quarter amounts to a $16 million increase in payroll. Employment declines of 3.5 million hours per quarter, valued at $9.47 per hour, equate to a loss of $132 million – and a net loss of $116 million – on an annual basis.
[54] See Clemens (2015), Clemens and Wither (2016), and Clemens and Strain (2017) for evidence of the effects of the Great Recession on impacts of minimum wage increases.

6.5 Elasticity estimates

Column 1 of Table 8 shows our estimate of the elasticity of labor demand with respect to changes in wages computed as the ratio of our estimated effect on hours to our estimated effect on wages, using the synthetic control method, for the six quarters after the Ordinance was enforced.[55] We also compute measures of statistical uncertainty for these elasticities since they are the ratio of two estimates.[56] During the first phase-in, when the minimum wage was $11 per hour, estimated elasticities range from -0.97 to -1.80 (averaging -1.31). Notably, we cannot reject elasticity = -1 with 95% confidence, which is consistent with our finding in Table 7 that we could not reject zero effect on payroll, and we cannot reject elasticity = 0, which is consistent with our finding in Table 6 that we could not reject zero effect on hours. These findings are not artifacts of setting the threshold at $19 per hour. As shown in the upper part of Figure 9, the estimated elasticities range between -1 and 0 when the threshold is set anywhere between $17 and $25 per hour. In summary, the relatively modest estimated wage and hours impacts of the first phase-in create considerable statistical uncertainty regarding the associated elasticity estimate.

After the minimum wage increased to $13 per hour, we find much larger estimated elasticities ranging from -2.66 to -3.46 (averaging -2.98). During these three quarters, we can reject the hypothesis that the elasticity equals zero (consistent with Table 5), and we can reject the hypothesis that the elasticity equals -1 in the first quarter of 2016, consistent with the significant decline in payroll during this quarter shown in Table 6. Point estimates of elasticities imply that, within Seattle, low-wage workers lost $3 from lost employment opportunities for every $1 they gain due to higher hourly wages. These very large elasticities are not artifacts of setting the threshold at $19 per hour. As shown in the lower part of Figure 9, the estimated

[55] One might think that the decline in hours worked was due to a voluntary cut in hours, and thus interpret our findings as showing a labor supply elasticity in the region where the labor supply curve is "backwards bending." While there may be some voluntary reductions in hours by some workers, it would be unreasonable to expect such workers to reduce their hours so far that their total earnings declined. Given that we find that hours fall more than wages rise, the results are more likely to reflect a decline in labor demand.

[56] We computed standard errors for the estimates elasticities using the delta method, taking into account the correlation between estimated effect of the minimum wage on employment and wages.

elasticities are very close to -3 when the threshold is set anywhere between $17 and $25 per hour.[57]

The larger elasticities in the second phase-in period relative to the first suggest that total earnings paid to low-wage workers in Seattle might be maximized with a statutory minimum wage somewhere in the range of $9.47 to $11. By contrast, increases beyond $11 appear to have resulted in net earnings losses in Seattle for these workers.

6.6 Reconciling these estimates with prior work

Most prior studies compute employment elasticities by dividing regression-estimated percentage changes in employment by the percentage change in the statutory minimum wage. Applied in this case, this method would use a denominator of 16.2% (i.e., ($11-$9.47)/$9.47) for the first phase-in period, and 37.3% ($13-$9.47)/$9.47) for the second. The conventional method clearly overstates the actual impact on wages given that many affected workers' wages are above the old minimum but below the new. This method is also unsuitable for evaluating the impacts on workers who began over the new minimum wage but are nonetheless affected by cascading wage increases (defined as the range of either $11 or $13 to $19 per hour). In column 2 of Table 8, we use the conventional approach for computing employment elasticities and find estimates in the range of -0.08 to -0.28 (averaging -0.20). This range is high but not outside of the envelope of estimates found in prior literature (see Appendix Table 3).[58] Thus, computing the elasticity based on the Ordinance's impact on *actual* average wages suggests that the conventional method yields substantial underestimates.

We conclude our analysis by attempting to reconcile our results with prior studies focused on restaurant industry employment. In Table 9, we walk our results back to a sample and outcome that is similar to Card and Krueger's (1994) examination of fast food employment in New Jersey and Pennsylvania in response to New Jersey's increase in its minimum wage. The traditional focus on restaurant employment reflects its common perception as a canonical low-wage industry, and the general absence of data resources allowing a more precise analysis of jobs

[57] While it may be argued that our wage effects combine a large effect on the lowest-paid workers with near-zero impacts on those paid above $13 at baseline, this only implies an overestimated elasticity for the least-paid workers if the employment effects are somehow concentrated among higher-paid workers. Our evidence does not support this conjecture.
[58] Estimates on the high end are plausible because theory suggests that labor demand elasticity would generally be larger for a small, open economy such as Seattle than for a state or the nation.

paying low wages. In 46 of 50 states, there is no data resource allowing the systematic computation of average hourly wage rates for the entire UI-covered workforce.

Column 1 of Table 9 repeats the main results findings from column 1 of Table 6, and is included as a point of reference. Moving from column 1 to column 5 of Table 9, we make one change at a time to evaluate the sensitivity of our results to various modeling choices. In column 2, we use the same specification as in column 1, but restrict the analysis to hours in low-wage jobs in Food Services and Drinking Places (NAICS industry 722). The results are quite comparable to those in column 1 for all industries. We find significant declines in hours worked by low-wage restaurant workers in two of the last three quarters when the wage increased to $13 per hour, and this reduction averages -10.1%. Moving from column 2 to 3, we switch the focus to headcount employment, the outcome used in most prior literature. Again, these results are quite comparable suggesting that nearly all of the reduction in hours worked by low-wage restaurant workers is coming from a reduction in jobs rather than a reduction in hours worked by those who have such jobs.

In columns 4 and 5, we shift from examining low-wage jobs to *all* jobs in the restaurant industry. Here we see a *dramatic change*: the effects on all jobs (hours in all jobs) are insignificant in all quarters and averages +0.4% (-0.8%) in the last three quarters.[59] Thus, by using the imprecise proxy of all jobs in a stereotypically low-wage industry, prior literature may have substantially underestimated the impact of minimum wage increases on the target population.

In summary, utilizing methods more consistent with prior literature allows us to almost perfectly replicate the conventional findings of no, or minor, employment effects. These methods reflect data limitations, however, that our analysis can circumvent. We conclude that the stark differences between our findings and most prior literature reflect in no small part the impact of data limitations on prior work.

7. Conclusion

There is widespread interest in understanding the effects of large minimum wage increases, particularly given efforts in the US to raise the federal minimum wage to $15 per hour

[59] The finding of a more negative effect on all hours than on all jobs in Food and Drinking Places is consistent with Neumark and Wascher's (2000) critique of Card and Krueger (1994).

and the adoption of high minimum wages in several states, cities and foreign countries in the past few years. There is good reason to believe that increasing the minimum wage above some level is likely to cause greater employment losses than increases at lower levels. Wolfers (2016) argues that labor economists need to "get closer to understanding the optimal level of the minimum wage" (p. 108) and that "(i)t would be best if analysts could estimate the marginal treatment effect at each level of the minimum wage level" (p. 110). This paper extends the literature in a number of ways, one of which is by evaluating effects of two consecutive large local minimum wage increases.

Beyond basic causal inference challenges, prior studies have analyzed minimum wage effects using data resources that do not permit the direct observation of hourly wages. In those situations, researchers resort to using proxies for low-wage workers by examining particular industries that employ higher concentrations of low-wage labor or by restricting the analysis to teenagers. This paper demonstrates that such strategies likely misstate the true impact of minimum wage policies on opportunities for low-skilled workers. Our finding of zero impact on headcount employment in the restaurant industry echoes many prior studies. Our findings also demonstrate, however, that this estimation strategy yields results starkly different from methods based on direct analysis of low-wage employment.

Our preferred estimates suggest that the Seattle Minimum Wage Ordinance caused hours worked by low-skilled workers (i.e., those earning under $19 per hour) to fall by 9.4% during the three quarters when the minimum wage was $13 per hour, resulting in a loss of 3.5 million hours worked per calendar quarter. Alternative estimates show the number of low-wage jobs declined by 6.8%, which represents a loss of more than 5,000 jobs. These estimates are robust to cutoffs other than $19.[60] A 3.1% increase in wages in jobs that paid less than $19 coupled with a 9.4%

[60] The finding of significant employment losses, particularly after the second minimum wage increase in 2016, may seem incongruent with unemployment statistics for the City of Seattle, which suggest very low numbers of unemployed individuals seeking work. The Bureau of Labor Statistics' Local Area Unemployment Statistics program estimates city-level unemployment statistics on the basis of unemployment insurance claims, data from other government surveys such as the Current Population Survey, and statistical modeling. The unemployment statistics pertain to the residents of a city, not individuals employed in a city (indeed, unemployed workers are employed in no city). Our analysis pertains instead to individuals employed in Seattle.

In Washington State, workers are eligible for UI benefits only after they have accumulated 680 hours of work. In low-wage, high-turnover businesses, the proportion of separated workers who reach this threshold may be low. Further, longitudinal analysis of ESD data suggest that reduced employment largely impacts new entrants to the labor force, rather than experienced workers. New entrants are not eligible for UI benefits and thus cannot generate claims. These unemployed new entrants might be captured in the CPS, but with a relatively small sample size these estimates are subject to significant noise and are smoothed considerably.

loss in hours yields a labor demand elasticity of roughly -3.0, and this large elasticity estimate is robust to other cutoffs.

These results suggest a fundamental rethinking of the nature of low-wage work. Prior elasticity estimates in the range of zero to -0.2 suggest there are few suitable substitutes for low-wage employees, that firms faced with labor cost increases have little option but to raise their wage bill. Seattle data show – even in simple first differences – that payroll expenses on workers earning under $19 per hour either rose minimally or fell as the minimum wage increased from $9.47 to $13 in just over nine months. An elasticity of -3 suggests that low-wage labor is a more substitutable, expendable factor of production. The work of least-paid workers might be performed more efficiently by more skilled and experienced workers commanding a higher wage. This work could, in some circumstances, be automated. In other circumstances, employers may conclude that the work of least-paid workers need not be done at all.

Importantly, the lost income associated with the hours reductions exceeds the gain associated with the net wage increase of 3.1%. Using data in Table 3, we compute that the average low-wage employee was paid $1,897 per month. The reduction in hours would cost the average employee $179 per month, while the wage increase would recoup only $54 of this loss, leaving a net loss of $125 per month (6.6%), which is sizable for a low-wage worker.

The estimates may be much larger than those reported in prior minimum wages studies for three reasons. First, theory suggests that labor demand elasticity would generally be larger for a small, open economy such as Seattle than for a state or the nation. Yet, there is evidence to suggest that our results are not simply divergent from the literature due to this issue. Note that Seattle data produce an effect estimate of zero when we adopt the traditional approach of studying restaurant employment at all wage levels.

Second, rather than using the statutory change in the minimum wage as the denominator in an elasticity computation, we use the change in actual wage rates for low-skill workers, which we can estimate from the Washington data. Because the actual change is necessarily smaller than the statutory change, the arithmetic of elasticity computation leads to larger estimated elasticities than those derived using conventional methods of computing the elasticity of demand for low-skill workers with respect to the statutory change in minimum wage.

Third, we analyze the impact of raising the minimum wage to a significantly higher level than what has been analyzed in most prior work. Deflating by the Personal Consumption

Expenditures price index, the real value of the federal minimum wage has never reached the $13 level studied in our analysis. Theory suggests that the impact of raising the minimum wage depends critically on the starting point; Seattle started from the nation's highest state minimum wage, and our own evidence indicates that the effects differed dramatically from the first phase-in period to the second.

A few cautions should be noted. Our analysis includes only firms reporting employment at specific locations, as we cannot properly locate employment for multi-location firms that do not report employment separately by location. It may be the case that the labor demand elasticity of locatable firms is larger than that of multi-site firms who do not report employment at specific locations. Yet, as discussed above, multi-site firms that we surveyed were more likely to self-report cuts in employment than smaller firms.[61]

Further, we lack data on contractor jobs which get 1099 forms instead of W-2s and on jobs in the informal economy paid with cash. If the Ordinance prompted an increase in low-wage workers being paid as contractors or under the table, our results would overstate the effect on jobs and hours worked. However, such a move would not be without consequence for the workers, who would lose protections from the Unemployment Insurance and Worker's Compensation systems and not receive credit toward future Social Security benefits for such earnings (though they would not have to pay the full amount of taxes for Social Security and Medicare).

In addition, some employers may have shifted jobs out of Seattle but kept them within the metropolitan area, in which case the job losses in Seattle overstate losses in the local labor market. Reductions in payroll attributable to the minimum wage may exceed reductions in income for the affected workers, to the extent they were able to take advantage of relocated opportunities in the metropolitan area. Finally, the long-run effects of Seattle's minimum wage increases may be substantially greater, particularly since subsequent changes beyond a final increase to $15 per hour will be indexed to inflation, unlike most of the minimum wage increases that have been studied in the literature, which have quickly eroded in real terms (Wolfers, 2016).

[61] If we ignore our survey evidence and suppose that multi-site firms' wage impact was the same as reported here but their hours impact was zero, the elasticity would still be high compared to earlier work – around -1.9 (as single-site businesses employ 62% of the workforce).

One cannot assume our specific findings generalize to minimum wage policies set by other localities or at the federal or state level. The impacts of minimum wage policies established by other local governments likely depend on the industrial structure, characteristics of the local labor force, and other features of the local and regional economy.

Last, there may be important forms of effect heterogeneity across workers. Some workers may well have experienced significant wage increases with no reduction in hours; others may have encountered significantly greater difficulty in securing any work at all. From a welfare perspective, it is critical to understand how this heterogeneity plays out across low-skilled workers in varying life circumstances. Such an exploration is beyond the scope of this paper, which uses a data resource that identifies no pertinent information about individual workers. Future work will take advantage of linkages across administrative data resources within Washington State to understand how the minimum wage affects workers in varying demographic categories, or with a history of reliance on means-tested transfer programs.

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Tables and Figures

Table 1: Minimum Wage Schedule in Seattle under the Seattle Minimum Wage Ordinance

	Large Employers[a]		Small Employers	
Effective Date	No benefits	With benefits[b]	No benefits or tips	Benefits or tips[c]
	Before Seattle Ordinance			
January 1, 2015	$9.47	$9.47	$9.47	$9.47
	After Ordinance			
April 1, 2015	$11.00	$11.00	$11.00	$10.00
January 1, 2016	$13.00	$12.50	$12.00	$10.50
January 1, 2017	$15.00[d]	$13.50	$13.00	$11.00
January 1, 2018		$15.00[e]	$14.00	$11.50
January 1, 2019			$15.00[f]	$12.00
January 1, 2020				$13.50
January 1, 2021				$15.00[g]

Notes:

a A large employer employs 501 or more employees worldwide, including all franchises associated with a franchise or a network of franchises.
b Employers who pay towards medical benefits.
c Employers who pay toward medical benefits and/or employees who are paid tips.
Total minimum hourly compensations (including tips and benefits) is the same as for small employers who do not pay towards medical benefits and/or tips.
d For large employers, in the years after the minimum wage reaches $15.00 it is indexed to inflation using the CPI-W for Seattle-Tacoma-Bremerton Area.
e Starting January 1, 2019, payment by the employer of medical benefits for employees no longer affects the hourly minimum wage paid by a large employer.
f After the minimum hourly compensation for small employers reaches $15 it goes up to $15.75 until January 1, 2021 when it converges with the minimum wage schedule for large employers.
g The minimum wage for small employers with benefits or tips will converge with other employers by 2025.

Table 2: Characteristics of Included and Excluded Firms, Washington State

	Included in Analysis	Excluded from Analysis	Share Included
Number of Firms	123,180	14,917	89.2%
Number of Establishments (i.e., Sites)	140,451	Unknown	
Total Number of Employees	1,672,448	1,019,875	62.1%
Number of Employees paid <$19/hour	725,231	425,023	63.0%
Employees / Firm	14	68	
Employees / Establishment	12	Unknown	

Notes: Firms are defined as entities with unique federal tax Employer Identification Numbers. Statistics are computed for the average quarter between 2005.1 and 2016.3. “Excluded from Analysis” includes firms whose location could not be determined.

Table 3: Employment Statistics for Seattle's Locatable Establishments

	Quarters After	Number of Jobs			Total Hours (thousands)			Average Wage			Total Payroll ($mlns.)		
		Hourly wage rates:			Hourly wage rates:			Hourly wage rates:			Hourly wage rates:		
Quarter	Passage/ Enforcement	Under $13	Under $19	All	Under $13	Under $19	All	Under $13	Under $19	All	Under $13	Under $19	All
Panel A: All Industries													
2014.2	0	39,807	92,959	292,640	14,117	37,408	130,007	11.14	14.14	36.93	157	529	4,802
2014.3	1	40,706	94,913	300,892	14,527	38,565	132,604	11.15	14.15	37.76	162	546	5,007
2014.4	2	35,421	89,598	303,089	11,999	35,589	136,012	11.27	14.37	39.78	135	511	5,410
2015.1	3	35,085	90,813	305,229	11,335	34,269	132,275	11.28	14.41	40.61	128	494	5,371
2015.2	4/1	35,075	92,668	311,886	12,174	37,270	139,197	11.47	14.48	38.52	140	540	5,362
2015.3	5/2	33,959	93,382	320,807	11,589	37,472	142,638	11.54	14.58	39.83	134	546	5,681
2015.4	6/3	30,002	87,067	320,195	9,924	34,943	146,960	11.64	14.74	41.73	116	515	6,133
2016.1	7/4	24,662	87,122	321,360	7,645	33,031	140,429	11.82	14.97	43.90	90	494	6,164
2016.2	8/5	24,420	88,431	331,927	8,315	35,681	149,514	11.87	15.01	44.04	99	535	6,584
2016.3	9/6	23,232	86,842	336,517	8,046	35,867	153,603	11.87	15.03	43.60	96	539	6,697
Panel B: Food and Drinking Places (NAICS 722)													
2014.2	0	10,614	18,788	28,276	3,707	6,772	9,941	10.96	12.99	17.53	41	88	174
2014.3	1	10,825	19,581	29,815	3,792	7,229	10,763	10.94	13.10	17.82	41	95	192
2014.4	2	9,778	19,278	30,237	3,253	6,857	10,458	11.05	13.35	18.54	36	92	194
2015.1	3	9,682	19,493	30,505	3,044	6,567	10,100	11.08	13.44	18.62	34	88	188
2015.2	4/1	9,006	19,122	30,500	3,025	6,874	10,629	11.38	13.67	18.65	34	94	198
2015.3	5/2	8,376	19,622	31,895	2,843	7,282	11,500	11.47	13.94	19.09	33	101	219
2015.4	6/3	7,566	19,550	32,439	2,461	7,107	11,398	11.54	14.15	19.74	28	101	225
2016.1	7/4	5,869	18,651	31,469	1,730	6,307	10,396	11.83	14.54	20.07	20	92	209
2016.2	8/5	6,155	18,504	31,980	1,983	6,756	11,222	11.90	14.56	19.92	24	98	224
2016.3	9/6	6,050	18,542	32,402	2,034	7,236	12,088	11.85	14.59	20.11	24	106	243

Note: Data derived from administrative employment records obtained from the Washington Employment Security Department. Non-locatable employers (i.e., multi-site single-account firms) are excluded.

Table 4: Falsification Test: Pseudo-Effect of Placebo Law Passed in 2012

Quarter	Quarters after (pseudo) Passage/ Enforcement	Difference-in-Differences between Seattle and: Outlying King County		Snohomish, Kitsap, and Pierce Counties		Synthetic Control: Washington excluding King County		Interactive Fixed Effects: Washington excluding King County	
		Wage	Hours	Wage	Hours	Wage	Hours	Wage	Hours
2012.3	1	0.001*	-0.044***	-0.003**	-0.014***	0.001	-0.014	-0.002	-0.012
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.015)	(0.003)	(0.013)
2012.4	2	-0.002***	-0.033***	-0.003*	-0.038***	0.001	-0.018	-0.001	-0.022
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.021)	(0.003)	(0.014)
2013.1	3	0.002***	-0.034***	0.001	-0.028***	0.001	-0.002	0.000	-0.017
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.020)	(0.003)	(0.038)
2013.2	4/1	0.003***	-0.022***	0.005***	-0.036***	0.001	0.004	0.001	-0.016
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.026)	(0.003)	(0.038)
2013.3	5/2	0.003***	-0.063***	-0.002	-0.063***	0.004	-0.006	-0.002	-0.024
		(0.001)	(0.007)	(0.003)	(0.012)	(0.005)	(0.022)	(0.004)	(0.041)
2013.4	6/3	0.003**	-0.069***	-0.006*	-0.095***	0.006	-0.009	0.000	-0.034
		(0.001)	(0.007)	(0.003)	(0.012)	(0.004)	(0.033)	(0.004)	(0.049)
2014.1	7/4	0.003**	-0.031***	0.001	-0.047***	0.005	0.028	-0.001	-0.008
		(0.001)	(0.007)	(0.003)	(0.012)	(0.004)	(0.029)	(0.004)	(0.053)
2014.2	8/5	0.006***	-0.031***	0.004	-0.059***	0.008***	0.014	0.003	-0.024
		(0.001)	(0.007)	(0.003)	(0.012)	(0.004)	(0.031)	(0.004)	(0.055)
2014.3	9/6	0.004**	-0.046***	-0.001	-0.073***	0.010*	0.013	0.000	-0.019
		(0.002)	(0.011)	(0.005)	(0.017)	(0.005)	(0.031)	(0.005)	(0.081)
Average		0.003	-0.041	0.000	-0.050	0.004	0.001	0.000	-0.019
Obs.		68	68	68	68	1,530	1,530	1,530	1,530

Notes: Standard errors in parentheses. Clustered standard errors reported for difference-in-differences; permutation inference standard errors are reported for synthetic control, iid standard errors are reported for interactive fixed effects. Estimates for all jobs paying < $19 in all industries. The number of observations used in the synthetic control and interactive fixed effects specifications equals the number of PUMAs (45) times the number of quarters included in this analysis (34). However, note that some of these PUMAs receive zero weight in the synthetic control results.***, **, and * denote statistically significance using a two-tailed test with p ≤ 0.01, 0.05, and 0.10, respectively.

Table 5: Main Results: Effect on Wages of Low-Wage Jobs

Quarter	Quarters after Passage/ Enforcement	Synthetic Control	Interactive FE
2014.3	1	0.003 (0.003)	0.003 (0.003)
2014.4	2	0.003 (0.003)	0.006** (0.003)
2015.1	3	0.005 (0.004)	0.007*** (0.003)
2015.2	4/1	0.014*** (0.004)	0.014*** (0.003)
2015.3	5/2	0.019*** (0.005)	0.019*** (0.004)
2015.4	6/3	0.018*** (0.004)	0.018*** (0.004)
2016.1	7/4	0.031*** (0.005)	0.028*** (0.005)
2016.2	8/5	0.033*** (0.006)	0.029*** (0.005)
2016.3	9/6	0.036*** (0.007)	0.031*** (0.006)

Notes: n=1,890. Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. Estimates for all jobs paying < $19 in all industries, where the control region is defined as the state of Washington excluding King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Table 6: Main Results: Effect on Low-Wage Employment

Quarter	Quarters since Passage/ Enforcement	Hours SC	Hours IFE	Jobs SC	Jobs IFE
2014.3	1	0.008	0.004	0.004	-0.006
		(0.018)	(0.013)	(0.017)	(0.015)
2014.4	2	0.003	-0.001	-0.010	-0.023
		(0.018)	(0.013)	(0.021)	(0.015)
2015.1	3	-0.023	-0.018	0.000	-0.013
		(0.018)	(0.013)	(0.023)	(0.015)
2015.2	4/1	-0.013	-0.014	-0.014	-0.032**
		(0.019)	(0.014)	(0.019)	(0.015)
2015.3	5/2	-0.034	-0.022	-0.019	-0.035*
		(0.025)	(0.020)	(0.021)	(0.021)
2015.4	6/3	-0.021	-0.009	-0.045	-0.048***
		(0.033)	(0.019)	(0.029)	(0.020)
2016.1	7/4	-0.106***	-0.090***	-0.051*	-0.053***
		(0.031)	(0.024)	(0.028)	(0.021)
2016.2	8/5	-0.087***	-0.079***	-0.052*	-0.083***
		(0.031)	(0.027)	(0.028)	(0.020)
2016.3	9/6	-0.102***	-0.100***	-0.063*	-0.106***
		(0.042)	(0.034)	(0.036)	(0.024)

Notes: Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. N=1,890. Estimates for all jobs paying < $19 in all industries, where the control region is defined as the state of Washington excluding King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

***, **, and * denote statistically significance using a two-tailed test with p ≤ 0.01, 0.05, and 0.10, respectively.

Table 7: Main Results: Effect on Payroll for Low-Wage Jobs

Quarter	Quarters since passage/ enforcement	Synthetic Control	Interactive Fixed Effects
2014.3	1	0.011 (0.018)	0.010 (0.013)
2014.4	2	0.008 (0.018)	0.003 (0.013)
2015.1	3	-0.016 (0.019)	-0.014 (0.014)
2015.2	4/1	0.002 (0.019)	0.002 (0.014)
2015.3	5/2	-0.013 (0.025)	0.004 (0.020)
2015.4	6/3	-0.002 (0.034)	0.011 (0.019)
2016.1	7/4	-0.076*** (0.034)	-0.054* (0.029)
2016.2	8/5	-0.053 (0.032)	-0.040 (0.031)
2016.3	9/6	-0.065 (0.044)	-0.060 (0.038)

Notes: n=1,890. Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. Estimates for all jobs paying < $19 in all industries, where the control region is defined as the state of Washington excluding King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Table 8: Estimates of the Elasticity of Labor Demand with respect to Minimum Wages

Quarter	Quarters after Passage/ Enforcement	Denominator is synthetic control estimated wage effect		Denominator is statutory increase in minimum wage	
		Point Estimate	95% Conf. Int.	Point Estimate	95% Conf. Int.
2015.2	4/1	-0.97	(-3.75, 1.81)	-0.08	(-0.32, 0.15)
2015.3	5/2	-1.80	(-4.49, 0.90)	-0.21	(-0.51, 0.09)
2015.4	6/3	-1.16	(-4.81, 2.50)	-0.13	(-0.53, 0.27)
2016.1	7/4	-3.46	(-5.87, -1.04)	-0.28	(-0.45, -0.12)
2016.2	8/5	-2.66	(-4.79, -0.54)	-0.23	(-0.40, -0.07)
2016.3	9/6	-2.82	(-5.38, -0.27)	-0.27	(-0.50, -0.05)

Notes: Confidence interval based on permutation inference. Estimates for all jobs paying < $19 in all industries, where the control region is defined as the state of Washington excluding King County. % Δ Min. Wage is defined as ($11 - $9.47)/$9.47 for quarters 1-3 after enforcement, and as ($13 - $9.47)/$9.47 for quarters 4-6 after enforcement.

Table 9 : Effect of Restricting Analysis to Food Service and Drinking Places

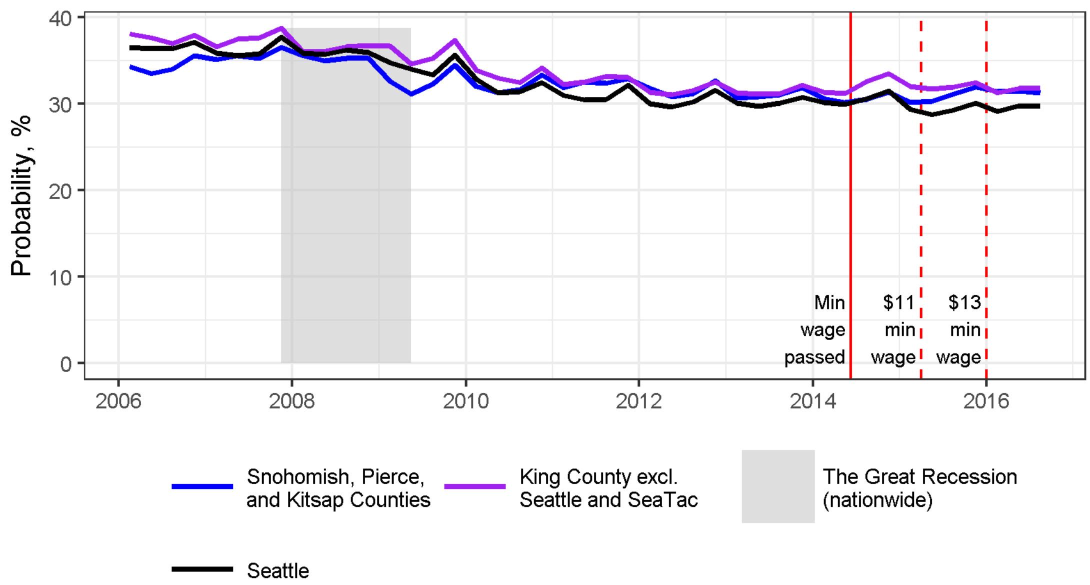
		All industries	Restaurant Industry (NAICS 722)			
	Quarter since Passage/ Enforcement	Wages under $19	Wages under $19		All wage levels	
Quarter		Hours	Hours	Jobs	Jobs	Hours
2014.3	1	0.008	-0.008	0.039	0.038	-0.008
		(0.018)	(0.030)	(0.030)	(0.029)	(0.029)
2014.4	2	0.003	-0.008	-0.006	0.035	0.009
		(0.018)	(0.031)	(0.038)	(0.037)	(0.030)
2015.1	3	-0.023	-0.022	-0.005	-0.001	-0.008
		(0.018)	(0.043)	(0.039)	(0.038)	(0.039)
2015.2	4/1	-0.013	-0.040	-0.033	0.008	-0.003
		(0.019)	(0.038)	(0.038)	(0.036)	(0.038)
2015.3	5/2	-0.034	-0.071	-0.019	0.031	-0.027
		(0.025)	(0.050)	(0.049)	(0.051)	(0.052)
2015.4	6/3	-0.021	-0.036	-0.077*	0.002	0.023
		(0.033)	(0.054)	(0.047)	(0.048)	(0.056)
2016.1	7/4	-0.106***	-0.101*	-0.110**	-0.016	-0.005
		(0.031)	(0.059)	(0.052)	(0.057)	(0.069)
2016.2	8/5	-0.087***	-0.099*	-0.122**	0.031	0.006
		(0.031)	(0.060)	(0.058)	(0.066)	(0.070)
2016.3	9/6	-0.102***	-0.102	-0.105*	-0.004	-0.024
		(0.042)	(0.066)	(0.056)	(0.067)	(0.078)

Notes: n=1,890. Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control. The control region is defined as the state of Washington excluding King County. Estimates using Synthetic Control reported. NAICS 722 = Food services and drinking places. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

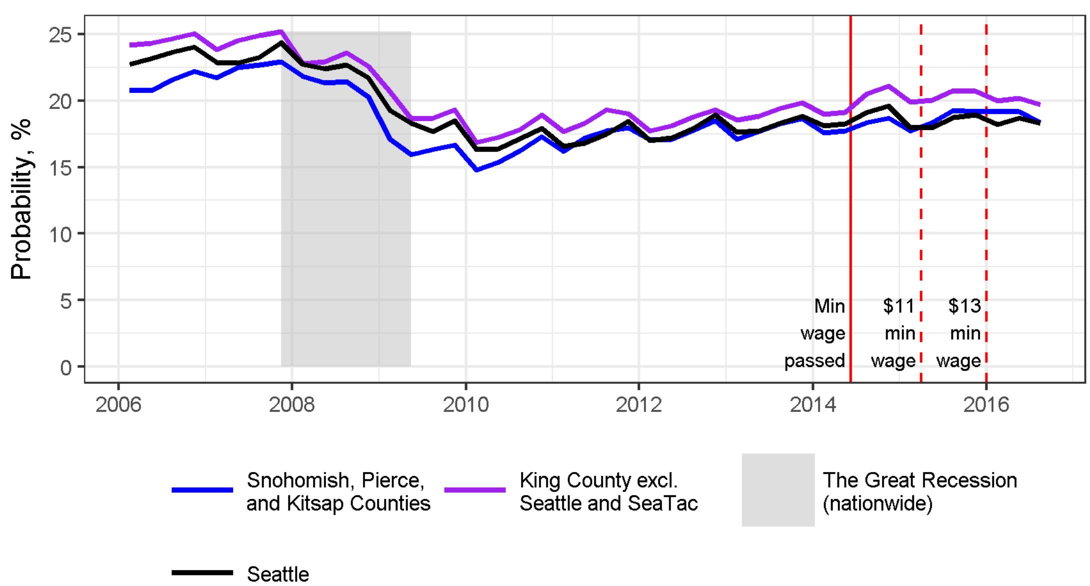
***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Figure 1: Rates of Transition from Locatable to Non-Locatable Employment

Panel A. *P*(non-locatable job in t | locatable and paid under $19/hour in t-4, employed in WA in t) by initial location

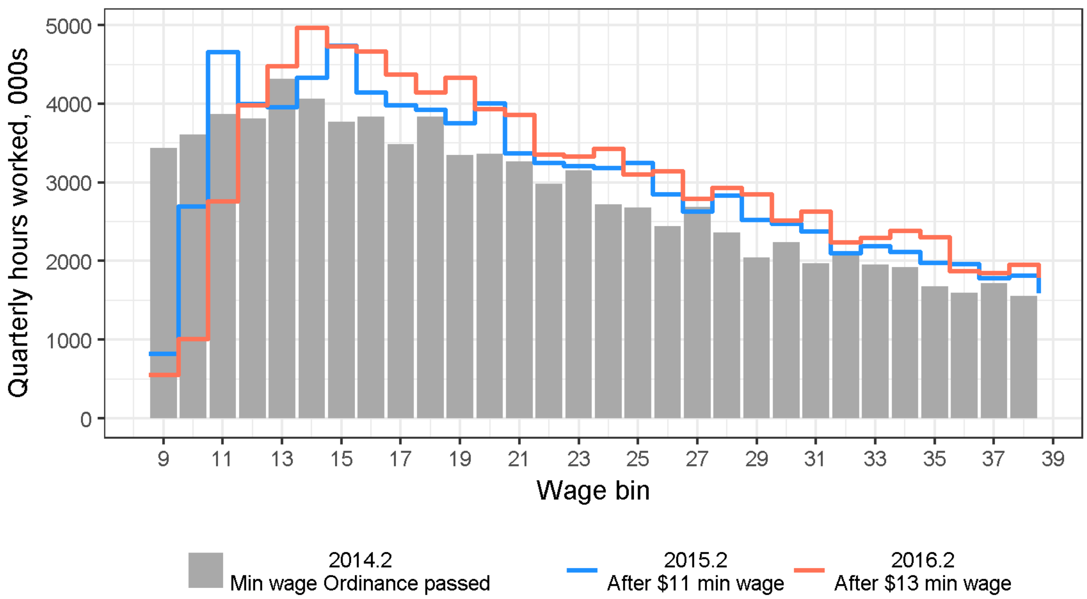


Pane B. *P*(non-locatable job in t | locatable and paid under $19/hour in t-4) by initial location



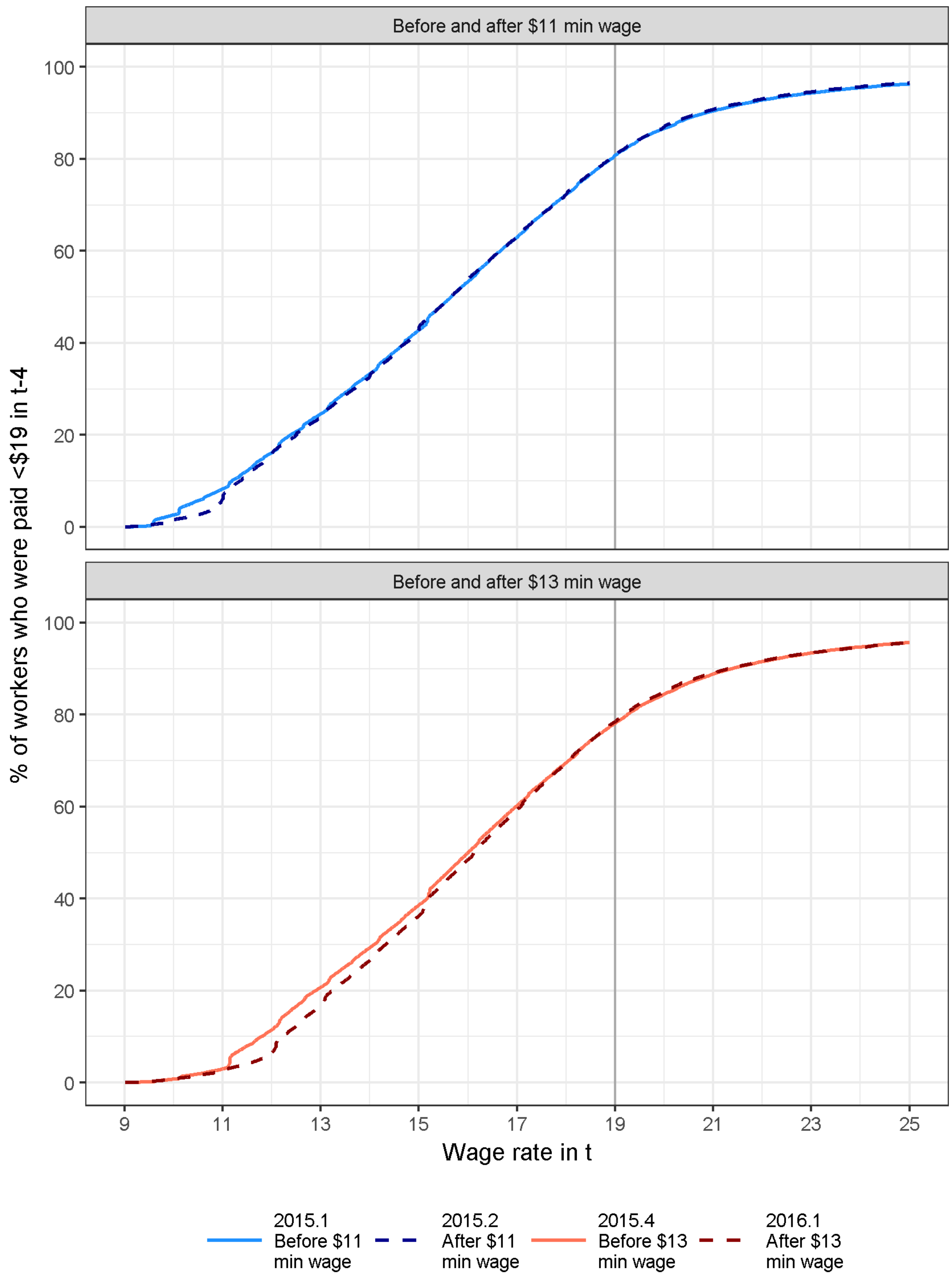
Notes: Non-locatable jobs are defined as those in a non-locatable business anywhere in Washington State. Hourly wages are inflation-adjusted to the 2nd quarter of 2015 using CPI-W.

Figure 2: Changes in the Wage Distribution in Seattle



Notes: Authors calculations based on UI records from State of WA using the sample of jobs in locatable employers in Seattle. Wage rates and earnings are expressed in constant prices of 2015 Q2.

Figure 3: Cumulative Density Function for Wages of Low-wage Workers



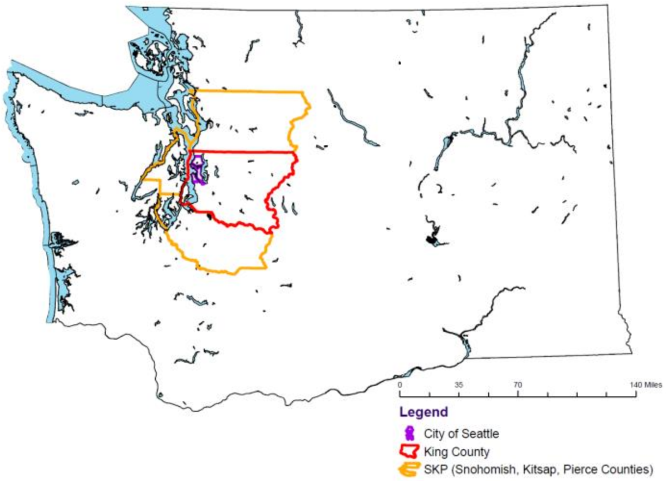
Notes: Workers who were employed in Seattle by locatable establishments in periods t and t-4, and paid less than $19 in t-4.

Figure 4: Geography of Seattle and King, Snohomish, Kitsap, and Pierce Counties

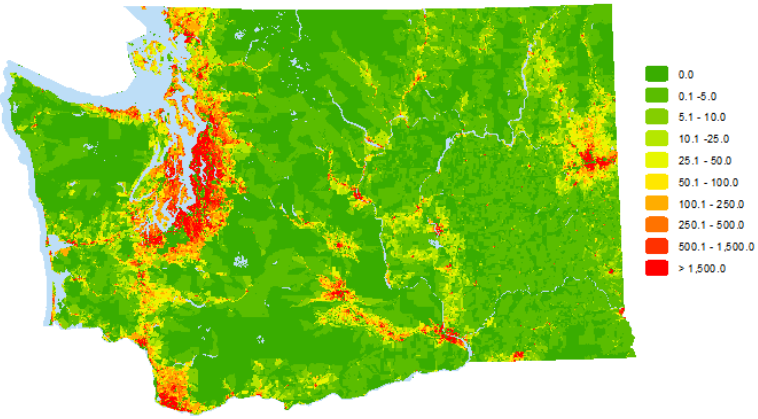
Panel A: Seattle's Water Boundaries

Source: https://www.google.com/maps/

Panel B: Difference-in-Differences Regions



Panel C: Population Density by Census Block, 2010



Source: http://www.ofm.wa.gov/pop/census2010/pl/maps/map05.asp

Figure 5: Geography of Washington's PUMAs

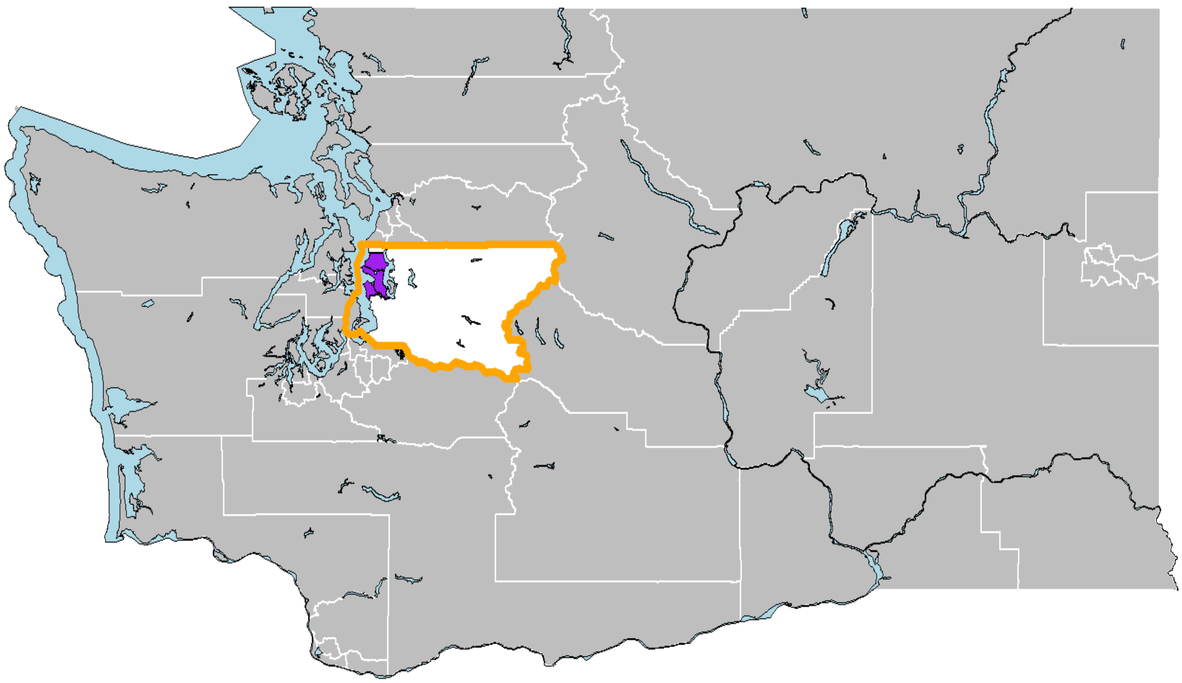
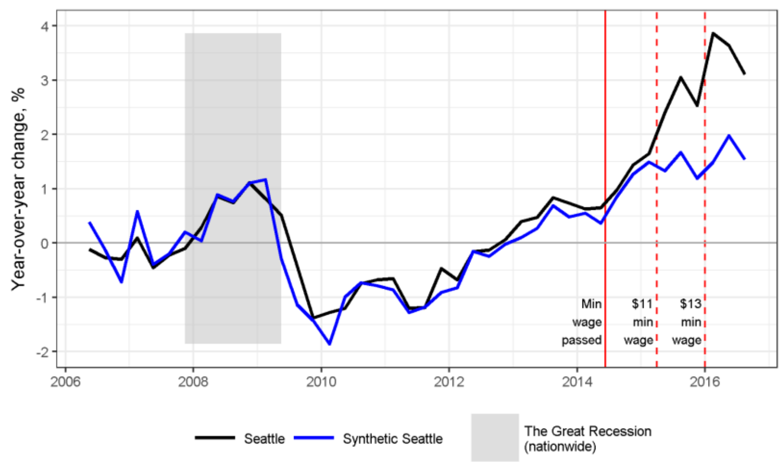
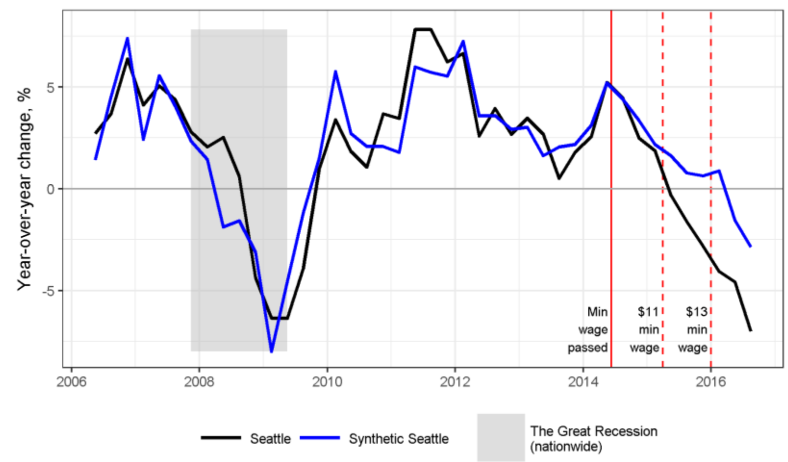


Figure 6: Employment, Wages, and Payroll in Seattle Compared to Synthetic Seattle in Jobs Paying Less than $19 Per Hour

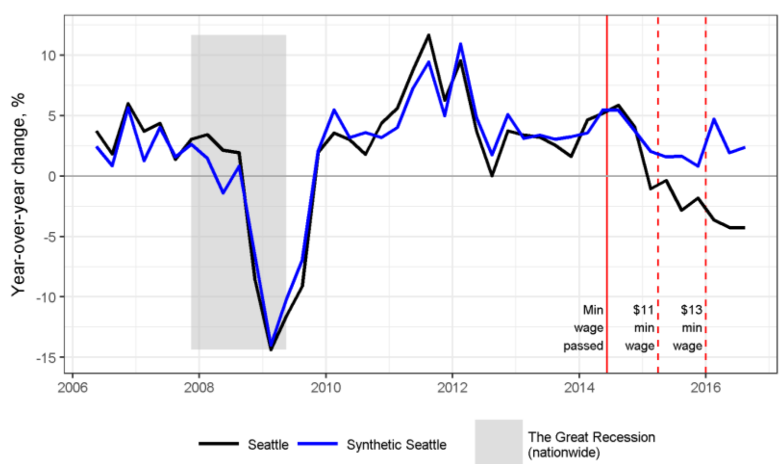
Panel A: Average Wage



Panel C: Number of Jobs



Panel B: Hours Worked



Panel D: Payroll

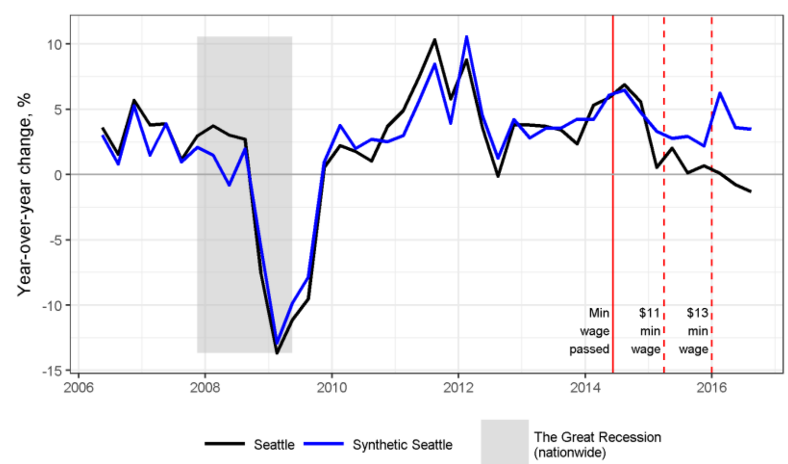
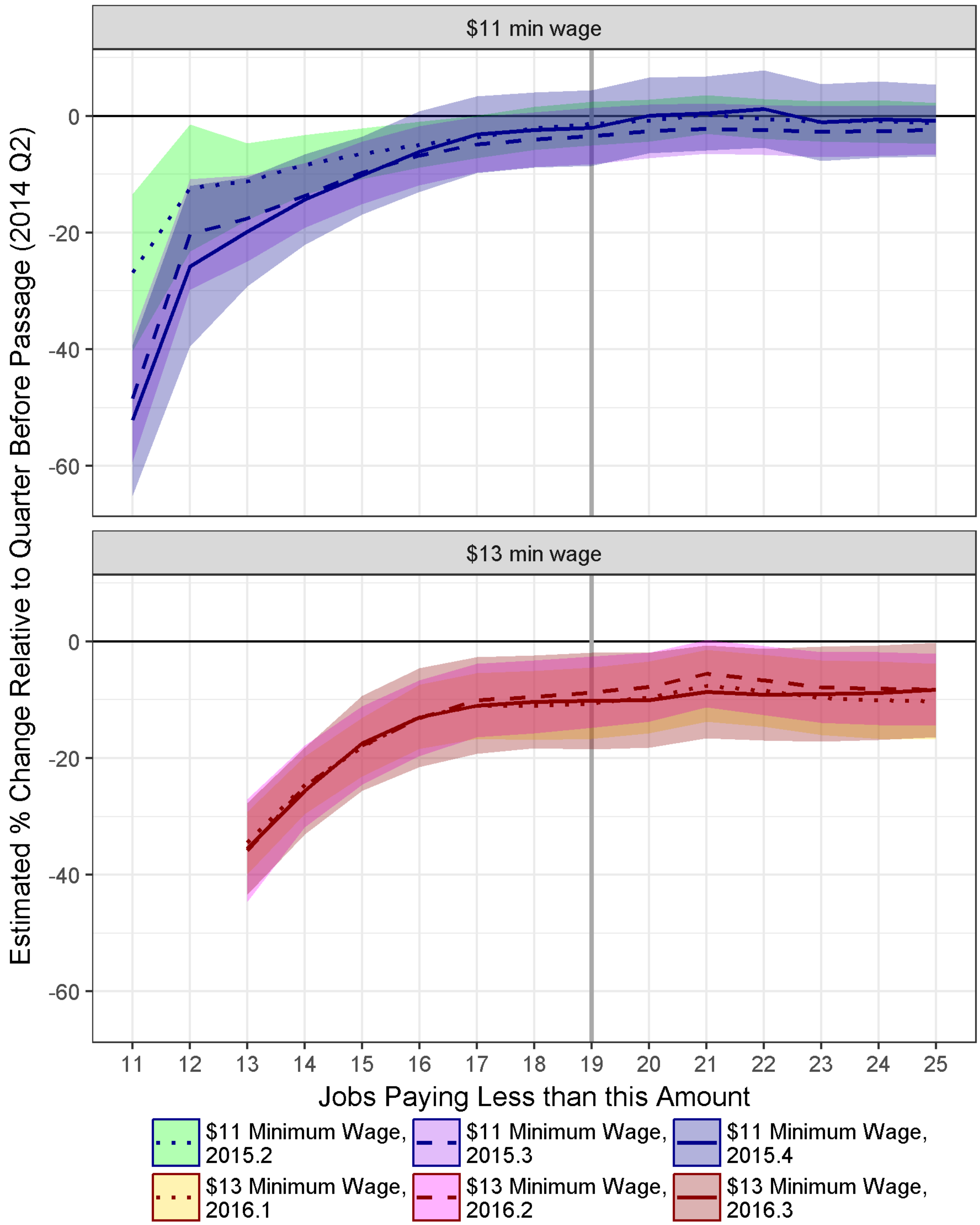
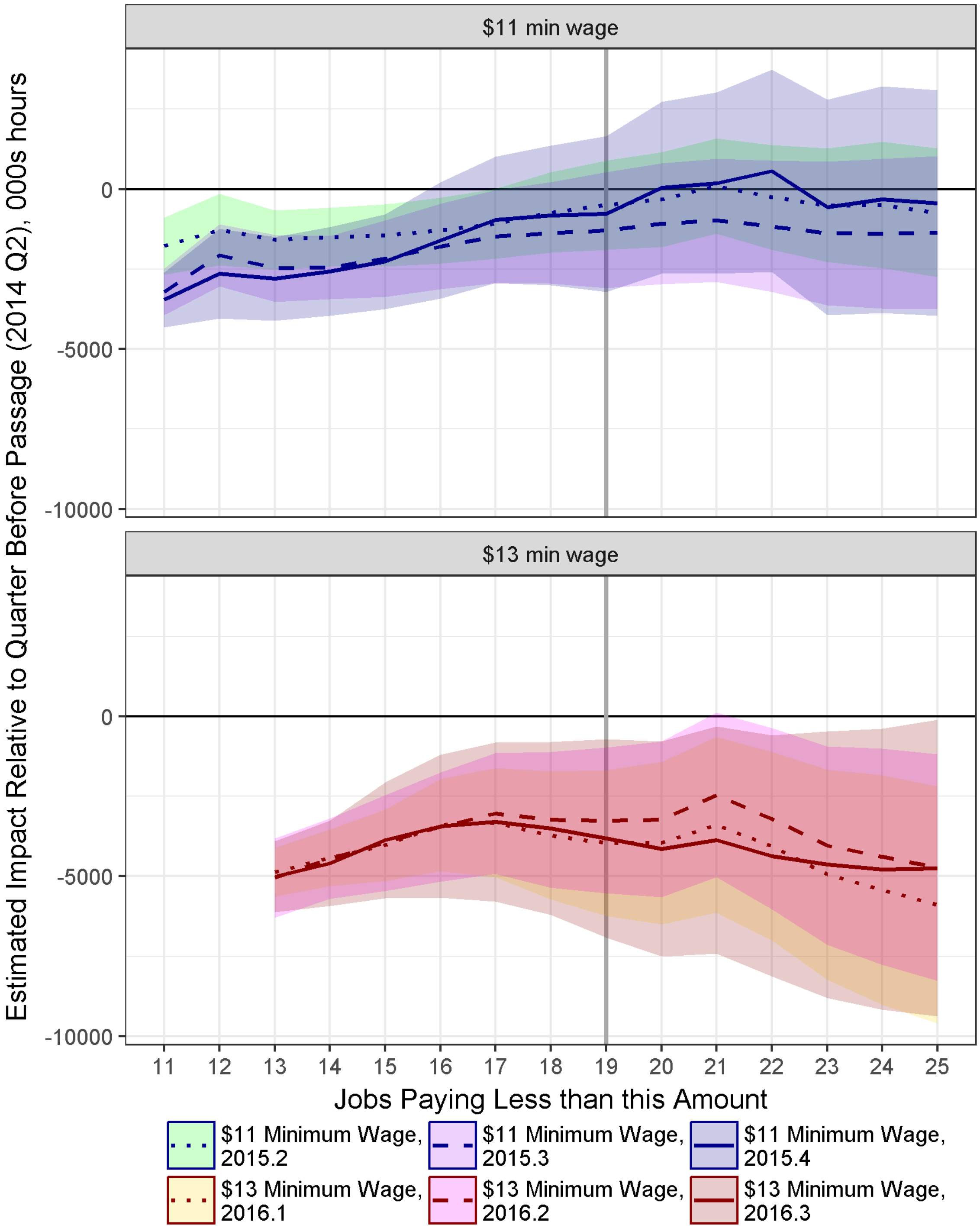


Figure 7: Sensitivity of the Estimated Effects on Percentage Change in Hours Worked Using Different Thresholds



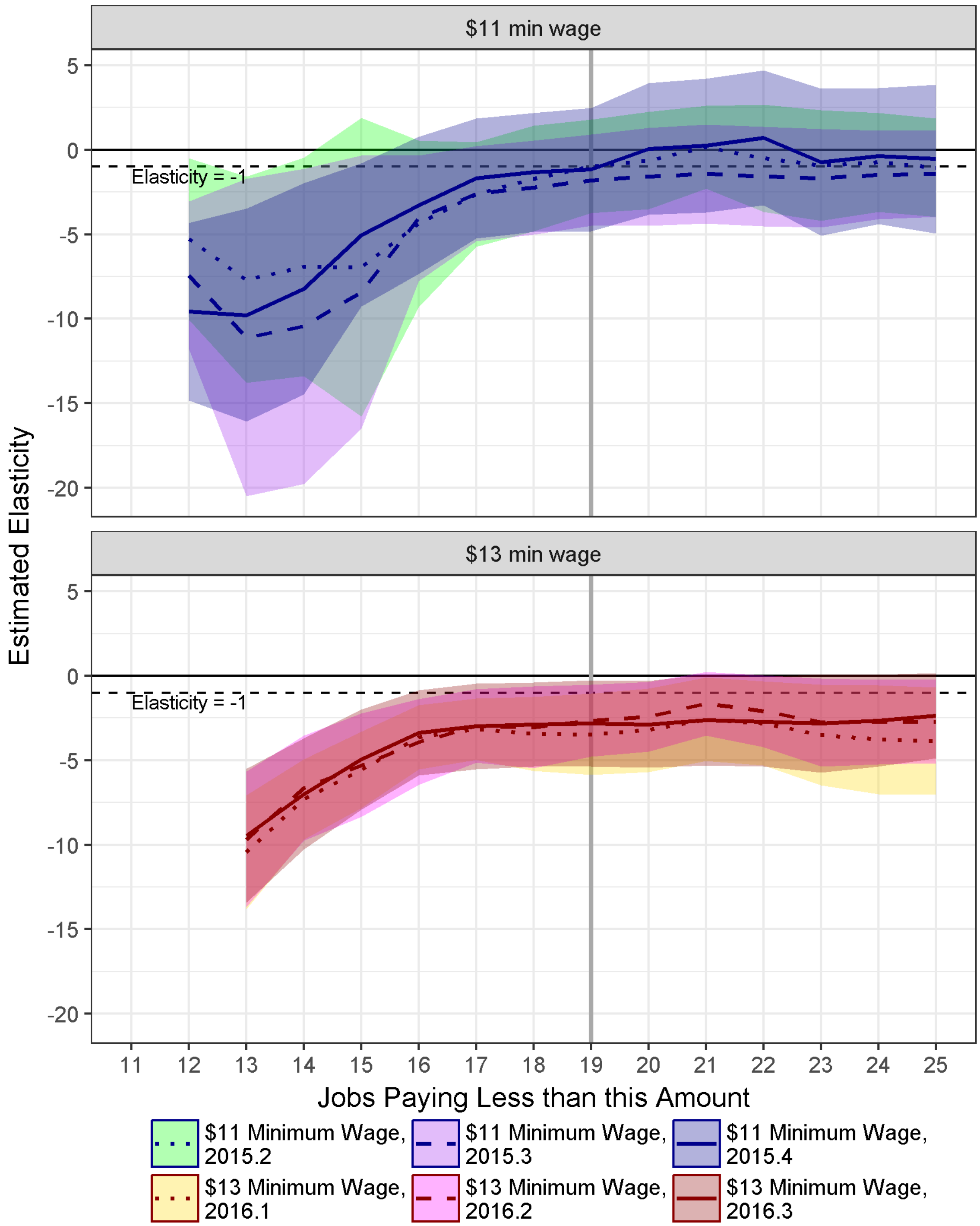
Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

Figure 8: Sensitivity of the Estimated Effects on Total Hours Worked Using Different Thresholds



Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

Figure 9: Sensitivity of the Estimated Elasticity of Labor Demand With Respect to Wages Using Different Thresholds



Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

On-Line Appendix Tables and Figures

Appendix Table 1: Number of Jobs in Seattle's Locatable Establishments, by Industry and Wage Level

Industry (NAICS Sector)	Total Number of Employees			Number of Employees paid <$19 per hour		
	Included in Analysis	Excluded from Analysis	Share Included	Included in Analysis	Excluded from Analysis	Share Included
Agriculture, Forestry, Fishing and Hunting	60,714	20,065	75.2%	50,650	17,053	74.8%
Mining, Quarrying, and Oil and Gas Extraction	1,677	857	66.2%	325	91	78.1%
Utilities	6,777	7,513	47.4%	670	320	67.7%
Construction	130,621	19,380	87.1%	31,720	3,546	89.9%
Manufacturing	146,599	130,360	52.9%	61,200	20,323	75.1%
Wholesale Trade	74,148	45,109	62.2%	26,516	14,746	64.3%
Retail Trade	135,748	173,901	43.8%	85,816	115,401	42.6%
Transportation and Warehousing	47,059	46,900	50.1%	17,915	10,082	64.0%
Information	72,647	31,425	69.8%	7,617	6,734	53.1%
Finance and Insurance	36,354	58,924	38.2%	9,335	16,697	35.9%
Real Estate and Rental and Leasing	31,130	14,672	68.0%	15,741	7,163	68.7%
Professional, Scientific, and Technical Services	117,455	32,765	78.2%	22,423	6,229	78.3%
Management of Companies and Enterprises	3,832	3,798	50.2%	458	1,142	28.6%
Administrative and Support and Waste Management and Remediation Services	96,906	51,992	65.1%	48,732	33,148	59.5%
Educational Services	179,519	62,173	74.3%	57,383	15,665	78.6%
Health Care and Social Assistance	212,455	143,618	59.7%	106,209	66,186	61.6%
Arts, Entertainment, and Recreation	49,248	9,025	84.5%	31,737	5,273	85.8%
Accommodation and Food Services	132,324	79,971	62.3%	106,242	60,561	63.7%
Other Services (except Public Administration)	58,944	19,379	75.3%	31,243	12,882	70.8%
Public Administration	78,291	68,002	53.5%	13,295	11,746	53.1%
Total	1,672,448	1,019,875	62.1%	725,231	425,023	63.0%

Notes: Firms are defined by federal tax Employer Identification Numbers. Statistics are computed for the average quarter between 2005.1 to 2016.3. "Excluded from Analysis" includes two categories of firms: (1) Multi-location firms (flagged as such in UI data), and (2) Single-location firms which operate statewide or whose location could not be determined.

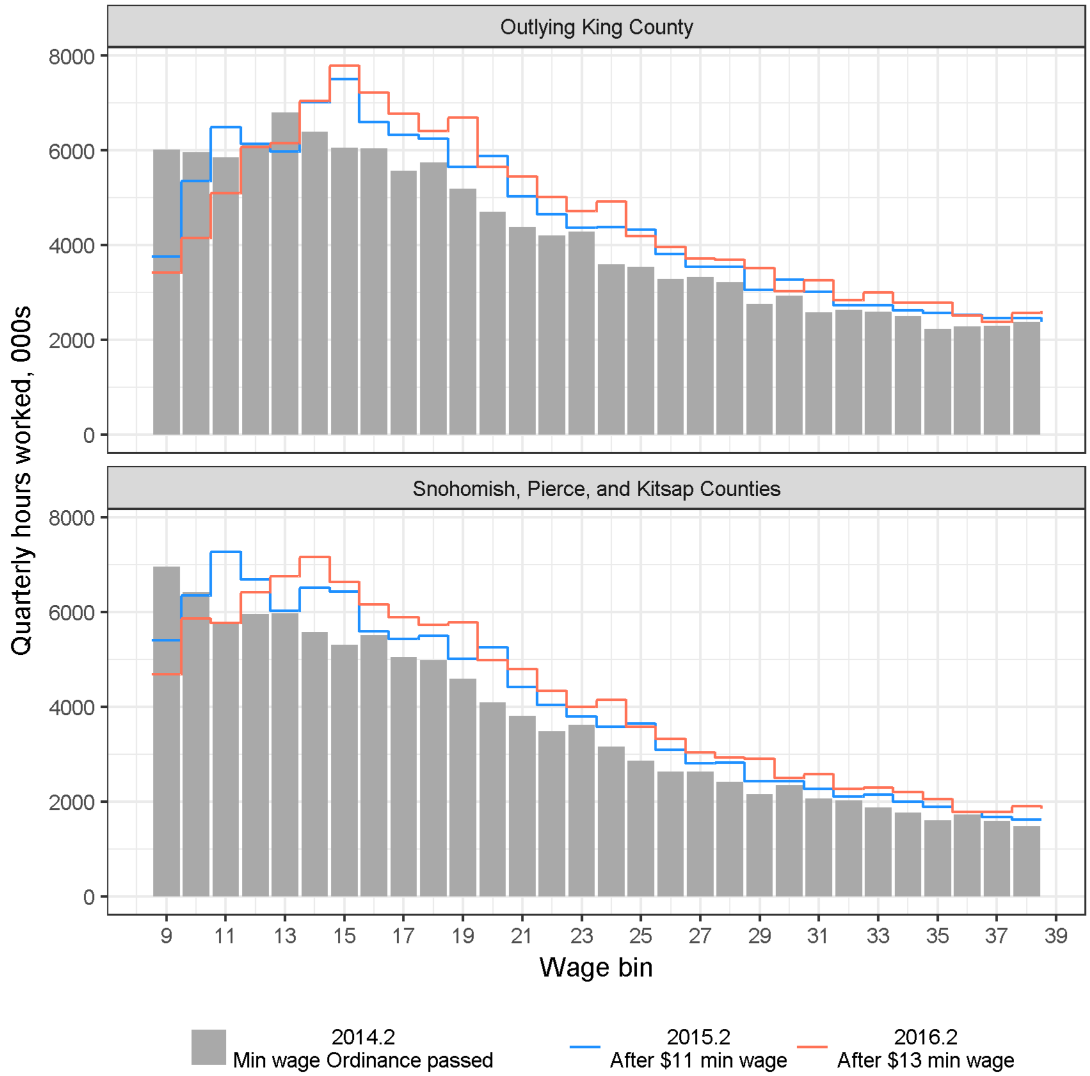
Appendix Table 2: Number of Jobs in Seattle's Locatable Establishments, by Wage Level

		Number of jobs, absolute value						
	Quarters After:	Jobs paying						
Quarter	Passage / Enforcement	Under $13	$13 to $19	$19 to $25	$25 to $30	$30 to $35	$35 to $40	$40 and above
Panel A: Seattle								
2014.2	**0**	39,807	53,152	44,076	27,793	21,848	20,016	85,948
2014.3	**1**	40,706	54,207	43,795	27,375	21,683	19,908	93,218
2014.4	**2**	35,421	54,177	43,494	28,947	22,920	20,685	97,445
2015.1	**3**	35,085	55,728	43,341	28,919	23,102	20,891	98,163
2015.2	**4/1**	35,075	57,593	45,609	30,085	23,920	19,192	100,412
2015.3	**5/2**	33,959	59,423	45,208	30,140	23,889	21,355	106,833
2015.4	**6/3**	30,002	57,065	44,548	30,547	24,154	22,310	111,569
2016.1	**7/4**	24,662	62,460	45,794	30,730	24,585	22,158	110,971
2016.2	**8/5**	24,420	64,011	49,437	32,155	25,670	22,800	113,434
2016.3	**9/6**	23,232	63,610	49,047	31,277	24,816	23,059	121,476
Panel B: Washington State (including Seattle)								
2014.2	**0**	458,807	434,216	307,615	174,202	130,385	108,336	401,680
2014.3	**1**	481,075	431,208	307,262	177,187	130,441	104,748	440,004
2014.4	**2**	431,551	451,306	312,764	188,893	139,294	114,271	439,626
2015.1	**3**	433,749	441,660	304,120	184,817	136,687	113,934	432,791
2015.2	**4/1**	434,072	461,186	317,136	186,442	137,569	110,101	444,056
2015.3	**5/2**	441,220	461,944	315,665	191,594	139,622	111,502	492,744
2015.4	**6/3**	400,306	472,108	319,016	196,468	144,892	118,198	486,026
2016.1	**7/4**	392,573	470,059	314,359	193,384	142,870	116,854	464,950
2016.2	**8/5**	370,939	478,860	338,816	192,767	144,546	118,098	480,613
2016.3	**9/6**	370,333	466,528	327,986	191,790	141,932	114,350	516,659

Appendix Table 3: Elasticity Estimates from Selected Literature

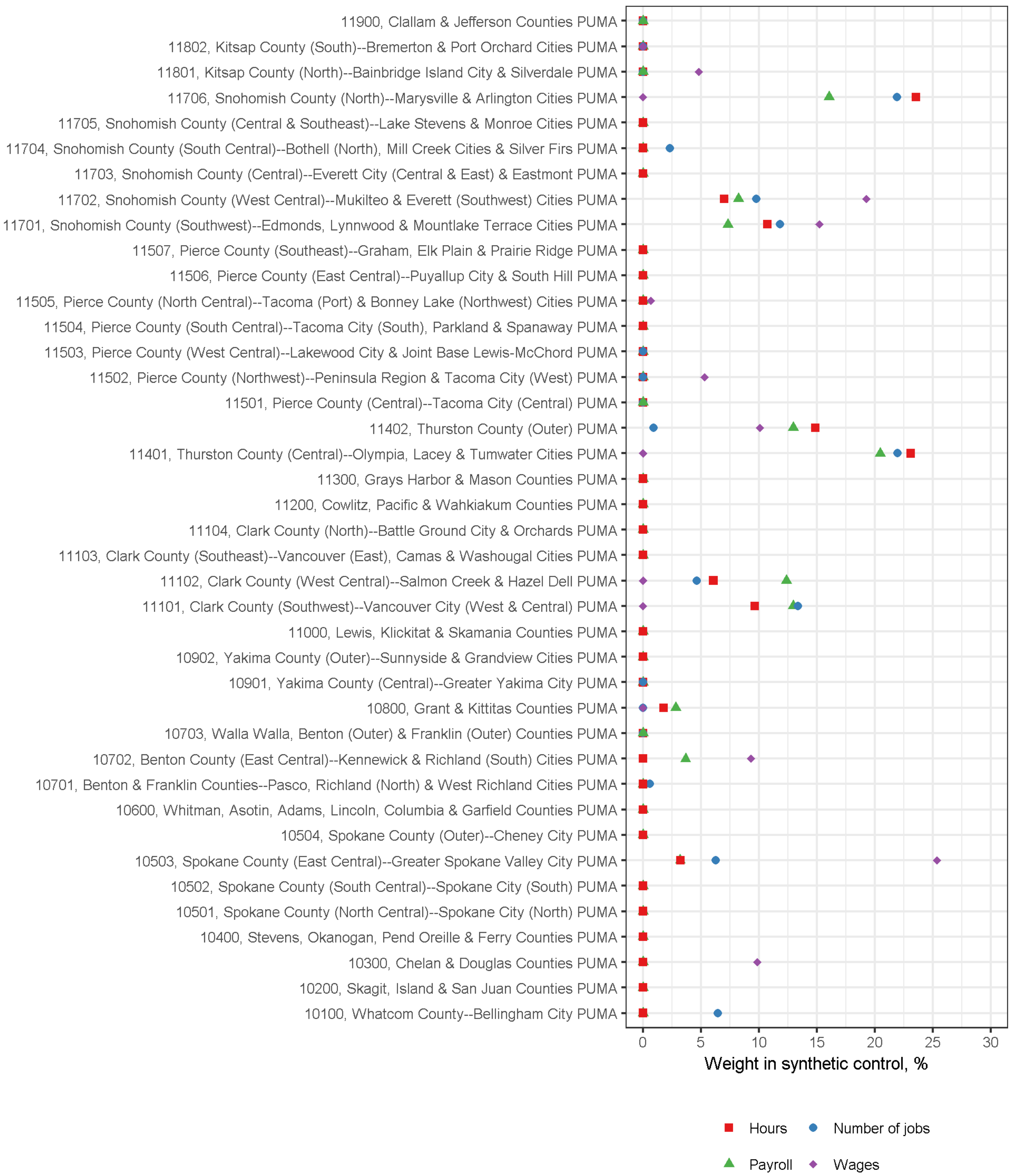
Level of Government	Industry and Outcome	Years	Method	Elasticity
State	Restaurant Employment All Jobs	1990-2010	Interactive FE	-0.04
			Common Correlated Effects-Pooled Estimator	-0.01
			Common Correlated Effects-Mean Group Estimator	-0.01
State	Restaurant Employment All Jobs	2000-2011	DnD (State and Time FE)	-0.12
			Synthetic Matching Estimator	-0.06
State	Restaurant Employment All Jobs	1990-2006	DnD (Census division-by-period fixed effects and County FE)	-0.02
			+ State linear trend	-0.04
			Contiguous Border County Pair Sample (County and Quarter FE)	-0.11
			Contiguous Border County Pair Sample (County-pair × period FE)	0.02
State	Restaurant Employment All Jobs	2000-2011	DnD (County and Quarter FE)	-0.07
			DnD (Contiguous County-Pair Quarter FE + County FE)	-0.02
State	Restaurant Employment All Jobs	1990-2005	DnD (County and Quarter Fixed Effects)	-0.10
			+ Linear County Trends	-0.01
			+ Quadratic County Trends	-0.05
			+ Cubic County Trends	-0.04
			+ Quartic County Trends	-0.06
			+ Fifth-order County Trends	-0.05
		1990-2012	DnD (County and Quarter FE)	0.00
			+ Linear County Trends	-0.04
			+ Quadratic County Trends	-0.02
			+ Cubic County Trends	-0.04
			+ Quartic County Trends	-0.02
			+ Fifth-order County Trends	-0.01
State	Restaurant Employment All Jobs	1990-2014	DnD relative to All Counties (County and Quarter FE)	-0.24
			DnD Contiguous Border County Pair with (County and Quarter FE)	-0.18
			DnD Contiguous Border County Pair with (County-pair × Quarter FE)	0.02
			Unweighted Average	-0.05
			Unweighted Standard Deviation	0.06

Appendix Figure 1: Changes in the Wage Distribution in Outlying King County and Snohomish, Pierce, and Kitsap Counties.



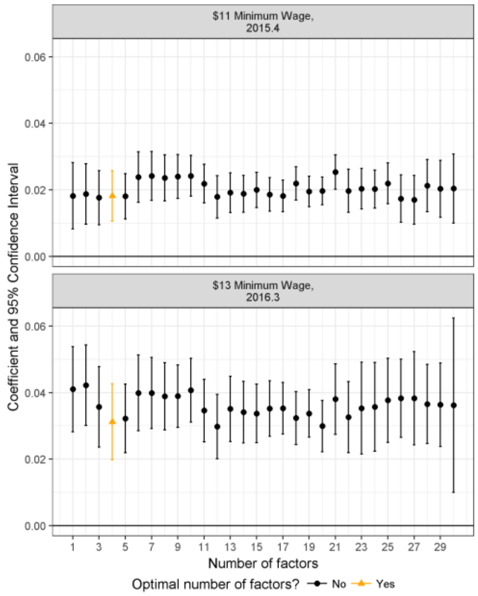
Notes: Authors calculations based on UI records from State of WA using the sample of jobs in locatable employers. Wage rates and earnings are expressed in constant prices of 2015 Q2.

Appendix Figure 2: Weights Chosen by Synthetic Control Estimator, by Outcome.

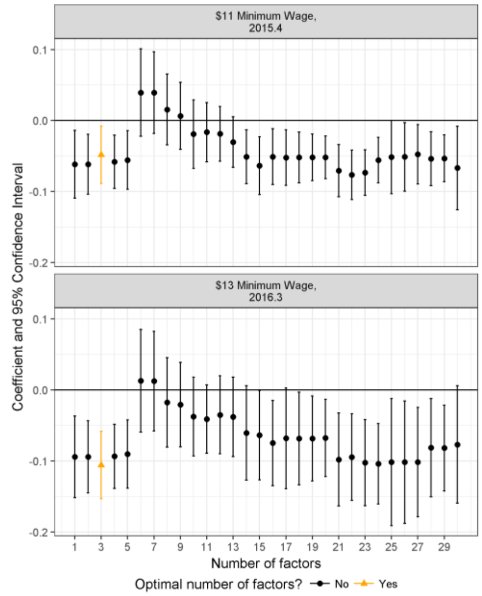


Appendix Figure 3: Sensitivity of the Interactive Fixed Effects Estimates to the Number of Factors Used

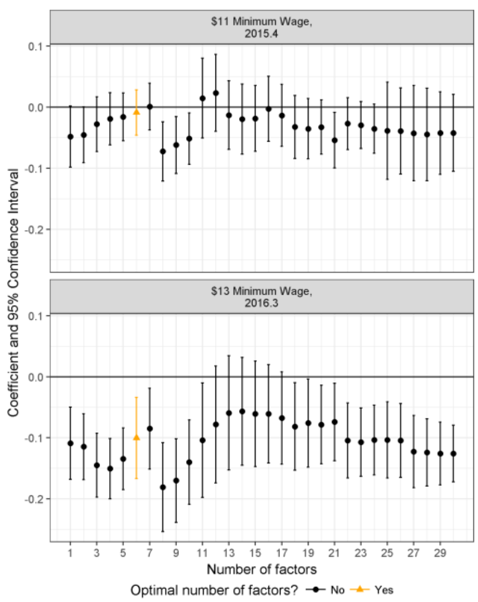
Panel A: Average Wage, Jobs paying <$19 per hour



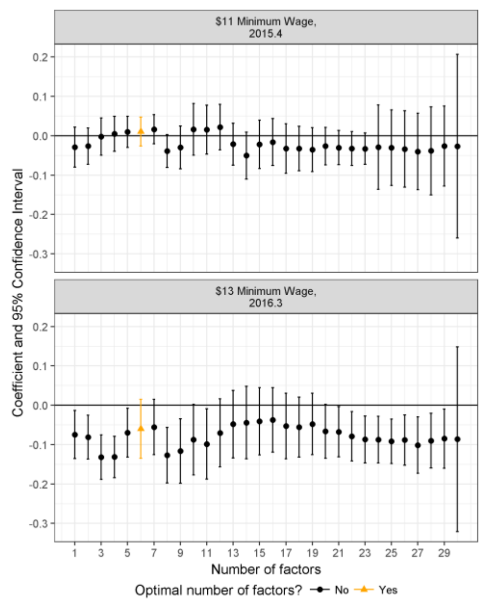
Panel C: Number of Jobs, Jobs paying <$19 per hour



Panel B: Hours Worked, Jobs paying <$19 per hour

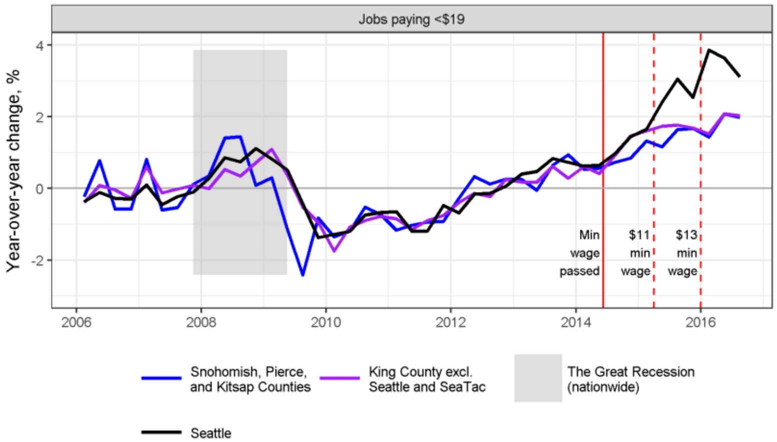


Panel D: Payroll, Jobs paying <$19 per hour

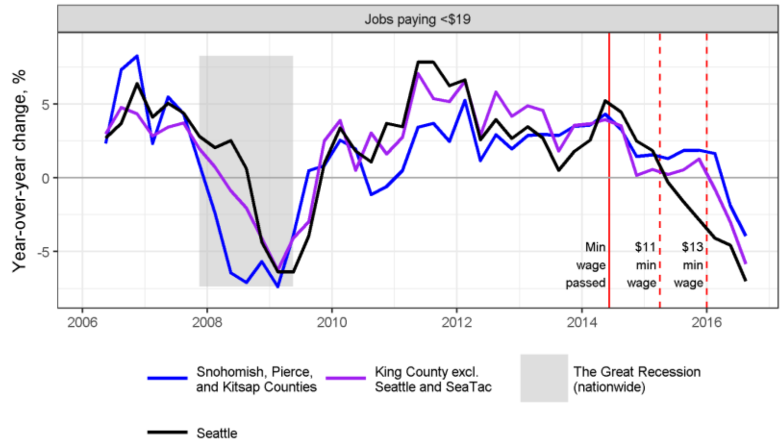


Appendix Figure 4: Employment, Wages, and Payroll in Seattle Compared to Outlying King County and Snohomish, Kitsap, and Pierce Counties

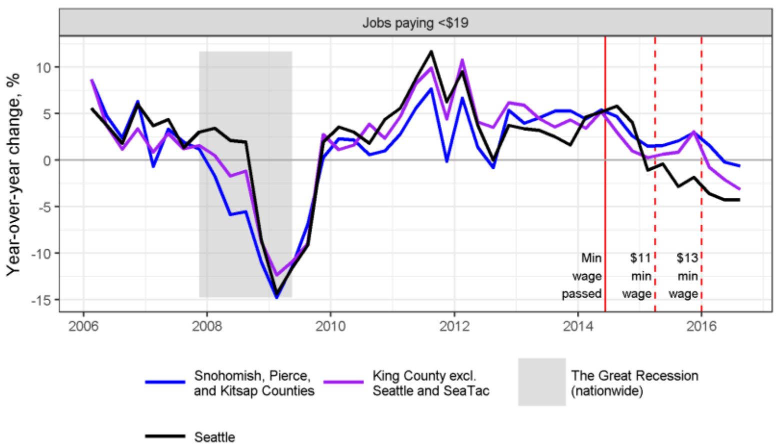
Panel A: Average Wage



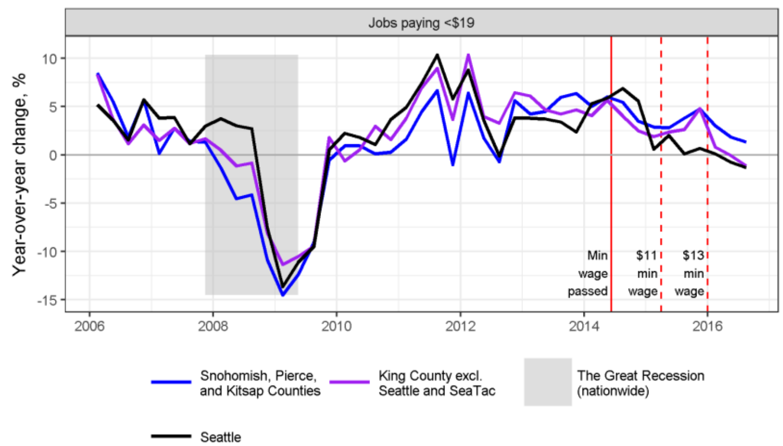
Panel C: Number of Jobs



Panel B: Hours Worked



Panel D: Payroll



NATIONAL CONFERENCE of STATE LEGISLATURES

State Minimum Wages | 2019 Minimum Wage by State

1/7/2019

The table below reflects current state minimum wages in effect as of January 1, 2019, as well as future enacted increases.

Summary

2019 Highlights

- Eighteen states began the new year with higher minimum wages. Eight states (Alaska, Florida, Minnesota, Montana, New Jersey, Ohio, South Dakota, and Vermont) automatically increased their rates based on the cost of living, while ten states (Arizona, Arkansas, California, Colorado, Maine, Massacusetts, Missouri, New York, Rhode Island and Washington) increased their rates due to previously approved legislation or ballot initiatives. Other states that will see rate increases during the 2019 calendar year include: D.C., Delaware, Michigan, and Oregon.
- New Jersey enacted AB 15 in February, which will greadually increase the minimum wage rate to $15 by 2024. (The minimum wage for tipped employees will increase to $9.87 over the same period.) The schedule of annual increases was delayed for certain seasonal workers and employees of small employers, and a training wage of 90 percent of the minimum wage was created for cetain employees for their first 120 hours of work.
- Illinois enacted SB 1 in February, which will phase in a minimum wage increase to $15 by 2025. The measure also adjusted the youth wage for workers under age 18 (it will gradually increase to $13.00 by 2025) and created an tax credit program to offset labor cost increases for smaller employers.

2018 Highlights

- Eighteen states began the new year with higher minimum wages. Eight states (Alaska, Florida, Minnesota, Missouri, Montana, New Jersey, Ohio, and South Dakota) automatically increased their rates based on the cost of living, while eleven states (Arizona, California, Colorado, Hawaii, Maine, Michigan, New York, Rhode Island, Vermont and Washington) increased their rates due to previously approved legislation or ballot initiatives.
- Massachusetts enacted a measure (HB 4640) to increase the state minimum wage to $15.00 over five years. The tipped wage would rise to $6.75 from $3.75 over the same time period.
- Delaware enacted SB 170, which phases in a two-step increase. The rate rises from $8.25 to $8.75 effective January 1, 2019 (as amended by HB 483), and will increase again to $9.25 effective October 1, 2019.
- Voters in Arkansas and Missouri approved ballot initiatives phasing in increases to $11.00 and $12.00 per hour, respectively.
- The Michigan legislature enacted SB 1171, which raises the minimum wage on an annual basis until it reaches $12.05 in 2030.

2017 Highlights

- Nineteen states began 2017 with higher minimum wages. Seven states (Alaska, Florida, Missouri, Montana, New Jersey, Ohio and South Dakota) automatically increased their rates based on the cost of living, five states (Arizona, Arkansas, Colorado, Maine and Washington) increased their rates through ballot initiatives previously approved by voters, and seven states (California, Connecticut, Hawaii, Massachusetts, Michigan, New York and Vermont) did so as a result of legislation passed in prior sessions. Washington D.C., Maryland and Oregon raised their respective minimum wages on July 1, 2017 due to previously enacted legislation.
- Rhode Island was the only state to enact a minimum wage increase during 2017 legislative sessions.

2016 Highlights

- Voters in Arizona, Colorado, Maine, and Washington approved November ballot measures to raise their respective minimum wages. Arizona, Colorado, and Maine will incrementally increase their minimum wages to $12 an hour by 2020. Washington's will be increased incrementally to $13.50 an hour by 2020.

- New York became the second state to pass a new law that would raise the minimum wage in New York City to $15 per hour by the end of 2018. Washington D.C. followed suit, enacting a law to raise the minimum wage in the District to $15 per hour by July 1, 2020.
- On April 4, California Governor Jerry Brown signed Senate Bill 3 into law. The new law increases the minimum wage to $15 per hour by Jan. 1, 2022, for employers with 26 or more employees. For employers with 25 or fewer employees the minimum wage will reach $15 per hour by Jan. 1, 2023. Increases may be paused by the governor if certain economic or budgetary conditions exist. Beginning the first Jan. 1 after the minimum wage reaches $15 per hour for smaller employers, the minimum wage is indexed annually for inflation.
- On March 23, Governor Kenneth Mapp of the Virgin Islands signed Act 7856, establishing an $8.35 minimum wage with scheduled annual increases on June 1, 2017 and 2018 until the rate reaches $10.50.
- On March 2, Oregon Governor Kate Brown signed SB 1532 into law. It establishes a series of annual minimum wage increases from July 1, 2016 through July 1, 2022. Beginning July 1, 2023, the minimum wage rate will be indexed to inflation based on the Consumer Price Index.
- Fourteen states begin the new year with higher minimum wages. Of those, 12 states increased their rates through legislation passed in the 2014 or 2015 sessions, while two states automatically increased their rates based on the cost of living.
- Of the 11 states that currently tie increases to the cost of living, eight did not increase their minimum wage rates for 2016. Colorado provided for an 8-cent increase and South Dakota granted a 5-cent increase per hour. Increases in Nevada are required to take effect in July.
- Maryland, Minnesota and D.C. have additional increases scheduled for 2016. Nevada will announce in July whether or not there will be a cost of living increase to their indexed minimum wage.

2015 Highlights

- The Rhode Island legislature enacted an increase, taking the state minimum wage to $9.60 effective Jan. 1, 2016. (HB 5074 / S194)
- The increases D.C. and Maryland passed during the 2014 session take effect July 1, 2015. D.C.'s new wage of $10.50 an hour makes it the first jurisdiction to cross the $10 threshold among the states. Maryland's minimum wage rose to $8.25 on July 1.
- Delaware also passed an increase in 2014, which took effect June 1, 2015, increasing the state's minimum wage to $8.25 an hour.

2014 highlights

- Lawmakers in Connecticut, Delaware, Hawaii, Maryland, Massachusetts, Michigan, Minnesota, Rhode Island, Vermont, West Virginia and D.C. enacted increases during the 2014 session.
- Voters in Alaska, Arkansas, Nebraska and South Dakota approved minimum wage increases through ballot measures.

Currently, 29 states and D.C. have minimum wages above the federal minimum wage of $7.25 per hour.

Five states have not adopted a state minimum wage: Alabama, Louisiana, Mississippi, South Carolina and Tennessee. New Hampshire repealed their state minimum wage in 2011 but adopted the federal minimum wage by reference.

State Legislation

- Minimum wage legislation database
- Blog: Minimum Wage Developments (August 2018)

STATE MINIMUM WAGE LEGISLATION

State	Minimum Wage	Future Enacted Increases	Indexed Automatic Annual Adjustments
Alabama	none		
Alaska	$9.89		Indexed annual increases begin Jan. 1, 2017. (2014 ballot measure)

State	Minimum Wage	Future Enacted Increases	Indexed Automatic Annual Adjustments
American Samoa	varies [1]		
Arizona	$11.00	$12.00 eff. 1-1-20	Rate increased annually based on cost of living beginning Jan. 2021 (2016 ballot measure)
Arkansas	$9.25	$10.00 eff. 1-1-20 $11.00 eff. 1-1-21	
California[2]	$12.00	$13.00 eff. 1-1-20 $14.00 eff. 1-1-21 $15.00 eff. 1-1-22	Indexed annual increases based on CPI begin Jan. 1, 2023
Colorado	$11.10	$12.00 eff. 1-1-20	Rate increased annually based on cost of living beginning Jan. 1 2021 (2016 ballot measure)
Connecticut	$10.10[3]		
Delaware	$8.75	$9.25 eff. 10-1-19	
D.C.	$13.25	$14.00 eff. 7-1-19 $15.00 eff. 7-1-20	Indexed annual increases based on CPI begin July 1, 2021
Florida	$8.46		Annual increase based cost of living. (Constitutional amendment 2004)
Georgia	$5.15		
Guam	$8.25		
Hawaii	$10.10		
Idaho	$7.25		
Illinois	$8.25	$9.25 eff. 1-1-20 $10.00 eff. 7-1-20 $11.00 eff. 1-1-21 $12.00 eff. 1-1-22 $13.00 eff. 1-1-23 $14.00 eff. 1-1-24 $15.00 eff. 1-1-25	

State	Minimum Wage	Future Enacted Increases	Indexed Automatic Annual Adjustments
Indiana	$7.25		
Iowa	$7.25		
Kansas	$7.25		
Kentucky	$7.25		
Louisiana	none		
Maine	$11.00 [5]	$12.00 eff. 1-1-20	Indexed annual increases based on CPI begin Jan 1, 2021
Maryland	$10.10		
Massachusetts	$12.00 [6]	$12.75 eff. 1-1-20 $13.50 eff. 1-1-21 $14.25 eff. 1-1-22 $15.00 eff. 1-1-23	
Michigan	$9.25	$9.45 eff. 3-29-19 $9.65 eff. 2020 $9.87 eff. 2021 $10.10 eff. 2022 $10.33 eff. 2023 $10.56 eff. 2024 $10.80 eff. 2025 $11.04 eff. 2026 $11.29 eff. 2027 $11.54 eff. 2028 $11.79 eff. 2029 $12.05 eff. 2030	
Minnesota	$9.86/$8.04 [7]		Indexed annual increases begin Jan. 1, 2018. (2014 legislation)
Mississippi	none		

State	Minimum Wage	Future Enacted Increases	Indexed Automatic Annual Adjustments
Missouri	$8.60 [8]	$8.60 eff. 1-1-19 $9.45 eff. 1-1-20 $10.30 eff. 1-1-21 $11.15 eff. 1-1-22 $12.00 eff. 1-1-23	Minimum wage increased or decreased by cost of living starting Jan. 1, 2024. (2018 ballot measure)
Montana	$8.50/$4.00 [9]		Increases done annually based on the CPI and effective Jan. 1 of the following year. (2006 ballot measure)
Nebraska	$9.00		
Nevada	$8.25/$7.25 [10]		Increases subject to the federal minimum wage and consumer price index. Increases take effect July 1. (Constitutional amendment 2004/2006).
New Hampshire	repealed by HB 133 (2011)		
New Jersey	$8.85 [11]	$10.00 eff. 7-1-19 $11.00 eff. 1-1-20 $12.00 eff. 1-1-21 $13.00 eff. 1-1-22 $14.00 eff. 1-1-23 $15.00 eff. 1-1-24	Indexed annual increases based on the CPI beginning 2025. (2019 legislation)
New Mexico	$7.50		
New York	$11.10[12]	$11.80 eff. 12-31-19 $12.50 eff. 12-31-20 After 12-31-20, the rate is adjusted annually for inflation until it reaches $15.00	
North Carolina	$7.25		
North Dakota	$7.25		
Northern Mariana Islands	$7.25		
Ohio	$8.55/$7.25[13]		Indexed annual increases based on the CPI. (Constitutional amendment 2006)

State	Minimum Wage	Future Enacted Increases	Indexed Automatic Annual Adjustments
Oklahoma	$7.25/$2.00 [14]		
Oregon	$10.75[15]	$11.25 eff. 7-1-19 $12.00 eff. 7-1-20 $12.75 eff. 7-1-21 $13.50 eff. 7-1-22	Indexed annual increases based on the CPI are effective July 1, 2023 (2016 legislation)
Pennsylvania	$7.25		
Puerto Rico	$7.25/$5.08 [16]		
Rhode Island	$10.50		
South Carolina	none		
South Dakota	$9.10		Annual indexed increases begin Jan. 1, 2016. (2014 ballot measure.)
Tennessee	none		
Texas	$7.25		
Utah	$7.25		
Vermont	$10.78		Beginning Jan. 1, 2019, minimum wage increased annually by 5% or the CPI, whichever is smaller; it cannot decrease. Note: Vermont started indexing in 2007 but enacted additional increases in 2014. (2014 legislation)
Virgin Islands	$10.50		
Virginia	$7.25		
Washington	$12.00	$13.50 eff. 1-1-2020	Annual indexed increases began Jan. 1, 2020. (ballot measure 2016)
West Virginia	$8.75		
Wisconsin	$7.25		
Wyoming	$5.15		

Sources: U.S. Dept. of Labor, http://www.dol.gov/esa/minwage/america.htm; and state web sites.

Notes

[1] **American Samoa**: The Fair Minimum Wage Act of 2007 (Public Law 110-28) sets minimum wage rates within American Samoa and provides for additional increases in the minimum wage of $0.50 per hour each year on May 25, until reaching the minimum wage generally applicable in the United States. The wage rates are set for particular industries, not for an employee's particular occupation. The rates are minimum rates; an employer may choose to pay an employee at a rate higher than the rate(s) for its industry.

[2] **California:** The minimum wage scheduled increases are delayed by one year for employers with 25 or fewer employees. The rate increases to $10.50 per hour effective 1/1/2018 and is increased by $1.00 increments annually until it reaches $15.00 effective 1/1/2023

[3] **Connecticut**: The Connecticut minimum wage rate automatically increases to 1/2 of 1 percent above the rate set in the Fair Labor Standards Act if the Federal minimum wage rate equals or becomes higher than the State minimum.

[4] **Illinois:** Employers with 50 or fewer full time employees are eligible for a tax credit equal to a certain percentage of the cost of their annual wage increases. Employers are only eligible for the credit if the average wage for employees making $55,000 or less increases over the year. The amount of the credit that can be claimed is as follows: 25 percent for the 2020 reporting period; 21 percent for 2021; 17 percent for 2022; 13 percent for 2023; 9 percent for 2024; 5 percent for 2025; 5 percent for 2026; 5 percent for 2027, but only for employers with no more than five employees.

[5] The **Maine** minimum wage is automatically replaced with the Federal minimum wage rate if it is higher than the State minimum.

[6] The **Massachusetts** minimum wage rate automatically increases to 10 cents above the rate set in the Fair Labor Standards Act if the Federal minimum wage equals or becomes higher than the State minimum.

[7] **Minnesota**: With the passage of H.B. 2091 (2014), the annual sales volume threshold was reduced to $500,000. For large employers, with an annual sales volume of $500,000 or more, the minimum wage is currently $9.50; for small employers, those with an annual sales volume of less than $500,000, the minimum wage is $7.75.

[8] **Missouri** - In addition to the exemption for federally covered employment, the law exempts, among others, employees of a retail or service business with gross annual sales or business done of less than $500,000.

[9] **Montana**: the $4.00 rate applies to businesses with gross annual sales of $110,000 or less; $8.15 applies to all others.

[10] **Nevada**: $8.25 without health benefits; $7.25 with health benefits.

[11] **New Jersey:** For small employers (six employees or fewer) the schedule of increases is as followers: $10.30 eff. 1-1-20; $11.10 eff. 1-1-21; $11.90 eff. 1-1-22; $12.70 eff. 1-1-23; $13.50 eff. 1-1-24; $14.30 eff. 1-1-25; $15.00 eff 1-1-26.

[12] **New York:** The new minimum wage varies across the state based on geographical location and, in New York City, employer size.

NEW YORK MINIMUM WAGE

Year	NYC Large Employers (11 or more employees)	NYC Small Employers (10 or fewer employees)	Ny Downstate (Nassau, Suffolk, and Westchester counties)
12/31/2017	$13.00	$12.00	$11.00
12/31/2018	$15.00	$13.50	$12.00
12/31/2019	--	$15.00	$13.00
12/31/2020	--	--	$14.00
12/31/2021	--	--	$15.00

[13] **Ohio**: $7:25 for employers grossing $299,000 or less

[14] **Oklahoma**: Employers of ten or more full time employees at any one location and employers with annual gross sales over $100,000 irrespective of number of full time employees are subject to federal minimum wage; all others are subject to state minimum wage of $2.00 (OK ST T. 40 § 197.5).

[15] **Oregon:** In addition to the new standard minimum wage rate, SB 1532 sets out a higher rate for employers located in the urban growth boundary, and a lower rate for employers located in nonurban counties. Their respective planned increases are below.

OREGON MINIMUM WAGE

Year	Portland Metro	Nonurban Counties
7/1/2016	$9.75	$9.50
7/1/2017	$11.25	$10.00
7/1/2018	$12.00	$10.50
7/1/2019	$12.50	$11.00
7/1/2020	$13.25	$11.50
7/1/2021	$14.00	$12.00
7/1/2022	$14.75	$12.50
7/1/2023	$1.25 over standard min. wage	$1 below standard min. wage

[16] **Puerto Rico**: Employers covered by the Federal Fair Labor Standards Act (FLSA) are subject to the Federal minimum wage of $7.25. Employers not covered by the FLSA will be subject to a minimum wage that is at least 70 percent of the Federal minimum wage or the applicable mandatory decree rate of $5.08, whichever is higher. The Secretary of Labor and Human Resources may authorize a rate based on a lower percentage for any employer who can show that implementation of the 70 percent rate would substantially curtail employment in that business.

Other Exceptions

- **Missouri, Oklahoma, Texas, Puerto Rico, Utah**, and **Virginia** exclude from coverage any employment that is subject to the Federal Fair Labor Standards Act.
- **Hawaii, Kansas**, and **Michigan** exclude from coverage any employment that is subject to the Federal Fair Labor Standards Act, if the State wage is higher than the Federal wage.
- The **Georgia** state minimum wage **is** $5.15. Employees covered under the federal Fair Labor Standards Act are subject to the federal minimum wage of $7.25, but those not covered under the FLSA may be paid the state minimum wage of $5.15.

Denver

7700 East First Place
Denver, CO 80230
Tel: 303-364-7700 | Fax: 303-364-7800

Washington

444 North Capitol Street, N.W., Suite 515
Washington, D.C. 20001
Tel: 202-624-5400 | Fax: 202-737-1069

ESSAYS ON ISSUES

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Chicago Fed Letter

How does a federal minimum wage hike affect aggregate household spending?

by Daniel Aaronson, vice president and director of microeconomic research, and Eric French, senior economist and research advisor

This article finds that a federal minimum wage hike would boost the real income and spending of minimum wage households. The impact could be sufficient to offset increasing consumer prices and declining real spending by most non-minimum-wage households and, therefore, lead to an increase in aggregate household spending. The authors calculate that a $1.75 hike in the hourly federal minimum wage could increase the level of real gross domestic product (GDP) by up to 0.3 percentage points in the near term, but with virtually no effect in the long term.

A central part of President Obama's 2013 State of the Union address was a proposal to gradually raise the hourly federal minimum wage from $7.25 to $9. Proponents of a higher minimum wage argue it provides economic stimulus by putting money into the hands of people who are especially likely to spend the extra income.[1] Opponents say a higher minimum wage forces firms that employ minimum wage workers to cut jobs or raise prices on goods and services. In this *Chicago Fed Letter*, we use estimates from our research to analyze both arguments.[2]

1. 2012 distribution of wages in U.S. economy

Worker category	Number of workers (in millions)	Share of workers	Total wage payments ($ billions)	Share of wage payments
$6–$7.25/hour	2	0.02	23	0.00
$6–$9/hour	15	0.13	204	0.04
$6–$10/hour	22	0.19	338	0.07
All hourly workers	69	0.59	2,165	0.43
All workers	117	1.00	5,073	1.00

Notes: Sample weights are used to make the *Current Population Survey* (CPS) respondents comparable to the work force of the U.S. economy aged 16 years and older. Workers paid below the minimum wage of $7.25 per hour appear in the CPS mostly on account of measurement errors in self-reported data. Workers whose reported wages fall below $6 per hour are excluded. Note that tips are included in the wage payment calculations.
Source: Authors' calculations based on data from the U.S. Bureau of Labor Statistics, *Current Population Survey*.

We begin by assessing the number of workers whose wages would be affected by a $1.75 hike in the hourly federal minimum wage. Next, based on our prior research, we predict the likely effects of an increase in the hourly federal minimum wage on total household income, consumer prices, and aggregate household spending. We show that a $1.75 increase in the minimum wage could raise real GDP by about 0.3 percentage points over the short run (first year). Allowing more workers to lose their jobs or allowing the spending response to be smaller than our baseline estimates lowers our projected impact of the minimum wage hike on real GDP over the short run. In addition, we predict the hike's impact on real GDP to be close to zero over the long run.[3]

We view the minimum wage as essentially a "tax and transfer" program. Firms that have to pay higher wages to their workers respond by raising prices on their goods and services. Higher prices on goods and services offset the income benefit for minimum wage workers and reduce the real income of non-minimum-wage workers who did not get a wage increase. Still, an increase in aggregate household spending can arise if minimum wage workers have a higher propensity to spend—particularly in the short run—than non-minimum-wage workers.

Whose wages are affected by a minimum wage hike?

Figure 1 highlights the low end of the U.S. wage distribution using data from the U.S. Bureau of Labor Statistics' *Current Population Survey* (CPS). Approximately 2 million workers, or 2% of the work force, were paid at or just below the current hourly federal minimum wage of $7.25 in 2012. Roughly 15 million workers, representing 13% of the work force, made $6–$9 per hour (i.e., at or somewhat below the proposed new federal minimum). Employers are not required to raise the wages of workers already earning above the new minimum wage. However, in practice they may. Therefore, we include an additional 7 million workers who made slightly more than the proposed new federal minimum wage—i.e., those earning $9–$10 per hour.

Although a substantial share of workers would be affected by minimum wage legislation, its effect on wage payments would be relatively smaller. We estimate that in 2012 roughly $200 billion, or 4% of total CPS-reported wage payments, went to workers earning $6–$9 per hour, and $338 billion, or 7% of total CPS-reported wage payments, went to those earning $6–$10 per hour.

When inferring the likely impact on total household income, consumer prices, and aggregate household spending from the proposed federal minimum wage hike, we face two important issues. First, 19 states and a handful of cities currently offer a minimum wage above—and sometimes well above—the federal minimum wage. So, if the hourly federal minimum wage were raised by $1.75, these states and cities might raise their hourly minimum wages above $9. To partly account for this, we allow earnings and spending to rise somewhat for the wage group earning $9–$10 per hour. Second, the aggregate wage income of $5.07 trillion computed from the CPS for 2012 is lower than the aggregate wage income of $6.88 trillion reported in the U.S. Bureau of Economic Analysis's *National Income and Product Accounts of the United States* for that year. Most likely this difference arises from an understatement of the earnings of high-income individuals in the CPS, because such individuals are difficult to reach via household surveys. If aggregate wage income has been understated, figure 1 overstates the share of total wage payments going to low-wage individuals. Accounting for this possible overstatement reduces the share of total wage payments going to those making $6–$9 per hour from 4% to 3%.

In the near term, a minimum wage hike can stimulate economic activity by putting money into the hands of people who are especially likely to spend it.

Household income

Next, we compute what happens to total household income as a result of an increase in the hourly federal minimum wage from $7.25 to $9. In Aaronson, Agarwal, and French (2012), we used data from three large, representative data sets—the CPS, the U.S. Census Bureau's *Survey of Income and Program Participation*, and the U.S. Bureau of Labor Statistics' *Consumer Expenditure Survey*—to estimate the impact of a minimum wage hike on household income with adult minimum wage workers. We found that the average real income of households with adult minimum wage workers rose by $250 per quarter during the first few quarters in response to a $1 increase in the minimum wage.[4]

If we assume that 15 million workers earning $6–$9 per hour in 2012 receive a $1.75 hourly wage increase and that the income response is proportional to what we found before, aggregate income will rise by $250 × $1.75 × 15 million = $6.6 billion per quarter, or roughly $26 billion during the year immediately following the hike. Those making $9–$10 per hour likely receive a smaller income increase than those making less. Assuming that the income increase for those earning $9–$10 per hour is only one-third of that for those earning $6–$9 per hour (or $250/3 = $83), we find that those earning $9–$10 per hour would receive $83 × $1.75 × 7 million = $1 billion per quarter, or $4 billion per year. We also found in Aaronson, Agarwal, and French (2012) that the income response to a minimum wage increase is isolated to the groups of workers at and just above the minimum wage. Therefore, the total income gain for all workers is approximately $30 billion per year.

Our analysis in Aaronson, Agarwal, and French (2012) was of adult minimum wage workers—specifically, minimum wage workers who are a household's head and spouse aged 18 and older (or in the absence of a spouse, another working household member aged at least 18). Teenagers (unless they happen to be counted as one of their household's two adult workers) and low-skilled workers without jobs prior to the minimum wage increase were omitted from our analysis. There is some evidence that minimum wage hikes might make it harder to get a job, especially for teenagers, who represent 23% of the minimum wage labor force.[5] We return to this issue later.

Consumer prices

Using a variety of U.S. and Canadian data, we demonstrated in Aaronson (2001) and Aaronson, French, and MacDonald (2008) that immediately after a minimum wage increase, limited-service restaurants (i.e., fast-food restaurants) employing minimum wage workers pass close to 100% of the higher labor costs on to consumers in the form of higher prices.

We conjecture that other (nonrestaurant) firms employing minimum wage workers or using intermediate inputs requiring minimum wage labor also pass close to 100% of the higher labor costs on to consumers in the form of higher prices.[6] A simple way to predict how a $1.75 increase in the hourly federal minimum wage affects the price level is to compare the increase in earnings resulting from the hike to the level of real GDP (for aggregate prices) or to the level of total household consumption (for aggregate consumer prices) under the assumption of no disemployment effects. Based on our estimate of a $30 billion earnings impact in the first year, we calculate that aggregate prices would rise by 0.19% (= $30 billion/$15.685 trillion of 2012 real GDP) and aggregate consumer prices would go up by 0.27% (= $30 billion/$11.12 trillion of 2012 total household consumption).[7]

Aggregate household spending

Finally, to quantify the aggregate household spending response to a federal minimum wage hike, we need to consider the spending of both minimum wage and non-minimum-wage earners in response to the minimum wage hike.

Minimum wage earner spending

In Aaronson, Agarwal, and French (2012), we found that real spending in households with adult minimum wage workers rises, on average, by approximately $700 per quarter during the first few quarters following a $1 hike in the hourly minimum wage. This additional spending, which exceeds the immediate income gain of $250 per quarter, is primarily on durable goods, particularly new vehicles (financed with credit). Our research shows that these patterns can be partly reconciled by augmenting a standard dynamic model of consumer behavior to allow for the ability to borrow against durable goods. The intuition for this result is simple. Suppose a household must make a 20% down payment on an auto purchase. The existence of this borrowing opportunity implies an extra $250 per quarter in income can be leveraged up to $1,250 ($250/0.2 = $1,250) in additional spending. This amount of spending is well beyond what we find in the actual data, perhaps because some minimum wage households cannot finance nondurable purchases with credit.

The spending estimate of $700 per quarter in response to a $1 hike in the hourly minimum wage applies to households with adult minimum wage workers. It seems likely that teenagers, who make up 23% of all minimum wage workers, have less access to credit and therefore will not be able to leverage their earnings. Instead, let us assume that teenage minimum wage workers spend all their income as they earn it. Given the number of teen and adult workers who are likely affected by a $1.75 hike in the hourly federal minimum wage (including those earning $9–$10 per hour), we calculate that spending among minimum wage households could add as much as $73 billion to the economy in the year following the hike, which is 0.47% of real GDP and 0.66% of total household consumption in 2012.

Non-minimum-wage earner spending

Workers who earn above the minimum wage may decrease their real spending as a consequence of a minimum wage hike because they typically face higher product and service prices without the benefit of an earnings boost. Suppose that the spending propensity of non-minimum-wage workers is such that they reduce their real spending by $800 for every $1,000 of real income lost.[8] Those losing the $1,000 of real income through higher prices may not reduce their spending by the full $1,000 but may instead reduce their savings. We predict that this loss for non-minimum-wage earners results in a $25 billion decline in real spending in the year following the minimum wage hike.

Total spending

Combining the estimates for minimum wage earners and non-minimum-wage earners, we predict that an increase of $1.75 in the hourly federal minimum wage raises aggregate household spending by roughly $48 billion in the year following the minimum wage hike, or 0.3% of 2012 real GDP.

However, a few words of caution are in order. First, as we mentioned already, our analysis is based on household income and spending responses from samples of adult minimum wage workers who had a minimum wage job before the hike. There is some evidence that minimum wage hikes might make it harder to get a job, especially for teenagers. Additionally, some workers, particularly teenagers, may lose their jobs as a consequence of a minimum wage hike. For these reasons, we introduce "disemployment elasticities" of –0.5 for teenagers and –0.25 for adults (i.e., for every 10% increase in the minimum wage, the employment of teenagers and adults making the minimum wage would fall by 5% and 2.5%, respectively). Our reading is that these elasticities are at the high end of the literature. Nevertheless, allowing for disemployment of these magnitudes reduces the aggregate spending gain following a $1.75 hike in the federal minimum wage to $28 billion, or 0.2% of 2012 real GDP. The aggregate spending gain would decline to zero if we assume a disemployment elasticity of –0.7 for both teens and adults. Therefore, while more disemployment than we allow for is certainly plausible and would clearly lower our estimate of the spending response, it's unlikely to completely eliminate the entire boost to aggregate spending.

Additionally, for those with low income and poor credit scores, it may be harder to purchase cars on credit after the financial crisis than it was during the sample period of 1980–2008, which we used to estimate the spending response. Indeed, our estimated aggregate spending response is high relative to the rest of the literature. Instead, if we assume that the marginal propensity to spend (i.e., the propensity to spend the next dollar) for households with adult minimum wage workers is half as large in the year following a minimum wage hike as what

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we estimate in the data, the aggregate spending response to a $1.75 increase in the hourly federal minimum wage would be only $4 billion, or 0.02% of 2012 real GDP. This result highlights the mechanism of our prediction—any additional consumer spending from a minimum wage hike arises from differences in the propensity to spend among different income groups.

Finally, it's important to stress that the aggregate household spending response discussed in this article is relevant for only the first few quarters after a minimum wage hike. Beyond that time frame, households must pay off debt they incurred in the short run by spending less. Thus, a minimum wage hike provides stimulus for a year or so, but serves as a drag on the economy beyond that.

Conclusion

Proponents of minimum wage increases often claim that minimum wage hikes will significantly boost the economy. We are skeptical that minimum wage hikes boost GDP in the long run. Nevertheless, we do find evidence that putting money into the hands of consumers, especially low-wage consumers, leads to predictable increases in spending in the short run.

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[1] See, e.g., New York Times Company, 2013, "From the bottom up," *New York Times*, February 17, available at www.nytimes.com/2013/02/18/opinion/wages-from-the-bottom-up.html.

[2] Daniel Aaronson, Sumit Agarwal, and Eric French, 2012, "The spending and debt response to minimum wage hikes," *American Economic Review*, Vol. 102, No. 7, December, pp. 3111–3139; Daniel Aaronson, Eric French, and James MacDonald, 2008, "The minimum wage, restaurant prices, and labor market structure," *Journal of Human Resources*, Vol. 43, No. 3, Summer, pp. 688–720; and Daniel Aaronson, 2001, "Price pass-through and the minimum wage," *Review of Economics and Statistics*, Vol. 83, No. 1, February, pp. 158–169.

[3] For further explanation of the calculations in this article, see www.chicagofed.org/digital_assets/others/people/research_resources/aaronson_daniel/aaronson_french_cfl_313_calculations.xlsx and www.chicagofed.org/digital_assets/others/people/research_resources/aaronson_daniel/aaronson_french_cfl_313_calculations_documentation.pdf.

[4] To put this estimate into perspective, note that adult minimum wage employees work, on average, roughly 300 hours per quarter. Under three assumptions—there is no disemployment (i.e., job loss) due to the minimum wage hike, all workers who are paid close to the minimum wage are covered by minimum wage laws, and there is no measurement error—we anticipate a $1 minimum wage hike to increase each adult minimum wage employee's quarterly earnings by $300.

[5] U.S. Bureau of Labor Statistics, 2012, "Characteristics of minimum wage workers: 2011," report, Washington, DC, March 2, available at www.bls.gov/cps/minwage2011.htm.

[6] Prices and incomes might also rise following a minimum wage hike because of the increase in aggregate demand for goods and services. We do not account for this possibility in our analysis.

[7] Real GDP and total household consumption data are from the U.S. Bureau of Economic Analysis.

[8] See, e.g., Jonathan A. Parker, Nicholas S. Souleles, David S. Johnson, and Robert McClelland, 2011, "Consumer spending and the economic stimulus payments of 2008," National Bureau of Economic Research, working paper, No. 16684, January, available at www.nber.org/papers/w16684.

Do Minimum Wage Increases Really Reduce Public Assistance Receipt?*

Joseph J. Sabia
Professor of Economics
Forrest D. McKerley Chair of Health Economics
University of New Hampshire, San Diego State University & IZA
10 Garrison Avenue
Durham, NH 03824
Email: jsabia@mail.sdsu.edu

Thanh Tam Nguyen
Department of Economics
University of New Hampshire
10 Garrison Avenue
Durham, NH 03824
Email: tn1028@wildcats.unh.edu

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Do Minimum Wage Increases Really Reduce Public Assistance Receipt?

Abstract

Advocates of minimum wage increases claim that an unintended benefit of such hikes is a reduction in means-tested public program participation. Using three decades of data from the Current Population Survey, the Survey of Income and Program Participation (SIPP), and the National Income and Product Accounts (NIPA), this study comprehensively examines the effect of minimum wage increases on five large means-tested public programs. We conclude that recent evidence in support of minimum wage-induced declines in public assistance is based on empirical models that conflate minimum wage effects with effects of the state business cycle and fail falsification tests. Results from more credible specifications show that minimum wage increases are largely ineffective at reducing net program participation. Our findings are more consistent with minimum wage-induced income redistribution whereby minimum wages decrease the probability of welfare take-up for some low-skilled individuals, but decreases the probability of welfare exit for others.

Keywords: minimum wage; welfare expenditures; means-tested public assistance

I. Introduction

> "There are so many very low-wage workers, and we pay for huge social welfare programs for them. [Raising the minimum wage] would save something on the order of tens of billions of dollars. Doesn't it make more sense for employers to pay their workers than the government?" -Republican Senatorial Candidate Ron Unz, *New York Times* (2013)

Policymakers advocating higher minimum wages have long touted their potential to reduce poverty (Roosevelt 1937; Clinton 1996; Obama 2013), but in an attempt to broaden political support to include economic conservatives, advocates now claim that higher minimum wages will reduce low-skilled individuals' participation in and taxpayers' spending on means-tested public assistance programs (Sanders 2016; Courtney 2014; McGovern 2014). In addition, minimum wage-induced reductions in government spending could result in fewer distortionary taxes, which would provide an efficiency rationale for minimum wage increases.

The effect of minimum wage increases on means-tested program participation is theoretically ambiguous. If minimum wage hikes increase the earnings of individuals living in poor or near-poor families (Congressional Budget Office 2014; Dube 2013; Neumark and Wascher 2002), earnings gains may render these individuals ineligible for means-tested public programs. In addition, earnings gains among public assistance recipients could reduce benefits received during the phase-out portion of income eligibility. On the other hand, if minimum wage increases induce adverse labor demand effects (Thompson 2009; Neumark and Wascher 2008; Neumark et al. 2014; Sabia et al. 2016; Clemens and Wither 2016), then some low-skilled individuals will be eligible for means-tested programs, increasing participation rates. On net, minimum wages may simply redistribute program participation among eligible and near-eligible individuals.

Moreover, the effects of minimum wage increases on means-tested programs may differ across (i) programs with heterogeneous eligibility requirements (both income eligibility thresholds and work requirements), (ii) states with heterogeneous policy rules, and (iii) across time as policy reforms change eligibility requirement rules or the business cycle impacts job opportunities.

Two recent highly influential studies by West and Reich (2014; 2015) find that minimum wage increases are associated with reductions in participation in the Supplemental Nutrition Assistance Program (SNAP) and Medicaid, with estimated program participation elasticities with respect to the minimum wage ranging from -0.2 to -0.4. However, the identification strategies employed in these studies – identifying state minimum wage changes off of a state-specific linear time trend or using control states within the same census division – have received substantial criticism in the minimum wage-employment literature. Neumark et al. (2014a,b) argue that this approach eliminates potentially valid sources of identifying variation, leaving "contaminated" variation that obscures adverse employment effects of minimum wages. Masking employment effects could negatively bias program participation elasticities.

Using survey and administrative data over three decades, we comprehensively evaluate the effectiveness of the minimum wage as a welfare reform policy across several means-tested public programs including the Supplemental Nutrition Assistance Program (SNAP), Medicaid, Housing Assistance programs (e.g. Section 8 housing), Temporary Assistance for Needy Families (TANF/AFDC), and the Special Supplemental Nutrition Program for Women, Infants and Children (WIC).

We highlight three major findings. First, while we can replicate the findings of West and Reich (2015; 2014) showing that minimum wage increases are associated with a reduction in program participation, we also show that the models upon which these results are based fail a number of falsification tests. Results from more credible specifications show that minimum wage increases are largely ineffective at reducing net program participation. Second, an examination of longitudinal data shows evidence of minimum wage-induced income redistribution caused by adverse employment effects, whereby some welfare recipients who see income gains are more likely to exit the welfare rolls, but other non-recipients who lose their jobs are more likely to join the rolls due to a reduction in job opportunities. Finally, we find little evidence that minimum wage hikes reduce welfare caseloads or public expenditures on needs-based public programs, and appear least effective during economic downturns. We conclude that the most convincing evidence points to little evidence that minimum wage increases are an ineffective welfare reform policy.

II. Prior Literature on Minimum Wages and Program Participation

The effectiveness of higher minimum wages in reducing means-tested program participation depends on the distribution of earnings and employment effects of minimum wages as well as how well targeted minimum wages are to those who qualify for assistance. The prior literature on this topic is much thinner than the extensive (and controversial) minimum wage-employment literature (Card and Krueger 1995; Neumark and Wascher 2008; Sabia 2008; Dube et al. 2010; Allegretto et al. 2011; Neumark et al. 2014a,b; Meer and West 2013; Clemens and Wither 2016), and the findings do not reach a consensus.

One set of studies uses survey data from the Survey of Income and Program Participation (SIPP), and nearly all focus on the Temporary Assistance for Needy Families/Aid to Families with Dependent Children (TANF/AFDC) program. Using data from the 1986 to 1988 SIPP panels and a difference-in-difference approach, Brandon (1995) finds that higher minimum wages are associated with a reduction in the probability of exit from AFDC, consistent with adverse labor demand effects. An update using data from the 1996 to 2004 SIPP produces a similar pattern of results (Brandon 2008). However, SIPP-based results from shorter panels reach different conclusions. Using data from the 1990 and 1991 panels of the SIPP, Turner (1999) find that minimum wage increases are associated with an increase in the probability of welfare exit. And, in a study of the Great Recession period, Clemens (2015) finds little evidence that minimum wage increases affect social insurance payments. Together findings underscore potential heterogeneous effects of minimum wages in relatively short panels (Baker et al. 1999; Page et al. 2005), which may suggest that minimum wages have different effects (i) at different phases of the business cycle (Sabia 2014a), and (ii) due to changes in program eligibility that may affect the likelihood that minimum wages bind for welfare recipients (Sabia and Nielsen 2015).

A second set of studies has used aggregate state-level administrative data to estimate the effect of minimum wage increases on welfare use, again tending to focus on TANF/AFDC. Using data from 1976 to 1998 and a difference-in-difference approach, a Council of Economic Advisers (1999) study finds that minimum wage increases were associated with a reduction in AFDC caseloads. However, using data from 1983 to 1996, Page et al. (2005) reach the opposite result: a 10 percent increase in the minimum wage is associated with a 1 to 2 percent increase in welfare caseloads. The authors show that (i) the treatment of state-specific time trends and (ii) the time period chosen for the analysis, explain differences in their findings from that of the Council of Economic Advisers.[1,2]

[1] Consistent with Neumark et al. (2014a; 2014b), the pattern of findings suggests that controls for state-specific linear time trends may conflate minimum wage effects with effects of the state business cycle.

[2] While not specifically exploring the effects of minimum wage increases on welfare caseloads, Grogger (2003) uses the minimum wage as a control variable in estimating the effects of other policies on welfare caseloads. Grogger finds a statistically insignificant positive effect.

The final set of studies are based largely on survey data from the Current Population Survey (CPS). These studies have focused on Medicaid and the Supplemental Nutrition Assistance Program (SNAP, formerly known as food stamps). Using data from the 1990 to 2012 March CPS, West and Reich (2015) estimate a difference-in-difference model fully saturated with controls for state-specific linear time trends and census division-specific year effects. They obtain SNAP participation elasticities with respect to the minimum wage of -0.24 and -0.32. Then, drawing data from the National Income and Product Accounts (NIPA) and an identical identification strategy, they estimate a SNAP expenditure elasticity with respect to the minimum wage of -0.19. West and Reich (2014) find a similar pattern of results when estimating the effect of minimum wage hikes on Medicaid participation using an identical identification strategy.

The findings by West and Reich (2014; 2015) have been extremely influential in recent policy debates over the minimum wage as an effective welfare reform. But the specifications upon which these studies reached their conclusion been the subject of substantial empirical criticism in the minimum wage-employment literature. Neumark et al. (2014a; 2014b) argues that the inclusion of controls for state-specific linear time trends not only "throws the baby out with the bathwater" in terms of the amount of identifying variation, but also isolates identifying variation that is "contaminated" in such a way as to conflate estimated minimum wage effects with effects of the state business cycle. These authors also show that states within census divisions do not uniformly serve as better counterfactuals for "treatment states" that increase their minimum wages. Neumark et al. (2014a,b) show convincingly that the chief consequence of specification preferred by West and Reich (2015; 2014) is to obscure negative employment effects of minimum wage increases.[3] Obscuring adverse employment effects could explain why

[3] Using an alternate form of identification, a new working paper by Clemens and Wither (2016) exploits changes in the 2008-2009 Federal minimum wage and initial (2008) state minimum wage levels to identify the effect of minimum wage increases on low-skilled employment. They find that the 30 percent increase in the average minimum wage was associated with a 0.7 percentage-point reduction in the employment-to-population ratio.

West and Reich (2015; 2014) find such large reductions in program participation and public expenditures following minimum wage increases.

Taken together, differences in findings across prior studies can be explained, in part, by differences in the (i) sources of identifying variation, (ii) particular time periods examined (often short windows), (iii) specific public program examined, and (iv) datasets employed. The current study contributes to the above literature by comprehensively examining the effects of minimum wage increases on means-tested program participation across public programs, data sources, identification strategies, and phases of the business cycle. We hold the findings by West and Reich (2014; 2015) up to falsification tests by examining whether their specification produces evidence of minimum wage-induced reductions in welfare use among households that could not have been plausibly affected by minimum wages. Finally, we examine whether minimum wage increases affect net government spending on means-tested public programs.

III. Means-Tested Programs

One of the distinguishing features of this study is that we explore a wide breadth of means-tested public benefit programs. Because eligibility standards differ across programs, as well as across states and over time, we evaluate possible heterogeneous impacts of minimum wages across these dimensions.

The SNAP program, administered by the United States Department of Agriculture (USDA), is the largest nutrition assistance program in the U.S. In 2014, 46.5 million Americans received SNAP benefits, with an average per month benefit level of $125.35 (USDA 2015a). Federal eligibility requires gross monthly household income to be below 130 percent of the Federal poverty threshold (FTP) and permits households to have no more than $2,250 in "countable resources." Other means-tested benefits such as TANF/AFDC or Supplemental

Security Income (SSI) are not counted against household income.[4] In prior decades, many states included vehicle assets against asset limits, but in April 2015, these limits were eliminated via Federal rule changes.[5] The link between SNAP participation and employment strengthened considerably following the passage of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which required individuals ages 18 to 60 without disabilities to be employed or actively seeking work in order to receive benefits (Social Security Administrations 2012). Together, (i) expansions in program eligibility rules to higher-asset households that are more likely to include workers, and (ii) stronger work requirements, increase the likelihood that minimum wage increases will affect SNAP recipients' earnings (either through wage gains or employment losses) and program participation.

Medicaid, administered jointly by Federal and state governments, offers free or low-cost health coverage to low-income families. States must provide coverage for "categorically needy" individuals, including SSI recipients, families with dependent children receiving cash assistance, poor pregnant women and children, and certain low-income Medicare beneficiaries (Center for Medicaid and CHIP Services 2015). In addition, states can offer coverage for medically needy persons, disabled individuals, and pregnant women whose incomes are above income eligibility limits for mandatory coverage. Medicaid has gone through various expansions over the last three decades. Between 1979 and 2014, 44 states obtained demonstration waivers from the Federal government—usually waivers granted under Section 1115 of the Social Security Act—often to expand Medicaid eligibility to near poor families and low-income adults without children. Federal legislation in the late 1980s expanded Medicaid coverage for low-income mothers and dependent children by increasing earnings and child age limits. Beginning in January 2014, the Patient Protection and Affordable Care Act required states that joined Federal

[4] However, there is some heterogeneity across states in age of eligibility and eligibility of those with disabilities.
[5] For instance, in 2014, 39 states excluded vehicles from asset tests (US Department of Agriculture 2014).

health care exchanges to increase Medicaid coverage to individuals and families whose income is at or below 138 percent of the Federal poverty line (Kaiser Family Foundation 2015). Because there is a much weaker link between employment and program participation for Medicaid relative to SNAP, minimum wage hikes may be more likely to affect SNAP participation than Medicaid use.[6]

Subsidized rental housing programs provide subsidies to very low-income families, the elderly, and the disabled to help them rent housing in the private market. The largest of these programs is the Housing Choice Voucher program, commonly known as the Section 8 voucher program.[7] Eligibility is based on a family's annual gross income, family composition and citizenship. In order to qualify for rental subsidies, families must have total incomes less than 80 percent of the median county income, with most subsidies going to very low income families with incomes less than 50 percent of the median county income. These eligibility rules generate substantial heterogeneity in eligibility across geographic locations and time, as income limit and maximum subsidies are updated annually. Relative to Medicaid and SNAP, housing program participants are more likely to be employed and affected by minimum wages.

TANF/AFDC provides temporary cash assistance to poor families with children. In order to qualify for TANF/AFDC, recipients must meet state-set family structure, income, and asset criteria. Under PRWORA, states gained flexibility in designing their own TANF programs within certain federally-set standards, including the enforcement of strict work requirements to qualify for federal aid, and a 60-month lifetime federally-funded benefit limit. Nonetheless,

[6] While not specifically studying the effect of minimum wage increases on Medicaid receipt, McCarrier et al. (2011) used data from the Behavioral Risk Factor Surveillance System from 1996 to 2007 and found that minimum wage increases were associated with a lower probability of unmet medical needs, but no change in the probability of having insurance.

[7] In addition to the Housing Choice Voucher Program, low-income renters may also receive housing assistance via such programs as the Section 8 New Construction and the Substantial Rehabilitation and Loan Management Set-Aside programs.

there are differences across states in the strictness of enforcement of these work requirements. For instance, most states require TANF applicants to search for jobs or register to work as quickly as possible (Falk 2012). As of July 2014, 19 states mandate job search activities before or at the time of application (Huber et al. 2015). Current TANF recipients are also subject to sanction if they fail to comply with work requirements, which range from partial reduction of benefits for the first noncompliance to a more severe penalty such as lifetime ineligibility for multiple violations (Falk 2012).

While the link between TANF and employment was strengthened in the 1990s, during the Great Recession, TANF recipients found it more difficult to meet work requirements. In fiscal year 2009, the average overall work participation rate for all TANF families was 29.4 percent (USDHHS Office of Family Assistance 2011). In response, many states provided benefits for vulnerable families through state-funded programs outside of TANF (Hahn et al. 2012). In addition, states have the flexibility to grant benefit eligibility extensions to certain TANF families when they reach their time limits (Huber et al. 2015). These eligibility criteria include (i) inability to find employment, (ii) provision of care for ill or disabled persons, (iii) provision of child care, (iv) pregnancy, (v) old age, and (vi) domestic violence victimization.[8]

Finally, the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) offers short-term food supplements and nutrition education for low-income women (pregnant, postpartum with a child 6 months or less, or breastfeeding with an infant between 6 and 12 months), infants and children up to age five. To be eligible to receive WIC benefits, applicants must (i) have household income below 185 percent of the FPT, or (ii) receive Medicaid, AFDC/TANF or SNAP/food stamps, and (iii) be nutritionally at risk based on the

[8] See Huber et al. (2015) for a complete list of state's time limit extensions eligibility requirements.

federal guidelines for the program (USDA 2015).[9] While the income criteria are similar across states, different states have different requirements for proof of income as well as different nutritional standards (Bitler et al. 2003). In 2014, almost 8.3 million people received WIC program benefits, with an average monthly per-person food voucher of $43.65 (USDA 2015).

In summary, differing eligibility standards related to family income, work requirements, and asset exemptions across states and over time suggest that minimum wages may affect different means-tested public program participation differently. While some programs, such as SNAP, are more closely linked to employment requirements, other programs—such as Medicaid, and subsidized rental housing—often lack strong employment requirements and target families that are less likely to be affected by minimum wage increases. Moreover, relatively higher income eligibility standards—such as exist for WIC (up to 185 percent of the FPT)—may increase the likelihood that more minimum wage workers are affected by them.

IV. Data and Measures

Current Population Survey. We begin by using repeated cross-sections of the March Current Population Survey (CPS) from 1980 to 2014 (corresponding to calendar years 1979 to 2013). The March CPS, which has been the workhorse of the minimum wage-poverty literature in the United States (see Burkhauser and Sabia 2007; Sabia and Burkhauser 2010; Sabia, Burkhauser, and Nguyen 2015), allows us to measure participation in several forms of public assistance receipt, including (1) SNAP, (2) Medicaid, (3) subsidized rental housing, (4) TANF/AFDC, and (5) WIC).[10]

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[9] According to the United States Department of Agriculture, "two major types of nutritional risk are recognized for WIC eligibility: (1) medically-based risks (designated as "high priority") such as anemia, underweight, maternal age, history of pregnancy complications, or poor pregnancy outcomes, and (2) Diet-based risks such as inadequate dietary pattern." (USDA 2010).

[10] The relevant questions in the CPS related to these programs are:

For public programs (1) through (3), we focus on working-age individuals ages 16-to-64, following the poverty literature (Sabia and Burkhauser 2010; Sabia and Nielsen 2015). For programs (4) and (5), we follow Moffitt (1999) and Schoeni and Blank (2000), and examine females ages 16-to-54. We then examine lower-skilled, less-educated individuals who are more likely to receive public assistance and be affected by minimum wage policy: non-whites, younger individuals ages 16-to-29 without a high school diploma, and less-educated (less than high school) single mothers ages 16-to-45 with young children (under age 18).

In Panel I of Table 1A, we show weighted means of program participation rates at the individual and household levels using CPS data from 1979 to 2013.[11] As expected, SNAP and Medicaid have the highest relative program participation rates (column 1), and participation is lower among workers (column 2) as compared to non-workers (column 3).[12] An examination of participation rates among less-educated populations most likely to receive means-tested public assistance (columns 4 through 6) suggests participation rates that are 2 to 11 times larger among less-educated single mothers, non-whites, and younger high school dropouts relative to the full sample (column 1).

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(1) SNAP/FSP: "Did (you/anyone in this household) get SNAP (Supplemental Nutrition Assistance Program), food stamps or a food stamp benefit card at any time during [previous year]?"
(2) Medicaid: "At any time in [previous year], was ... covered by Medicaid?"
(3) Subsidized rental housing: "Are you paying lower rent because the Federal, State, or local government is paying part of the cost?"
(4) AFDC/TANF: "At any time during [previous year], even for one month, did ... receive any CASH assistance from a state or county welfare program such as (State Program Name)?"
(5) WIC: "At any time during [previous year], was… on WIC, the Women, Infants, and Children Nutrition Program for themselves or on behalf of a child?"

Information on WIC receipt was added to the March CPS starting in 2001 (Bitler et al. 2003). Respondents are queried about SNAP receipt, and housing assistance receipt for any individuals in their households. Information about Medicaid, TANF, and WIC receipt is collected for each individual within the household.
[11] For data measured at the household-level, "Working Age" households are defined as households with at least one working-age individual residing in the household. A household with a "Worker" is defined as a household with at least one working-age individual who is a worker and a "Non-Worker" household is defined as a household without any workers. A household with "Less Educated Single Mothers," "Non-Whites," and "Younger High School Dropouts" is defined as one that includes one such individual in the household.
[12] For data measured at the household level, "Workers" is defined as having at least one worker in the household, while "Non-Workers" refers to there being no workers in the household.

While the March CPS is widely used to study poverty, an important disadvantage of this data source is severe underreporting of means-tested program participation (Wheaton 2008; Wheaton and Giannarelli 2000). For instance, in 2002, self-reported SNAP participation in the March CPS was 39 percent lower than administrative data shows, Medicaid participation was 29 percent lower, and TANF receipt was 46 percent lower (Wheaton 2008). While such measurement error should not produce biased estimates in the effect of minimum wages on program participation—unless such error is unexpectedly associated with minimum wage changes—we next turn to alternative data sources, which have been documented to more accurately capture public program participation.

Survey of Income and Program Participation. The SIPP is a nationally-representative longitudinal survey of the non-institutionalized, civilian population conducted by the U.S. Census Bureau. We draw data from the 1996-1999, 2001-2003, 2004-2007, and 2008-2013 panels, which correspond to calendar years 1996 to 2013.[13] One important advantage of the SIPP is the relatively short recall period (four months) for respondents to report household composition, income, program participation, and health insurance. This makes the SIPP less prone to error relative to other federal surveys where respondents are required to recall information from as long as a full year prior to the interview. There is also evidence that the SIPP measures true program participation with less error. Compared to the March CPS, the underreporting rate is 22 percent lower for SNAP participation, 9 percent lower for Medicaid participation and 5 percent lower for TANF participation (Wheaton 2008). Another key advantage of the SIPP is that its longitudinal data allow us to (i) explore individual-specific transitions into and out of poverty as well as onto and off of the welfare rolls, and (ii) estimate

[13] Following Sabia and Nielsen (2015), we drop data in the 2000 calendar year, for neither the 1996 panel nor the 2001 panel provides adequate overlap in this calendar year.

models that include individual fixed effects. Means of program participation among individuals and households using the SIPP are shown in Panel II of Table 1A.

Aggregate Welfare Caseloads. In addition to the two microdata sources, we also obtain administrative data on means-tested welfare caseloads between 1980 and 2013. SNAP caseloads are obtained from the Census Bureau-Small Area Income and Poverty Estimates[14], Medicaid caseloads from the Statistical Abstract (Social Insurance and Human Services, and Health and Nutrition, respectively)[15], and AFDC/TANF caseloads from the Office of Family Assistance (DHHS). [16,17] Consistent state-by-year caseload data on WIC participation and housing subsidy receipt are not available during the 1980 to 2013 period. In Panel I of Table 1B, we show weighted means of state welfare caseloads per 1,000 individual state residents. Medicaid caseloads are the highest (159.7 per 1,000), followed by SNAP (91.1 per 1,000) and AFDC/TANF (31.2 per 1,000).

Public Program Expenditures. Finally, we draw aggregate state-by-year data on means-tested program expenditures from the National Income and Product Accounts (NIPA). The NIPA data are collected by the Bureau of Economic Analysis and have been used by a number of scholars to study public welfare spending (Aschauer 1989; Hanson 2010; West and Reich 2015). We draw data from 1980 to 2013 and construct real (in 2013 dollars) per capita expenditures on four programs: SNAP, Medicaid, AFDC/TANF and WIC/Other[18] In Panel II of Table 1B, we show means of real (2013 dollars) means-tested expenditures per capita. Per-capita spending is

[14] SNAP/food stamp caseloads are available between 1981 and 2012.
[15] We obtain consistent Medicaid caseload data for all states between 1983 and 2013.
[16] AFDC/TANF caseloads are missing in 1984.
[17] Medicaid caseloads are collected for the fiscal year.
[18] Data on expenditures on housing subsidies over the 1980-2013 period are not available from the NIPA. In the NIPA, WIC expenditures are grouped with expenditures on General Assistance Foster care and adoption assistance, Child Tax Credits, Economic Stimulus Act of 2008 rebates, American Recovery and Reinvestment Act of 2009 (ARRA) Making Work Pay tax credits, Government Retiree tax credits, Adoptive tax credits and Energy Assistance benefits. Estimation excluding WIC benefits in our measure of total expenditures produced a similar pattern of results.

highest for Medicaid program ($928.5), followed by SNAP ($126.5), WIC/Other ($112.7) and AFDC/TANF ($104.8).[19]

V. Empirical Approach

We begin by pooling repeated cross-sectional data from the March 1980 to March 2014 CPS and estimating the canonical difference-in-difference model used in the minimum wage literature, estimating the below regression equation at the individual and household levels:

$$Program_{ist} = \beta_0 + \beta_1 MW_{st} + \beta_2{}'\mathbf{X}_{st} + \beta_3{}'\mathbf{Z}_{it} + \alpha_s + \tau_t + \varepsilon_{st}, \qquad (1a)$$

where $Program_{ist}$ is an indicator for whether respondent i (or household i) residing in state s in year t received a particular form of means-tested public benefit; MW_{st} is the natural log of the higher of the state or federal minimum wage; $\mathbf{X}_{st}$ is a vector of state-specific, time-varying controls including the prime-age adult wage rate, prime-age unemployment rate, per capita state GDP, the state refundable EITC credit, and key state welfare policies, including whether the state program exempts some or all vehicles from the asset test for SNAP eligibility, the presence of at least one Medicaid Section 1115 demonstration waiver, Medicaid expansions to low-income childless adults[20], the presence of binding work requirements and time limits for TANF receipt, excluding owned home value from asset tests for TANF, and maximum TANF benefit level for a family of three; $\mathbf{Z}_{it}$ is a vector of individual controls including race/ethnicity, marital status, educational attainment, age, family size, and number of children under age 18 living in the household[21]; α_s is a time-invariant state effect; and τ_t is a state-invariant year effects. We

[19] Trends in program participation and program expenditures across all four data sources are available in Figures 1 through 4 of Sabia and Nguyen (2016).
[20] Prior to the Affordable Care Act, a number of states—including Arizona, Minnesota, Pennsylvania, Tennessee and Washington–expanded Medicaid coverage to low-income childless adults without the use of a Section 1115 waiver through the use of exclusively state-funded programs.
[21] When we estimate household-level regressions, the controls of **Z** are measured for the head of households, following West and Reich (2015).

estimate equation (1) via probit, but also experiment with linear probability models, with a generally similar pattern of results.

The key parameter of interest in equation (1a), β_1, is the effect of the minimum wage on means-tested program participation. Identification of β_1 comes from within-state variation in minimum wages. Of the 1,734 state-by-year cells observed from 1980 to 2013, there were over 500 minimum wage increases initiated by state legislatures. In addition, there were four Federal minimum wage increases (1979-81, 1990-91, 1996-97, and 2007-09), which also generate some state-level minimum wage variation because of heterogeneous state minimum wage levels at the time of Federal hikes.

Next, following West and Reich (2014; 2015), we experiment with their preferred specification that adds controls for geographic-specific time-varying unobserved heterogeneity:

$$Program_{ist} = \beta_0 + \beta_1 MW_{st} + \beta_2' \mathbf{X}_{st} + \beta_3' \mathbf{Z}_{it} + \alpha_s + \tau_t + \alpha_s * t + c_d * \tau_t + \varepsilon_{st}, \quad (1b)$$

where α_s*t is a state-specific linear time trend and $c_d * \tau_t$ is a census division-specific year effect.

We next turn to the SIPP and estimate a model similar to equation (1a) except that we exploit the longitudinal nature of the data to estimate transitions onto and off of the welfare rolls, and include both month and individual fixed effects as additional controls:

$$Program_{ismt} = \beta_0 + \beta_1 MW_{smt} + \beta_2' \mathbf{X}_{st} + \beta_3' \mathbf{Z}_{it} + \alpha_s + \pi_m + \tau_t + \theta_i + \varepsilon_{ismt}, \quad (2)$$

where π_m is a vector of month fixed effects and θ_i is a vector of individual fixed effects. The inclusion of individual fixed effects allows us to examine the effects of minimum wages on individual-specific net transitions off of and onto means-tested benefit programs.[22]

[22] We estimate equation (2) via linear probability model. In SIPP public-release data, respondents in Maine and Vermont are grouped together and respondents in North Dakota, South Dakota, and Wyoming are grouped together in the 1996 and 2001 panels, prohibiting assignment of state policies and economic data. Therefore, respondents in these states are excluded from all SIPP analyses. In the SIPP regressions, we control for individuals' time-varying demographic characteristics (excluding gender and race), state-specific time-varying controls and program policies used in equation (1), and an indicator for the fourth month of the reference period.

In addition, following Sabia and Nielsen (2015), we disaggregate transitions. We condition the sample on those initially receiving (or not receiving) some form of public assistance in the first month of interview of year *t* and estimate the effect of minimum wage increases on transitions onto (or off of) public assistance over that calendar year:

$$Transition_{ist} = \beta_0 + \beta_1 MW_{st} + \beta_2{}'\mathbf{X}_{st} + \beta_3{}'\mathbf{Z}_{it} + \alpha_s + \tau_t + \theta_i + \varepsilon_{ismt}, \quad (3)$$

where $Transition_{ist}$ is an indicator variable equal to one (1) if the respondent *i* makes a transition from his or her initial state at any point during the remainder of that calendar year and equal to 0 otherwise. In equation (3), MW_{st} is then the higher of federal or state minimum wage that persists over calendar year *t* in state *s* (and a weighted average of that minimum wage over the year if the minimum wage changes mid-year). Again, we explore the sensitivity of estimates to controls for spatial heterogeneity.

In addition, we explore the sensitivity of our SIPP estimates to an alternate identification strategy advanced by Clemens and Wither (2016). This approach uses a Federal minimum wage increase rather than a state minimum wage increase to identify program participation effects, with the argument that state-specific minimum wage changes driven by Federal changes might be more exogenous to low-skilled individuals' economic well-being than are state legislative changes. Following Clemens and Wither (2016), we exploit heterogeneity in the bindingness of the 2008-2009 Federal minimum wage changes across states (27 of which were bound by the Federal minimum wage change based on initial state minimum wage levels) and across workers (some of whom earned wages such that they were bound by the minimum wage change) to identify minimum wage effects.

Finally, we draw aggregate state-level data to estimate the effect of minimum wage increases on per-capita state expenditures and caseloads:

$$E_{st} = \beta_0 + \beta_1 MW_{st} + \beta_2{}'\mathbf{X}_{st} + \alpha_s + \tau_t + \varepsilon_{st}, \quad (4)$$

where E_{st} measures the natural log of (i) per capita expenditures, (ii) expenditures per enrollee and (iii) caseloads per 1,000 individuals.[23]

VI. Results

Our main results are shown in Tables 2 through 8 and focus on program participation (or expenditure) elasticities with respect to the minimum wage, derived from estimates of β_1. Standard errors corrected for clustering at the state-level and all regressions are weighted.

Main Findings: CPS

Table 2 shows estimates from the canonical model described in equation (1a). The first three columns present results for the working-age population, with Panel I showing results at the individual-level and Panel II at the household-level. Column (1) includes exogenous demographic controls (age, race/ethinicity, gender), column (2) adds potentially endogenous individual controls (marital status, educational attainment, age, family size, and number of children under age 18 living in the household), and column (3) includes all state-level controls. Across specifications, the magnitude of the estimated minimum wage effect is relatively stable, providing some support for the hypothesis that minimum wage changes are exogenous to program participation.

Together, estimates using the canonical model provide little support for the hypothesis that minimum wage increases are effective at reducing means-tested program participation

[23] In equation (4), we control for the state-by-year share of male individuals, racial composition, average age and state population using data drawn from the Surveillance, Epidemiology and End Results (SEER) database between 1980 and 2013. State-by-year marriage rates, educational attainment, average household size and average number of children under age 18 in households are obtained using data from the CPS March between 1980 and 2014. Other state-specific time-varying controls and state public program policies are remained the same with those used in equation (1).

presents results for the working-age population (see column 3).[24] We find no evidence that minimum wage increases are associated with reductions in SNAP/Food stamp use, housing assistance receipt, TANF/AFDC use, or WIC receipt, whether measured at the individual- or household-level. Moreover, when individual-level Medicaid use is examined (Panel I), we find no evidence that minimum wage increases reduce Medicaid receipt.[25] Only when measured at the household-level (Panel II) is there some evidence of minimum wage-induced reductions in Medicaid use, though this effect is not seen when examining those households with at least one worker (Panel II, column 4), those most likely to be helped by minimum wage increases. The most consistent evidence we find in Table 2 is that minimum wage increases are associated with an increase in receipt of housing subsidies, with an estimated elasticity of 0.231 to 0.300. The finding in column (5) suggests that the increase in housing subsidy receipt may be driven by those who are laid off in response to minimum wage hikes. Across all programs, when we condition the sample on workers to give the minimum wage its best chance to reduce program participation, we find no evidence that minimum wage increases reduce net program use.

In Panel III, we estimate the effect of minimum wage increases on participation in *Any Program*, measured at the household-level.[26] Our results uniformly point to statistically insignificant and economically small minimum wage effects. The precision of our estimate in column (3) of Panel III is such that we can, with 95 percent confidence, rule out estimates program elasticities with respect to the minimum wage less than –0.170 and greater than 0.134.[27]

[24] Examining participation at the individual-level, the precision of our estimates is such that we can rule out negative elasticities smaller than -0.397 for SNAP/food stamp, -0.322 for Medicaid, -0.139 for AFDC/TANF, and -0.157 for WIC. We can also rule out positive elasticities larger than 0.163 for SNAP/food stamp, 0.348 for Medicaid, 0.197 for AFDC/TANF, and 0.021 for WIC.

[25] In Appendix Table 1, we generate estimates using linear probability models. With one exception (TANF/AFDC), the pattern of results is similar to what is shown in Table 2.

[26] WIC is excluded from our *Any Program* measure, as WIC is only measured from 2001 to 2014. Results including WIC, available upon request, are qualitatively similar.

[27] When we control for minimum wage leads to ensure that estimates are not contaminated by pre-trends and minimum wage lags to allow for longer-run policy impacts (Appendix Tables 2A and B), we continue to find no evidence that minimum wage increases affect program participation using the canonical model.

The results in Table 2 diverge sharply from West and Reich (2014; 2015), who identify minimum wage effects off of a state-specific linear time trend within census divisions. In Table 3, we show results from equation (1b), the preferred specification of West and Reich (2014; 2015). The first five columns show results measuring program participation at the individual-level and the final four columns at the household-level. Estimated minimum wage elasticities in column (1) of Table 3 are starkly different from those obtained in column (3) of Table 2, which uses the canonical model (equation 1a). Consistent with West and Reich, we find that minimum wage increases are associated with sharp reductions in SNAP participation, subsidized housing receipt (row 3, column 1), AFDC receipt (row 4, column 1) and WIC receipt (row 5, column 1). Estimated elasticities of program participation with respect to the minimum wage range from -0.091 to -0.400.

Which policy conclusion is correct – the null findings of Table 2 or the large, negative program participation effects in Table 3? There are important reasons to be skeptical of the results generated using the West-Reich model. Neumark et al. (2014a,b) warn that including controls for state-specific linear time trends and census division-specific year effects may conflate minimum wage variation with the state business cycle. Moreover, when we restrict the sample to employed individuals (Table 3, column 2)—giving the minimum wage its best chance to reduce program participation—we find that the estimated elasticities are uniformly smaller (in absolute magnitude) than for the full working-age sample (column 2 vs. column 1) and are nearly always statistically indistinguishable from zero. Instead, we find that minimum wage increases are associated with large reductions in public program participation for *non-workers* (column 3).

Minimum wage increases could only reduce public program participation among *non-workers* if other individuals living in their household are workers who see earnings gains from minimum wage increases, thus increasing household income and reducing program participation among other household members. But in column (4), when we restrict the sample to non-workers living in households *with only one working-age adult age 18 or older,* we find that in the West and Reich-preferred specification, minimum wage increases are associated with very large declines in means-tested program participation. This result suggests that the West and Reich-preferred specification fails an important falsification test and likely overstates minimum wage-induced reductions in program participation. The result in column (4) could only be explained by sample selection, wherein minimum wage increases induce employers to substitute workers who receive welfare for workers who do not, a finding that (i) has not been documented in the literature, and (ii) is at odds with evidence that welfare participation is linked to characteristics associated with higher unobserved marginal productivity (Irving and Loveless 2015; Moffitt et al. 2002). In contrast, results from the canonical model in column (5) pass this falsification test.

Further isolating the West-Reich finding, when we allow state-specific time trends to reach the 4th or 5th order polynomial (see Appendix Table 3), we find little evidence that minimum wage increases affect net welfare participation. This result is consistent with Neumark et al. (2014a), who find that controlling for higher-order polynomial state trends, in contrast to linear time trends, diminishes the degree to which negative employment effects of the minimum wage are confounded by the business cycle.

Columns (6) through (9) of Table 3 repeat this analysis using program participation measured at the household-level. The West-Reich model shows that minimum wage increases reduce program participation among households without any workers (column 8), while the canonical model shows no such effect (column 9). We also repeat this analysis using program participation measured at the family- as compared to household-level (see Appendix Table 4) and we uncover the same pattern of results.

In a study subsequent to West and Reich (2014; 2015), Allegretto et al. (Forthcoming) argue against choosing between the canonical model and the West and Reich-preferred model by using the post-least absolute shrinkage and selection operator (LASSO) regression method advanced by Belloni et al. (2014). This is a "data-driven" approach that chooses the set of right-hand side controls based on their importance in predicting program participation or state minimum wages. We allow all the individual- and state-level controls, including state-specific linear time trends and census division-specific year effects, to be included in the pool of the potential controls. The estimates using the post-LASSO double-selection method are presented in Table 4. This approach, like the West and Reich (2014; 2015) model, continues to fail falsification tests, showing that minimum wage hikes reduce program participation among households without workers (columns 4 and 7).

Together, the findings from Tables 2 through 4 suggests that the canonical model performs favorably relative to the West and Reich (2014; 2015) specification and the post-LASSO double selection model, passing falsification tests that the other models fail. Both the West and Reich and LASSO models appear to conflate minimum wage effects with effects of the state business cycle, consistent with Neumark et al. (2014a). Results from the most credible specification suggests that minimum wage increases have little effect on net public program participation.

Low-Skilled Sub-Populations

In Panel I of Table 5, we use our preferred specification from equation (1a) and examine low-skilled sub-populations that have been commonly examined in the minimum wage-poverty literature: non-whites (columns 1), individuals ages 16-to-29 without a high school diploma (columns 2), and single less-educated female heads of households ages 16-to-45 with children under age 18 (columns 3). There is little evidence that minimum wage hikes reduce program participation among these lower-skilled sub-groups. Only for SNAP is there some evidence of a reduction in program participation among less-educated single mothers (row 1, column 3).[28]

To examine whether the null findings of Table 2 can be explained by adverse labor demand effects among low-skilled individuals, in Panel II of Table 5, we use March 1980 to March 2014 CPS data to estimate the effects of minimum wage increases on employment, weeks, hours worked, and earnings among our low-skilled samples. We find no evidence that minimum wage increases are associated with net increases in unconditional earnings (row 1). For non-whites and younger less-educated individuals, this result appears to be explained by adverse employment (row 2), and conditional hours (row 3) and weeks (row 4) effects. Thus, the adverse labor demand effects of minimum wage increases appear to result in earnings redistribution that does not generate net declines in means-tested program participation.[29]

SIPP Findings

Table 6 shows results from the canonical model using SIPP data. The findings in columns (1) through (3), provide little evidence that minimum wages are associated with a reduction in the probability of SNAP, Medicaid, TANF, or WIC receipt.[30,31] Consistent with CPS-based results, we continue to find that minimum wage hikes are associated with an increase in subsidized housing receipt (row 3).[32] The remaining columns (columns 4 through 6) show

[28] In unreported results that are available upon request, we show estimates separately for workers and non-workers for these low-skilled sub-groups. The results continue to suggest little evidence that higher minimum wages are effective at reducing program participation, even among the low-skilled workers.
[29] Evidence for adverse labor demand effects of the minimum wage in Panel II of Table 4 stands in stark contrast to the West and Reich (2015)-preferred specification shown in column (1) through (3) of Appendix Table 5, which obscures these adverse employment effects (Neumark et al. 2014a 2014b).
[30] The precision of our estimates in column (1) is such that we can rule out negative elasticities smaller than -0.180 for SNAP, -0.166 for Medicaid, -0.571 for TANF, and -0.068 for WIC. Moreover, we can rule out positive elasticities larger than 0.140 for SNAP, 0.098 for Medicaid, 0.255 for TANF, and 0.282 for WIC.
[31] In the SIPP, employment is defined as having a paid job in at least one week of the reference month.
[32] When we restrict CPS data to the SIPP states and years, our results are qualitatively similar.

findings for low-skilled sub-groups. Our results point to little evidence that minimum wage increases reduce means-tested public program receipt, with a few exceptions. We find some evidence that minimum wage increases are associated with a reduction in SNAP participation for 16-to-29 year-olds without a high school diploma (row 1, column 5). However, at the same time, we find that for non-whites (column 4), minimum wage increases are associated with an increase in housing assistance (row 3) and WIC receipt (row 5). There is also evidence of an overall increase in program receipt for less-educated single mothers (column 6, row 5). Together, these findings are consistent with redistributive effects of minimum wage increases across low-skilled sub-groups and across public programs. In results available upon request, we find a similar pattern of redistribution when examining participation data at the household-level.

In Table 7, we present estimates from equation (3) to allow heterogeneous effects of minimum wages on transitions onto or off of public assistance. Column (1) presents results for working age individuals; column (2) presents results for those who report employment in each month;[33] and the remaining columns show results for less-skilled sub-groups. The pattern of findings suggests some evidence of redistributive effects of minimum wage increases. For instance, we find that among workers, minimum wage increases are associated with a reduction in the probability that non-Medicaid recipients begin receiving Medicaid. On the other hand, minimum wage increases are associated with a reduction in the probability that less-educated 16-to-29 year-old Medicaid recipients leave the program. To take another example, minimum wage increases are associated with a reduction in the probability that 16-to-29 year-old non-food stamp recipients begin participating in SNAP, but also with a decline in the probability that non-white SNAP recipients exit the program. These results are consistent with the hypothesis that

[33] We examine the effects of higher minimum wages on the transitions onto or off of public assistance for those who are employed *in the first month* of a calendar year and find similar results to those reported below.

minimum wage increases redistribute earnings among low-skilled individuals via adverse labor demand effects, for which we find evidence in columns (4) through (6) of Appendix Table 5.

Using the 2008-2009 Federal Minimum Wage Increases for Identification

One critique of the above identification strategies is that they rely chiefly on state legislative changes in minimum wages to identify program participation effects. In Table 8, we follow the approach of Clemens and Wither (2016), and explore the effect of the Federal minimum wage increases from $5.85 to $6.55 in July 2008 and from $6.55 to $7.25 in July 2009 using data between August 2008 and July 2012. We first exploit heterogeneity in bindingness of the minimum wage across states given differential state-specific minimum wage levels in January 2008. Our analysis uses data drawn from the 2008 panel of the SIPP to match longitudinal analysis in Clemens and Wither (2016).[34] Consistent with our findings in Table 6, we find little evidence that minimum wage increases are associated with a reduction in public program participation in either the 12-month period between August 2009 and July 2010 (Post 1) or the 24-month period between August 2010 and July 2012 (Post 2) relative to the baseline period (August 2008 to July 2009). Only for less-educated single mothers is there some evidence of minimum wage-induced reductions in housing assistance receipt.

We also exploit heterogeneity in bindingness of the minimum wage by workers' initial wages, again following Clemens and Wither (2016). We condition the sample on those who work at least one month between August 2008 and July 2009 and earning a wage below $7.50 (fully binding), a wage between $7.50 and $8.50 (partially binding), and a wage between $8.50 and $10.10 (non-binding). The results, presented in the final three columns of Table 8, provide no evidence that the 2008-2009 Federal minimum wage increases reduced program participation.

[34] We cannot measure program participation monthly in the CPS, which prevents us from measuring program participation following the 2008-2009 Federal minimum wage increases, as in Clemens and Wither (2016).

Caseloads and Expenditures

In Table 9, we turn to administrative data and present estimates of equation (4) for welfare caseloads per 1,000 individuals (Panel I), program expenditures per capita (Panels II) and program expenditures per enrollee (Panel III). We find very little evidence that minimum wage increases are associated with changes in Medicaid, or AFDC/TANF caseloads. For expenditures, the findings also point to little evidence that minimum wage increases are associated with significant reductions in government spending on SNAP/Food stamp, AFDC/TANF, Medicaid, or WIC, though the magnitude of the effect is largest for Medicaid spending. Again, these results are consistent with redistributive effects of minimum wage increases that do not reduce net participation in or spending on public programs.

VII. Heterogeneity in Effects of Minimum Wages over the State Business Cycle

Given recent work showing that adverse labor demand effects of minimum wage increases may be larger (in absolute magnitude) during economic recessions (Addison et al. 2013; Sabia 2014a), we explore the heterogeneity in the effects of minimum wages on public program participation over the state business cycle. In Table 10, we follow Sabia (2014a) and interact the minimum wage with three phases of the state business cycle: (1) recessions, measured by negative real state GDP growth, (2) weak to moderate growth, measured by positive growth of less than 2.5 percent, and (3) stronger growth, measured by growth greater than 2.5 percent. The results suggest that minimum wage increases are associated with increases in subsidized housing receipt (column 3 in Panels I and II) during economic recessions (row 1, Panel I), a time when recent research suggest that the adverse employment effects of minimum wages are larger (Addison et al. 2013; Sabia 2014a). However, minimum wage-induced increases in some program participation are smaller during economic expansions, and may become negative, consistent with evidence that the employment effects of minimum wages are much smaller during times of stronger economic growth. We see this result particularly in the SIPP sample for Medicaid, where reductions in program participation appear are larger during economic expansions.

In Table 11, we repeat the exercise in Table 10 using administrative data on welfare caseloads and expenditures. Panel I presents results on caseloads per 1,000 individuals, Panel II on expenditures per capita, and Panel III on expenditures per enrollee. Our results in Table 11 provide relatively little evidence that minimum wage increases reduce welfare caseloads or expenditures across the business cycle. Only for SNAP caseloads is there any evidence of beneficial effects of minimum wage increases and these effects appear concentrated in non-recessionary times.

VIII. Conclusions

While reducing poverty has long been a central talking point for policymakers advocating minimum wage increases, proponents have recently advanced the claim that higher minimum wages may reduce welfare spending. This study provides the most comprehensive study to date on the effect of minimum wage increases on means-tested public programs. Using data from multiple government sources, including the CPS, SIPP, and NIPA from over three decades, we evaluate the minimum wage as a tool of welfare reform. Our preferred specifications show that minimum wage increases are largely ineffective at reducing net participation in public assistance programs or in reducing expenditures on means tested public assistance. We further find that evidence for minimum wage induced-declines in public program participation produced by West and Reich (2014; 2015) are based on specifications that fail important falsification tests.

Our null findings are true across public programs, time periods examined, and data sources. Only for the SNAP program is there some (inconsistent) evidence that higher minimum wages reduce program participation. Rather, the findings we obtain (i) across low-skilled groups and (ii) using longitudinal data more clearly point to evidence that minimum wage increases redistribute income among low-skilled individuals, leading to welfare exit for some, but greater welfare use for others.

Adverse labor demand effects of minimum wage effects are one important reason why minimum wage hikes are ineffective at reducing net means-tested program participation. Poor target efficiency may be another (MaCurdy 2015; Sabia 2014b; Sabia and Burkhauser 2010; Lundstrom 2014; Stigler 1946). A substantial share of individuals receiving public assistance do not work. For example, in 2013, 35 percent of AFDC/TANF recipients, 49 percent of SNAP recipients, and 59 percent of WIC recipients were employed. But even among workers, minimum wages may be poorly targeted to those receiving public assistance. When we evaluate Senator Harkin and Congressman Miller's plan to raise the federal minimum wage from $7.25 to $10.10 per hour, Senator Patty Murray's proposal to raise the minimum wage to $12.00 per hour, and Senator Bernie Sanders's proposal to raise the minimum wage to $15.00 per hour, we find that each is not well targeted to welfare recipients. Only 16.0 percent of those who would be affected by a $10.10 Federal minimum wage are SNAP recipients and just 13.1 percent are Medicaid recipients. The targeting of minimum wage increases to welfare recipients becomes worse at $12.00 and $15.00 minimum wage levels. We conclude that minimum wages are an ineffective and blunt tool for welfare reform.

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Table 1A. Summary Statistics of Program Participation, CPS and SIPP

	Working Ages	Workers	Non-Workers	Non-White	Ages 16-29 without HS	Single Mothers without HS Ages 16-45
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I: March CPS 1979-2013						
Individual-Level						
SNAP/Food Stamp	0.077 (0.267)	0.050 (0.217)	0.171 (0.376)	0.148 (0.355)	0.174 (0.379)	0.611 (0.488)
	[3,798,071]	[2,943,160]	[854,911]	[1,128,449]	[352,576]	[30,316]
Medicaid	0.076 (0.265)	0.038 (0.192)	0.204 (0.403)	0.135 (0.342)	0.183 (0.387)	0.545 (0.498)
	[3,798,071]	[2,943,160]	[854,911]	[1,128,449]	[352,576]	[30,316]
Housing assistance	0.010 (0.1)	0.006 (0.08)	0.023 (0.150)	0.021 (0.144)	0.02 (0.14)	0.097 (0.295)
	[3,798,071]	[2,943,160]	[854,911]	[1,128,449]	[352,576]	[30,316]
AFDC/TANF[a]	0.035 (0.183)	0.018 (0.134)	0.081 (0.272)	0.069 (0.253)	0.075 (0.263)	0.406 (0.491)
	[1,679,508]	[1,231,641]	[447,867]	[527,660]	[168,183]	[30,316]
WIC[ab]	0.044 (0.204)	0.034 (0.180)	0.070 (0.255)	0.072 (0.258)	0.089 (0.285)	0.237 (0.425)
	[777,444]	[566,271]	[211,173]	[285,331]	[80,817]	[12,628]
Household-Level						
SNAP/Food Stamp	0.084 (0.278)	0.059 (0.236)	0.311 (0.463)	0.158 (0.365)	0.166 (0.372)	0.607 (0.488)
	[1,880,539]	[1,705,863]	[174,676]	[598,058]	[296,056]	[30,316]
Medicaid	0.142 (0.349)	0.112 (0.316)	0.413 (0.492)	0.253 (0.435)	0.263 (0.440)	0.689 (0.463)
	[1,880,539]	[1,705,863]	[174,676]	[598,058]	[296,056]	[30,316]
Housing assistance	0.013 (0.115)	0.009 (0.092)	0.058 (0.234)	0.026 (0.160)	0.020 (0.141)	0.096 (0.294)
	[1,880,539]	[1,705,863]	[174,676]	[598,058]	[296,056]	[30,316]
AFDC/TANF[a]	0.044 (0.206)	0.029 (0.167)	0.296 (0.456)	0.082 (0.275)	0.116 (0.302)	0.423 (0.494)
	[1,384,842]	[1,306,571]	[78,271]	[473,920]	[158,153]	[30,316]
WIC[ab]	0.052 (0.222)	0.049 (0.215)	0.100 (0.301)	0.066 (0.248)	0.082 (0.274)	0.261 (0.439)
	[639,142]	[603,435]	[35,707]	[334,549]	[145,062]	[12,759]
Any Program[c]	0.168 (0.374)	0.134(0.341)	0.476 (0.499)	0.296 (0.457)	0.304 (0.460)	0.764 (0.425)
	[1,880,539]	[1,705,863]	[174,676]	[598,058]	[296,056]	[30,316]
Panel II: SIPP 1996-2013						
Public Assistance Measures						
SNAP/Food Stamp	0.049 (0.217)	0.024 (0.152)	0.115 (0.319)	0.087 (0.282)	0.057 (0.231)	0.575 (0.494)
	[9,551,775]	[6,779,707]	[2,772,068]	[2,893,801]	[762,746]	[65,559]
Medicaid	0.089 (0.285)	0.039 (0.193)	0.218 (0.413)	0.151 (0.358)	0.229 (0.42)	0.539 (0.498)
	[9,551,775]	[6,779,707]	[2,772,068]	[2,893,801]	[762,746]	[65,559]
Housing assistance	0.010 (0.101)	0.006 (0.078)	0.021 (0.145)	0.021 (0.143)	0.022 (0.148)	0.089 (0.284)
	[9,551,775]	[6,779,707]	[2,772,068]	[2,893,801]	[762,746]	[65,559]

	Working Ages	Workers	Non-Workers	Non-White	Ages 16-29 without HS	Single Mothers without HS Ages 16-45
	(1)	(2)	(3)	(4)	(5)	(6)
AFDC/TANF[a]	0.019 (0.135)	0.007 (0.082)	0.044 (0.205)	0.037 (0.189)	0.039 (0.193)	0.231 (0.422)
	[4,133,931]	[2,802,407]	[1,331,524]	[1,356,214]	[361,122]	[65,559]
WIC[a]	0.056 (0.230)	0.035 (0.184)	0.101 (0.302)	0.102 (0.302)	0.129 (0.336)	0.252 (0.434)
	[4,133,931]	[2,802,407]	[1,331,524]	[1,356,214]	[361,122]	[65,559]
Any Program[c]	0.185 (0.389)	0.151 (0.358)	0.440 (0.496)	0.327 (0.469)	0.353 (0.478)	0.800 (0.400)
	[4,827,050]	[4,189,074]	[637,976]	[1,525,164]	[654,001]	[65,559]
Transition onto Public Assistance						
SNAP/Food Stamp	0.020 (0.142)	0.013 (0.112)	0.042 (0.202)	0.034 (0.182)	0.029 (0.168)	0.227 (0.419)
	[974,035]	[763,275]	[351,210]	[289,181]	[99,294]	[3,788]
Medicaid	0.038 (0.191)	0.022 (0.147)	0.087 (0.282)	0.066 (0.249)	0.1 (0.3)	0.255 (0.436)
	[926,640]	[746,544]	[312,529]	[265,727]	[79,420]	[3,820]
Housing assistance	0.005 (0.068)	0.003 (0.057)	0.009 (0.092)	0.009 (0.094)	0.009 (0.094)	0.036 (0.187)
	[1,016,134]	[781,545]	[386,963]	[310,704]	[101,995]	[7,399]
AFDC/TANF[a]	0.009 (0.095)	0.005 (0.068)	0.02 (0.139)	0.017 (0.129)	0.022 (0.147)	0.094 (0.291)
	[438,113]	[329,319]	[179,729]	[143,420]	[47,906]	[6,290]
WIC[a]	0.022 (0.147)	0.015 (0.122)	0.039 (0.194)	0.039 (0.195)	0.059 (0.235)	0.096 (0.294)
	[422,850]	[319,512]	[170,616]	[135,060]	[44,212]	[6,062]
Any Program	0.044 (0.205)	0.028 (0.166)	0.094 (0.291)	0.076 (0.265)	0.107 (0.309)	0.264 (0.441)
	[892,921]	[725,942]	[294,989]	[247,077]	[75,340]	[2,345]
Transition off of Public Assistance						
SNAP/Food Stamp	0.300 (0.458)	0.408 (0.492)	0.237 (0.425)	0.284 (0.451)	0.325 (0.468)	0.192 (0.394)
	[54,178]	[24,472]	[40,817]	[28,957]	[5,135]	[4,260]
Medicaid	0.337 (0.473)	0.456 (0.498)	0.279 (0.448)	0.337 (0.473)	0.34 (0.474)	0.246 (0.431)
	[101,573]	[41,203]	[79,498]	[52,411]	[25,009]	[4,228]
Housing assistance	0.389 (0.487)	0.408 (0.492)	0.396 (0.489)	0.387 (0.487)	0.406 (0.491)	0.424 (0.495)
	[12,079]	[6,202]	[8,352]	[7,434]	[2,434]	[649]
AFDC/TANF[a]	0.449 (0.497)	0.611 (0.487)	0.38 (0.485)	0.418 (0.493)	0.438 (0.496)	0.338 (0.473)
	[9,392]	[3,999]	[7,913]	[6,040]	[1,704]	[1,758]
WIC[a]	0.349 (0.477)	0.393 (0.488)	0.313 (0.464)	0.33 (0.47)	0.332 (0.471)	0.345 (0.475)
	[24,655]	[13,806]	[17,026]	[14,400]	[5,398]	[1,986]
Any Program	0.275 (0.446)	0.371 (0.483)	0.218 (0.413)	0.262 (0.740)	0.284 (0.451)	0.133 (0.339)
	[135,292]	[61,805]	[100,325]	[71,061]	[29,089]	[5,703]

Notes: Weighted means are obtained from data drawn from the Current Population Survey March Supplements between 1980 and 2014, and the Survey of Income and Program Participation between 1996 and 2013. Standard deviations are in parentheses and number of observations in brackets.

[a] Sample in columns (1) through (3) is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

Table 1B. Summary Statistics of per Capita Caseloads and Expenditures

	Mean	Std. Dev	N
Panel I: Caseloads per 1,000 individuals			
SNAP/Food Stamp	91.098	34.306	1,632
Medicaid[a]	159.700	118.078	1,469
AFDC/TANF[b]	31.169	20.286	1,683
Panel II: Expenditures per capita (2013$)			
SNAP/Food Stamp	126.456	63.379	1,734
Medicaid[c]	928.481	507.117	1,581
AFDC/TANF	104.826	78.606	1,734
WIC & other[d]	112.721	82.510	1,734
Total of above programs	1,158.131	547.678	1,581

Notes: Weighted means are obtained from data drawn from the Census Bureau—Small Area Income and Poverty Estimates between 1981 and 2012 (SNAP/Food stamp), the Statistical Abstract—Health and Nutrition between 1980 and 2011 (Medicaid), the Office of Family Assistance between 1980 and 2013 (AFDC/TANF), and the National Income and Product Accounts (expenditures) between 1980 and 2013.

[a] Medicaid caseloads are missing for Arizona between 1983 and 1990, and Hawaii in 1997 and 1999.

[b] AFDC/TANF caseloads are missing for 1984.

[c] Data are consistently available for all states and years between 1983 and 2013.

[d] WIC expenditures are grouped with expenditures on General Assistance Foster care and adoption assistance, Child Tax Credits, Economic stimulus Act of 2008 rebates, American Recovery and Reinvestment Act of 2009 (ARRA) Making Work Pay tax credits, Government Retiree tax credits, Adoptive tax credits and Energy Assistance benefits.

Table 2. Estimates of the Relationship between Minimum Wage Increases and Public Assistance Receipt, CPS, 1979-2013

	Working age			Workers	Non-workers
	(1)	(2)	(3)	(4)	(5)
	Panel I: Individual Level				
SNAP/Food stamp	-0.017	-0.013	-0.009	-0.002	-0.029
	(0.017)	(0.013)	(0.011)	(0.006)	(0.027)
N	3,798,071	3,798,071	3,798,071	2,943,160	854,911
Medicaid	-0.001	-0.000	0.001	0.010	-0.038
	(0.016)	(0.014)	(0.013)	(0.008)	(0.038)
N	3,798,071	3,798,071	3,798,071	2,943,160	854,911
Housing assistance	0.003***	0.003***	0.003**	0.001	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
N	3,798,071	3,798,071	3,798,071	2,943,160	854,911
AFDC/TANF[a]	-0.002	0.001	0.001	0.001	0.007
	(0.007)	(0.003)	(0.003)	(0.001)	(0.009)
N	1,679,508	1,679,508	1,679,508	1,231,641	447,867
WIC[ab]	-0.003	-0.001	-0.003	-0.001	-0.010
	(0.004)	(0.002)	(0.002)	(0.002)	(0.006)
N	777,444	777,444	777,444	566,271	211,173
	Panel II: Household-Level				
SNAP/Food stamp	-0.019	-0.018	-0.014	-0.005	-0.055
	(0.018)	(0.014)	(0.012)	(0.008)	(0.066)
N	1,880,539	1,880,539	1,880,539	1,705,863	174,676
Medicaid	-0.026	-0.035*	-0.036*	-0.020	-0.095
	(0.019)	(0.020)	(0.022)	(0.018)	(0.074)
N	1,880,539	1,880,539	1,880,539	1,705,863	174,676
Housing assistance	0.003*	0.003**	0.003*	0.002	0.030**
	(0.002)	(0.001)	(0.002)	(0.001)	(0.013)
N	1,880,539	1,880,539	1,880,539	1,705,863	174,676
AFDC/TANF[a]	0.003	0.000	0.000	-0.001	0.099
	(0.008)	(0.004)	(0.004)	(0.003)	(0.079)
N	1,384,842	1,384,842	1,384,842	1,306,571	78,271
WIC[ab]	-0.007	-0.003	-0.006	-0.007	0.024
	(0.007)	(0.006)	(0.006)	(0.005)	(0.030)
N	639,142	639,142	639,142	603,435	35,707
	Panel III: Overall Program Participation				
Any Program[c]	0.015	0.002	0.003	0.018	-0.043
	(0.014)	(0.015)	(0.013)	(0.011)	(0.063)
N	1,880,539	1,880,539	1,880,539	1,705,863	174,676
Exogenous controls?	Yes	Yes	Yes	Yes	Yes
Additional controls?	No	Yes	Yes	Yes	Yes
State-level controls?	No	No	Yes	Yes	Yes

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects from weighted probit estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Exogenous controls include age, gender, race/ethnicity, state dummies, and year dummies. Additional controls include marital status, educational attainment, household size, and number of children under age 18 in households. State level controls include the prime-age adult wage rate, prime-age unemployment rate, per capita state GDP, the state refundable EITC credit rate, and state welfare policies for SNAP/food stamp (indicators for vehicle exemptions per household for eligibility), Medicaid (the presence of at least one Section 1115 waiver or childless adult coverage expansions), and AFDC/TANF (the presence of binding work requirements for welfare receipt and time limits for benefits, state limitations on non-home real and personal property, maximum benefits for family of three with no income). Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

Table 3. Robustness of Estimates of Relationship between Minimum Wage Increases and Public Assistance Receipt to Controls for Geographic-Specific Time Trends, CPS, 1979-2013

	Individual Level					*Household Level*			
	Working age	**Workers**	**Non-workers**	**Non-workers (HH Adult=1[d])**	**Non-workers (HH Adult=1[d])**	**Working age**	**Workers**	**Non-workers**	**Non-workers**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SNAP/Food stamp	-0.021***	-0.006	-0.076***	-0.227**	-0.067	-0.024***	-0.007	-0.212***	-0.055
	(0.008)	(0.005)	(0.021)	(0.095)	(0.063)	(0.007)	(0.005)	(0.054)	(0.066)
N	3,798,071	2,943,160	854,911	110,365	110,365	1,880,539	1,705,863	174,676	174,676
Medicaid	-0.008	-0.000	-0.030	-0.206**	-0.086	-0.013	0.002	-0.123*	-0.095
	(0.009)	(0.005)	(0.024)	(0.087)	(0.071)	(0.013)	(0.011)	(0.072)	(0.074)
N	3,798,071	2,943,160	854,911	110,365	110,365	1,880,539	1,705,863	174,676	174,676
Housing assistance	-0.004**	-0.003*	-0.007	-0.050	0.032	-0.004**	-0.003	-0.021	0.030**
	(0.002)	(0.001)	(0.005)	(0.033)	(0.031)	(0.002)	(0.002)	(0.019)	(0.013)
N	3,798,071	2,943,160	854,911	110,365	110,365	1,880,539	1,705,863	174,676	174,676
AFDC/TANF[a]	-0.008***	-0.003*	-0.017*	-0.258**	0.037	-0.010***	-0.006**	-0.137**	0.099
	(0.003)	(0.002)	(0.009)	(0.123)	(0.116)	(0.003)	(0.002)	(0.070)	(0.079)
N	1,679,508	1,231,641	447,867	51,793	51,793	1,384,842	1,306,571	78,271	78,271
WIC[ab]	-0.004*	0.001	-0.030***	-0.021	0.009	-0.006	-0.004	-0.031	0.024
	(0.002)	(0.002)	(0.008)	(0.018)	(0.014)	(0.006)	(0.005)	(0.028)	(0.030)
N	777,444	566,271	211,173	23,872	23,872	639,142	603,435	35,707	35,707
Any Program[c]	-0.021*	0.001	-0.060**	-0.147*	-0.040	-0.018	0.003	-0.189***	-0.043
	(0.011)	(0.009)	(0.023)	(0.075)	(0.051)	(0.013)	(0.011)	(0.066)	(0.063)
N	3,798,071	2,943,160	854,911	110,365	110,365	1,880,539	1,705,863	174,676	174,676
State linear time trend?	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Division FE*Year FE?	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	No

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects from weighted probit estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

[d] Sample is restricted to households with only one working-age adult age 18 or older.

Table 4. Double Selection Post-LASSO Estimates of Relationship between Minimum Wage Increases and Public Assistance Receipt, CPS, 1979-2013

	Individual Level				*Household Level*		
	Working age	**Workers**	**Non-workers**	**Non-workers (HH Adult=1[d])**	**Working age**	**Workers**	**Non-workers**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SNAP/Food stamp	-0.020**	-0.005	-0.071***	-0.235**	-0.024***	-0.007	-0.208***
	(0.008)	(0.005)	(0.023)	(0.097)	(0.008)	(0.005)	(0.058)
N	3,798,071	2,943,160	854,911	110,365	1,880,539	1,705,863	174,676
Medicaid	-0.009	-0.001	-0.029	-0.254***	-0.014	0.001	-0.117
	(0.009)	(0.005)	(0.025)	(0.094)	(0.013)	(0.011)	(0.073)
N	3,798,071	2,943,160	854,911	110,365	1,880,539	1,705,863	174,676
Housing assistance	-0.004**	-0.003**	-0.007	-0.044	-0.004**	-0.003*	-0.022
	(0.002)	(0.002)	(0.006)	(0.039)	(0.002)	(0.002)	(0.020)
N	3,798,071	2,943,160	854,911	110,365	1,880,539	1,705,863	174,676
AFDC/TANF[a]	-0.008**	-0.003	-0.015	-0.232*	-0.011***	-0.006*	-0.117
	(0.003)	(0.002)	(0.010)	(0.123)	(0.004)	(0.003)	(0.073)
N	1,679,508	1,231,641	447,867	51,793	1,384,842	1,306,571	78,271
WIC[ab]	-0.011*	0.003	-0.058***	-0.038	-0.006	-0.005	-0.034
	(0.006)	(0.004)	(0.014)	(0.042)	(0.006)	(0.006)	(0.033)
N	777,444	566,271	211,173	23,872	639,142	603,435	35,707
Any Program[c]	-0.009	0.005	-0.050*	-0.232**	-0.018	0.003	-0.185***
	(0.011)	(0.008)	(0.027)	(0.099)	(0.013)	(0.011)	(0.070)
N	3,798,071	2,943,160	854,911	110,365	1,880,539	1,705,863	174,676

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects from weighted probit estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

[d] Sample is restricted to households with only one working-age adult age 18 or older.

Table 5. Estimates of the Relationship between Minimum Wage Increases by Low-Skilled Sub-Groups, CPS, 1979-2013

	Non-White	Ages 16-29 without HS	Single Mothers without HS, Ages 16-45
	(1)	(2)	(3)
	Panel I: Public Assistance Receipt		
SNAP/Food stamp	0.001	0.017	-0.193**
	(0.013)	(0.017)	(0.080)
N	1,128,449	352,576	30,316
Medicaid	0.028	-0.038	-0.023
	(0.021)	(0.026)	(0.066)
N	1,128,449	352,576	30,316
Housing assistance	0.003	0.005	0.042
	(0.003)	(0.004)	(0.038)
N	1,128,449	352,576	30,316
AFDC/TANF[a]	0.008	0.002	0.008
	(0.010)	(0.008)	(0.138)
N	527,660	168,183	30,316
WIC[ab]	0.003	0.013	-0.104
	(0.006)	(0.016)	(0.089)
N	285,331	80,817	12,628
Any program[c]	0.097***	0.022	-0.056
	(0.022)	(0.029)	(0.052)
N	598,058	296,056	30,316
	Panel II: Labor Market Outcomes[d]		
Ln(Earnings)	0.075	-0.630***	1.203
	(0.140)	(0.202)	(0.746)
N	1,080,091	347,330	29,759
	[23,146.710]	[5,236.063]	[7,944.093]
Employed	0.024	-0.085***	0.151
	(0.018)	(0.032)	(0.092)
N	1,080,091	347,330	29,759
	[0.692]	[0.486]	[0.522]
Ln(Hours) | Employed=1	-0.046**	-0.214***	0.011
	(0.018)	(0.046)	(0.072)
N	750,173	170,024	15,523
	[38.33]	[29.002]	[35.424]
Ln(Weeks) | Employed=1	-0.044***	-0.058*	0.076
	(0.013)	(0.032)	(0.129)
N	750,173	170,024	15,523
	[4.288]	[4.161]	[4.217]

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects from weighted probit estimates in Panel I and row 2 of Panel II as well as weighted OLS estimates in rows 1, 3 and 4 of Panel II are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable in Panel I is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2.

[a] Sample is restricted to women ages 16 to 54. Standard errors corrected for clustering on the state are in parentheses and means in brackets.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

[d] Earnings are unconditional and measured as annual earnings; hours as weekly hours, and weeks as annual weeks. We take the natural log of 1 for individuals who report zero earnings.

Table 6. Estimates of the Relationship between Minimum Wage Increases and Public Assistance Receipt, SIPP, 1996-2013

	Working age	Workers	Non-workers	Non-White	Ages 16-29 without HS	Single Mothers without HS Ages 16-45
	(1)	(2)	(3)	(4)	(5)	(6)
SNAP/Food stamp	-0.001	-0.004	0.006	0.006	-0.045***	-0.027
	(0.004)	(0.002)	(0.009)	(0.007)	(0.013)	(0.098)
N	9,551,775	6,779,707	2,772,068	2,893,801	762,746	65,559
Medicaid	-0.003	-0.005	0.013	-0.008	0.009	0.090
	(0.006)	(0.005)	(0.013)	(0.015)	(0.036)	(0.070)
N	9,551,775	6,779,707	2,772,068	2,893,801	762,746	65,559
Housing assistance	0.006***	0.002	0.016**	0.016***	0.006	-0.004
	(0.002)	(0.002)	(0.007)	(0.005)	(0.009)	(0.053)
N	9,551,775	6,779,707	2,772,068	2,893,801	762,746	65,559
AFDC/TANF[a]	-0.003	-0.000	-0.003	0.001	0.005	0.037
	(0.004)	(0.003)	(0.010)	(0.009)	(0.021)	(0.074)
N	4,133,931	2,802,407	1,331,524	1,356,214	361,122	65,559
WIC[a]	0.006	0.009*	0.004	0.022*	-0.017	-0.023
	(0.005)	(0.005)	(0.011)	(0.011)	(0.043)	(0.074)
N	4,133,931	2,802,407	1,331,524	1,356,214	361,122	65,559
Any program	0.005	0.000	0.024*	0.012	0.013	0.131*
	(0.007)	(0.007)	(0.013)	(0.017)	(0.034)	(0.066)
	9,551,775	6,779,707	2,772,068	2,893,801	762,746	65,559

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Weighted OLS estimates are obtained using data drawn from the Survey of Income and Program Participation between 1996 and 2013. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Time-variant individual controls include marital status, educational attainment, age (linear and squared), household size, and number of children under age 18 in households. State level controls include the prime-age adult wage rate, prime-age unemployment rate, per capita state GDP, the state refundable EITC credit rate, and state welfare policies for SNAP/food stamp (indicators for vehicle exemptions per household for eligibility), Medicaid (the presence of at least one Section 1115 waiver or childless adult coverage expansions), and AFDC/TANF (the presence of binding work requirements for welfare receipt and time limits for benefits, state limitations on non-home real and personal property, maximum benefits for family of three with no income). All regressions include controls for state effects, year fixed effects, month fixed effects, and individual fixed effects. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54 in columns (1) through (4), and women of stated ages in columns (5) and (6).

Table 7. Estimates of the Relationship between Minimum Wage Increases and Transition onto and off of Public Assistance, SIPP, 1996-2013

	Working Age		Workers		Non-White		Ages 16-29 without HS		Single Mothers Without HS Ages 16-45	
	Transition onto	*Transition off of*	*Transition onto*	*Transition off of*	*Transition onto*	*Transition off of*	*Transition onto*	*Transition off of*	*Transition onto*	*Transition off of*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SNAP/Food stamp	0.007	-0.127	-0.002	-0.041	0.016	-0.184*	-0.081***	0.333	-0.111	-0.059
	(0.008)	(0.086)	(0.008)	(0.230)	(0.012)	(0.113)	(0.030)	(0.602)	(0.481)	(0.373)
N	974,035	54,178	763,275	24,472	289,181	28,957	99,294	5,135	3,788	4,260
Medicaid	-0.010	-0.191**	-0.025**	-0.268	-0.012	-0.153	-0.007	-0.471**	-0.420	-0.555
	(0.012)	(0.082)	(0.011)	(0.208)	(0.032)	(0.115)	(0.091)	(0.202)	(0.304)	(0.482)
N	926,640	101,573	746,544	41,203	265,727	52,411	79,420	25,009	3,820	4,228
Housing assistance	-0.001	-0.499	-0.004	-0.735	-0.007	-0.495	-0.030*	-1.421**	0.003	-1.257
	(0.005)	(0.366)	(0.006)	(0.735)	(0.010)	(0.463)	(0.017)	(0.652)	(0.110)	(1.864)
N	1,016,134	12,079	781,545	6,202	310,704	7,434	101,995	2,434	7,399	649
AFDC/TANF[a]	-0.001	-0.045	-0.003	-0.462	0.000	-0.136	0.020	-0.844	0.014	-0.255
	(0.007)	(0.512)	(0.005)	(0.967)	(0.018)	(0.458)	(0.049)	(1.117)	(0.154)	(0.663)
N	438,113	9,392	329,319	3,999	143,420	6,040	47,906	1,704	6,290	1,758
WIC[a]	-0.006	-0.160	-0.007	-0.250	-0.016	-0.292	0.035	-0.237	0.033	0.158
	(0.009)	(0.203)	(0.008)	(0.411)	(0.024)	(0.247)	(0.102)	(0.538)	(0.122)	(0.828)
N	422,850	24,655	319,512	13,806	135,060	14,400	44,212	5,398	6,062	1,986
Any program	-0.007	-0.167**	-0.019*	-0.311	-0.014	-0.191*	-0.074	-0.131	-0.433	-0.039
	(0.010)	(0.067)	(0.010)	(0.216)	(0.037)	(0.098)	(0.080)	(0.378)	(0.563)	(0.172)
N	892,921	54,178	725,942	24,472	247,077	28,957	75,340	5,135	2,345	4,260

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Weighted OLS estimates are obtained using individual-by-year data drawn from the Survey of Income and Program Participation between 1996 and 2013. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 6. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54 in columns (1) through (6), and women of stated ages in columns (7) through (10).

Table 8. Estimates of the Relationship between the Bindingness of Federal Minimum Wage Increases and Public Assistance Receipt Using Clemens and Wither's (2016) Model, SIPP, 2008-2012

		Working age	Workers	Non-workers	Non-White	Ages 16-29 without HS	Single Mothers without HS Ages 16-45	Baseline Wages: Under $7.50	Baseline Wages: $7.50-$8.49	Baseline Wages: $8.50-$10.10
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SNAP/Food stamp:	Bound*Post 1	0.000	-0.000	-0.000	0.003	-0.004	-0.002	0.001	-0.001	0.007
		(0.002)	(0.001)	(0.005)	(0.004)	(0.005)	(0.032)	(0.009)	(0.009)	(0.005)
	Bound*Post 2	-0.001	-0.000	-0.004	-0.003	-0.015	-0.023	-0.003	0.002	-0.000
		(0.003)	(0.002)	(0.007)	(0.006)	(0.014)	(0.034)	(0.010)	(0.008)	(0.008)
N		1,966,918	1,385,862	581,056	625,021	88,199	10,121	113,537	103,781	150,954
Medicaid:	Bound*Post 1	-0.002	-0.001	-0.003	-0.002	-0.008	0.019	0.004	-0.006	0.001
		(0.003)	(0.002)	(0.006)	(0.007)	(0.011)	(0.029)	(0.011)	(0.009)	(0.009)
	Bound*Post 2	-0.005	-0.002	-0.010	-0.011	-0.045***	0.020	-0.016	0.015	-0.005
		(0.004)	(0.003)	(0.009)	(0.011)	(0.015)	(0.037)	(0.014)	(0.009)	(0.010)
N		1,966,918	1,385,862	581,056	625,021	88,199	10,121	113,537	103,781	150,954
Housing assistance:	Bound*Post 1	-0.001	-0.001	-0.001	-0.001	0.001	-0.058*	-0.002	0.002	-0.000
		(0.001)	(0.001)	(0.003)	(0.004)	(0.008)	(0.031)	(0.004)	(0.004)	(0.003)
	Bound*Post 2	-0.002	-0.001	-0.002	-0.002	0.002	-0.081**	-0.001	-0.000	-0.001
		(0.001)	(0.001)	(0.003)	(0.004)	(0.009)	(0.037)	(0.004)	(0.005)	(0.003)
N		1,966,918	1,385,862	581,056	625,021	88,199	10,121	113,537	103,781	150,954
AFDC/TANF[a]:	Bound*Post 1	0.001	-0.001	0.006*	0.001	0.009	0.005	-0.011*	0.003	-0.003
		(0.001)	(0.001)	(0.003)	(0.003)	(0.006)	(0.026)	(0.006)	(0.004)	(0.005)
	Bound*Post 2	-0.001	0.001	-0.003	-0.001	-0.009	0.026	-0.011**	0.006	-0.001
		(0.001)	(0.001)	(0.003)	(0.003)	(0.011)	(0.031)	(0.004)	(0.008)	(0.006)
N		817,453	548,521	268,932	286,572	43,002	10,121	64,417	56,705	75,069
WIC[a]:	Bound*Post 1	-0.000	0.002	-0.005	-0.004	-0.003	-0.007	-0.006	-0.004	-0.012
		(0.003)	(0.002)	(0.005)	(0.006)	(0.013)	(0.037)	(0.009)	(0.013)	(0.010)
	Bound*Post 2	-0.003	-0.002	-0.004	-0.008	-0.014	0.024	-0.004	-0.003	-0.022*
		(0.004)	(0.003)	(0.007)	(0.006)	(0.026)	(0.043)	(0.013)	(0.015)	(0.013)
N		817,453	548,521	268,932	286,572	43,002	10,121	64,417	56,705	75,069
Any program:	Bound*Post 1	-0.000	-0.000	-0.003	0.001	-0.015	0.021	-0.005	0.008	0.012
		(0.003)	(0.002)	(0.006)	(0.010)	(0.016)	(0.034)	(0.010)	(0.011)	(0.009)
	Bound*Post 2	-0.003	-0.000	-0.011	-0.011	-0.018	0.045	-0.007	0.012	0.009
		(0.004)	(0.003)	(0.008)	(0.015)	(0.024)	(0.040)	(0.010)	(0.014)	(0.011)
N		1,966,918	1,385,862	581,056	286,572	43,002	10,121	113,537	103,781	150,954

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Weighted OLS estimates are obtained using data drawn from the Survey of Income and Program Participation between 1996 and 2013. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 5. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54 in columns (1) through (4), and women of stated ages in columns (5) and (6).

Table 9. Estimates of the Relationship between Minimum Wage Increases and Welfare Caseloads and Expenditures, 1980-2013

	Panel I: Caseloads per 1,000 individuals		
	SNAP /Food stamp	*Medicaid*[a]	*AFDC /TANF*[b]
	(1)	(2)	(3)
Ln(MW)	-0.191	0.021	-0.275
	(0.125)	(0.258)	(0.443)
N	1,632	1,469	1,683

	Panel II: Expenditures per Capita				
	SNAP /Food stamp	*Medicaid*[c]	*AFDC /TANF*	*WIC & other*[d]	*All programs*
	(1)	(2)	(3)	(4)	(5)
Ln(MW)	-0.168	-0.117	0.041	-0.166	-0.158
	(0.122)	(0.141)	(0.176)	(0.277)	(0.098)
N	1,734	1,581	1,734	1,734	1,581

	Panel III: Expenditures per Enrollee[e]			
	SNAP /Food stamp	*Medicaid*[c]	*AFDC /TANF*	*All programs*
	(1)	(2)	(3)	(4)
Ln(MW)	0.024	-0.064	0.310	0.081
	(0.075)	(0.256)	(0.452)	(0.110)
N	1,632	1,469	1,683	1,419

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Weighted OLS estimates using data drawn from the Census Bureau—Small Area Income and Poverty Estimates (SNAP/food stamp caseloads) between 1981 and 2012, the Survey of Income and Program Participation between 1996 and 2013, the Statistical Abstract—Health and Nutrition (Medicaid caseloads) between 1980 and 2011, the Office of Family Assistance (AFDC/TANF caseloads) between 1980 and 2013, and the National Income and Product Accounts (expenditures) between 1980 and 2013. The dependent variables in Panel I, II and III are the natural log of caseloads per 1,000 individuals, per capita expenditures, and expenditures per enrollee for the public assistance program listed in the column title respectively. Controls include the prime-age adult wage rate, prime-age unemployment rate, per capita state GDP, the state refundable EITC credit rate, gender, racial composition, marriage rates, educational attainment, average age, household size, and average number of children under age 18 in households, the prime-age adult wage rate, prime-age unemployment rate, per capita state GDP, the state refundable EITC credit rate, and state welfare policies for SNAP/food stamp (indicators for vehicle exemptions per household for eligibility), Medicaid (Section 1115 waivers, including childless adult coverage expansion), and AFDC/TANF (the presence of binding work requirements for welfare receipt and time limits for benefits, state limitations on non-home real and personal property, maximum benefits for family of three with no income). Standard errors corrected for clustering on the state are in parentheses.

[a] Medicaid caseloads are missing for Arizona between 1983 and 1990, and Hawaii in 1997 and 1999.

[b] AFDC/TANF caseloads are missing for 1984.

[c] Medicaid caseloads and expenditures are collected between 1983 and 2013.

[d] WIC expenditures are grouped with expenditures on General Assistance Foster care and adoption assistance, Child Tax Credits, Economic stimulus Act of 2008 rebates, American Recovery and Reinvestment Act of 2009 (ARRA) Making Work Pay tax credits, Government Retiree tax credits, Adoptive tax credits and Energy Assistance benefits.

[e] Expenditures per enrollee excludes WIC program because data on WIC caseloads are not available over the sample period.

Table 10. Estimates of the Relationship between Minimum Wage Increases and Public Assistance Receipt over the Business Cycle, CPS and SIPP

	SNAP /Food stamp	Medicaid	Housing assistance	AFDC /TANF[a]	WIC[ab]	Any program[c]
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel I: CPS			
MW	-0.012	-0.001	0.003**	0.001	-0.001	0.001
	(0.011)	(0.013)	(0.001)	(0.003)	(0.002)	(0.012)
MW*GDP growth of 0-2.49%	0.000	0.002	0.000	0.001	0.001	0.002
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.004)
MW*GDP growth of ≥2.50%	-0.002	0.001	0.000	-0.001	-0.002	0.005
	(0.003)	(0.004)	(0.001)	(0.001)	(0.002)	(0.005)
N	3,798,071	3,798,071	3,798,071	1,679,508	777,444	1,880,539
			Panel II: SIPP			
MW	0.004	0.005	0.005*	-0.005	0.006	0.009
	(0.005)	(0.008)	(0.002)	(0.004)	(0.006)	(0.084)
MW*GDP growth of 0-2.49%	-0.007	-0.013**	0.000	0.001	0.002	-0.041**
	(0.005)	(0.005)	(0.001)	(0.002)	(0.004)	(0.016)
MW*GDP growth of ≥2.50%	-0.007	-0.013**	0.001	0.004	-0.002	-0.048**
	(0.004)	(0.006)	(0.002)	(0.004)	(0.006)	(0.019)
N	9,551,775	9,551,775	9,551,775	4,133,931	4,133,931	4,827,050

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects in Panel I are obtained from weighted probit regressions using data drawn from the Current Population Survey March Supplements between 1980 and 2014. Weighted OLS estimates in Panel II are obtained using data drawn from the Survey of Income and Program Participation between 1996 and 2013. The dependent variable is an indicator for receipt of the public assistance program listed in the column title. For the CPS estimates, each regression includes a set of controls identical to those noted in Table 2. For the SIPP estimates, each regression includes a set of controls identical to those noted in Table 6. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available for the Current Population Survey March Supplements between 2001 and 2014.

[c] In the CPS estimates, housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

Table 11. Estimates of the Relationship between Minimum Wage Increases and Welfare Caseloads and Expenditures over the Business Cycle

	Panel I: Caseloads per 1,000 individuals		
	SNAP/Food stamp	*Medicaid*[a]	*AFDC/TANF*[b]
	(1)	(2)	(3)
MW	-0.168	0.085	0.070
	(0.126)	(0.248)	(0.057)
MW*GDP growth of 0-2.49%	-0.063*	-0.017	0.013
	(0.032)	(0.107)	(0.016)
MW*GDP growth of ≥2.50%	-0.024	-0.167	-0.022
	(0.040)	(0.117)	(0.028)
N	1,632	1,469	1,275

	Panel II: Expenditures per Capita				
	SNAP /Food stamp	*Medicaid*[c]	*AFDC /TANF*	*WIC & other*[d]	*All programs*
	(1)	(2)	(3)	(4)	(5)
MW	-0.164	-0.127	0.067	-0.266	-0.137
	(0.130)	(0.156)	(0.169)	(0.265)	(0.105)
MW*GDP growth of 0-2.49%	-0.043	0.047	-0.026	0.054	-0.019
	(0.031)	(0.064)	(0.057)	(0.064)	(0.038)
MW*GDP growth of ≥2.50%	0.003	-0.023	-0.050	0.233***	-0.020
	(0.046)	(0.054)	(0.081)	(0.063)	(0.037)
N	1,734	1,581	1,734	1,734	1,581

	Panel III: Expenditures per Enrollee[e]			
	SNAP /Food stamp	*Medicaid*[c]	*AFDC /TANF*	*All programs*
	(1)	(2)	(3)	(4)
MW	0.017	0.013	0.588	0.166
	(0.072)	(0.272)	(0.514)	(0.131)
MW*GDP growth of 0-2.49%	0.016	-0.050	-0.244	-0.036
	(0.024)	(0.090)	(0.165)	(0.073)
MW*GDP growth of ≥2.50%	0.009	-0.091	-0.319	-0.098
	(0.026)	(0.110)	(0.210)	(0.090)
N	1,632	1,469	1,683	1,419

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Weighted OLS estimates using data drawn from the Census Bureau—Small Area Income and Poverty Estimates (SNAP/food stamp caseloads) between 1981 and 2012, the Survey of Income and Program Participation between 1996 and 2013, the Statistical Abstract—Health and Nutrition (Medicaid caseloads) between 1980 and 2011, the Office of Family Assistance (AFDC/TANF caseloads) between 1980 and 2013, and the National Income and Product Accounts (expenditures) between 1980 and 2013. The dependent variables in Panel I, II and III are the natural log of caseloads per 1,000 individuals, per capita expenditures, and expenditures per enrollee for the public assistance program listed in the column title respectively. Each regression includes a set of controls identical to those noted in Table 7. Standard errors corrected for clustering on the state are in parentheses.

[a] Medicaid caseloads are missing for Arizona between 1983 and 1990, and Hawaii in 1997 and 1999.

[b] AFDC/TANF caseloads are missing for 1984.

[c] Medicaid caseloads and expenditures are collected between 1983 and 2013.

[d] WIC expenditures are grouped with expenditures on General Assistance Foster care and adoption assistance, Child Tax Credits, Economic stimulus Act of 2008 rebates, American Recovery and Reinvestment Act of 2009 (ARRA) Making Work Pay tax credits, Government Retiree tax credits, Adoptive tax credits and Energy Assistance benefits.

[e] Expenditures per enrollee excludes WIC program because data on WIC caseloads are not available over the sample period.

Appendix Table 1. OLS Estimates of the Relationship between Minimum Wage Increases and Public Assistance Receipt, CPS, 1979-2013

	Working age	Workers	Non-workers
	(1)	(2)	(3)
	Panel I: Individual Level		
SNAP/Food stamp	-0.006	0.001	-0.018
	(0.013)	(0.009)	(0.023)
N	3,798,071	2,943,160	854,911
Medicaid	0.011	0.027**	-0.026
	(0.016)	(0.012)	(0.034)
N	3,798,071	2,943,160	854,911
Housing assistance	0.005**	0.002	0.017***
	(0.002)	(0.002)	(0.005)
N	3,798,071	2,943,160	854,911
AFDC/TANF[a]	-0.021***	-0.007	-0.046***
	(0.007)	(0.004)	(0.016)
N	1,679,508	1,231,641	447,867
WIC[ab]	-0.008	-0.003	-0.020
	(0.007)	(0.005)	(0.014)
N	777,444	566,271	211,173
	Panel II: Household-Level		
SNAP/Food stamp	-0.016	-0.004	-0.027
	(0.012)	(0.010)	(0.036)
N	1,880,539	1,705,863	174,676
Medicaid	-0.027	-0.010	-0.068
	(0.020)	(0.017)	(0.056)
N	1,880,539	1,705,863	174,676
Housing assistance	0.005	0.003	0.047**
	(0.004)	(0.002)	(0.018)
N	1,880,539	1,705,863	174,676
AFDC/TANF[a]	-0.030***	-0.019***	-0.020
	(0.009)	(0.007)	(0.058)
N	1,384,842	1,306,571	78,271
WIC[ab]	-0.010	-0.010	0.032
	(0.009)	(0.007)	(0.042)
N	639,142	603,435	35,707
	Panel III: Overall Program Participation		
Any Program[c]	0.004	0.021	-0.033
	(0.013)	(0.011)	(0.047)
N	1,880,539	1,705,863	174,676

*** significant at 1% level ** significant at 5% level

Notes: Marginal effects from weighted probit estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

Appendix Table 2A. Robustness of Estimates of the Relationship between Minimum Wage Increases and Public Assistance Receipt Controlling for Three Years of Leads, CPS, 1979-2013

	Working age	Workers	Non-workers	Non-White	Ages 16-29 without HS	Single Mothers without HS Ages 16-45
	(1)	(2)	(3)	(4)	(5)	(6)
SNAP/Food stamp	-0.014	-0.004	-0.044	-0.016	-0.000	-0.233**
	(0.014)	(0.009)	(0.040)	(0.032)	(0.035)	(0.111)
N	3,709,428	2,878,046	831,382	1,095,016	344,908	29,760
Medicaid	0.008	0.015*	-0.016	0.064**	-0.057	-0.036
	(0.014)	(0.008)	(0.042)	(0.026)	(0.034)	(0.134)
N	3,709,428	2,878,046	831,382	1,095,016	344,908	29,760
Housing assistance	-0.001	-0.001	0.001	0.001	0.004	0.050
	(0.002)	(0.001)	(0.005)	(0.005)	(0.006)	(0.063)
	3,580,372	2,783,136	797,236	1,044,699	333,594	28,981
AFDC/TANF[a]	0.004	0.001	0.016	0.014	-0.015	-0.062
	(0.003)	(0.002)	(0.010)	(0.010)	(0.015)	(0.140)
N	1,641,990	1,205,513	436,477	512,827	164,597	29,760
WIC	-0.001	0.001	-0.008	0.010	0.020	-0.243
	(0.003)	(0.002)	(0.008)	(0.010)	(0.023)	(0.169)
N	739,926	540,143	199,783	270,498	77,231	12,072
Any program	0.016	0.027*	-0.034	0.136***	0.024	-0.009
	(0.018)	(0.015)	(0.087)	(0.037)	(0.039)	(0.083)
N	1,836,857	1,666,982	169,875	580,146	289,440	30,068

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects from weighted probit estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54 in columns (1) through (4), and women of stated ages in columns (5) through (6).

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

Appendix Table 2B. Estimates of the Long-Run Relationship between Minimum Wage Increases and Public Assistance Receipt, CPS, 1979-2013

		Working age	Workers	Non-workers
		(1)	(2)	(3)
SNAP/Food stamp:	Ln(MW)	-0.009	-0.000	-0.037
		(0.008)	(0.005)	(0.021)
	$\text{Ln}(MW_{t-1})$	0.003	-0.002	0.026
		(0.010)	(0.007)	(0.028)
	$\text{Ln}(MW_{t-2})$	-0.006	-0.001	-0.024
		(0.014)	(0.008)	(0.040)
χ^2 test $\beta_t + \beta_{t-1} + \beta_{t-2} = 0$ (p-value)		0.55 (0.45)	0.11 (0.74)	0.69 (0.40)
N		3,798,071	2,943,160	854,911
Medicaid:	Ln(MW)	-0.004	0.005	-0.041
		(0.009)	(0.006)	(0.029)
	$\text{Ln}(MW_{t-1})$	-0.001	-0.003	0.011
		(0.008)	(0.006)	(0.023)
	$\text{Ln}(MW_{t-2})$	0.011	0.014*	-0.010
		(0.014)	(0.007)	(0.038)
χ^2 test $\beta_t + \beta_{t-1} + \beta_{t-2} = 0$ (p-value)		0.10 (0.75)	2.07 (0.15)	0.62 (0.43)
N		3,798,071	2,943,160	854,911
Housing assistance:	Ln(MW)	0.005**	0.002	0.015**
		(0.002)	(0.001)	(0.006)
	$\text{Ln}(MW_{t-1})$	-0.006**	-0.005**	-0.012
		(0.003)	(0.002)	(0.008)
	$\text{Ln}(MW_{t-2})$	0.006**	0.005**	0.008
		(0.003)	(0.002)	(0.007)
χ^2 test $\beta_t + \beta_{t-1} + \beta_{t-2} = 0$ (p-value)		5.55 (0.02)	4.58 (0.03)	6.16 (0.01)
N		3,798,071	2,943,160	854,911
AFDC/TANF[a]:	Ln(MW)	-0.002	-0.000	-0.006
		(0.003)	(0.002)	(0.013)
	$\text{Ln}(MW_{t-1})$	0.005	0.001	0.016
		(0.003)	(0.003)	(0.017)
	$\text{Ln}(MW_{t-2})$	-0.001	-0.001	0.001
		(0.003)	(0.002)	(0.014)
χ^2 test $\beta_t + \beta_{t-1} + \beta_{t-2} = 0$ (p-value)		0.27 (0.60)	0.10 (0.75)	0.69 (0.41)
N		1,679,508	1,231,641	447,867
WIC[ab]:	Ln(MW)	-0.003	0.001	-0.019*
		(0.003)	(0.002)	(0.010)
	$\text{Ln}(MW_{t-1})$	-0.000	-0.005	0.017
		(0.004)	(0.003)	(0.013)
	$\text{Ln}(MW_{t-2})$	0.002	0.003	-0.009
		(0.003)	(0.003)	(0.010)
χ^2 test $\beta_t + \beta_{t-1} + \beta_{t-2} = 0$ (p-value)		0.30 (0.59)	0.01 (0.93)	1.34 (0.25)
N		777,444	566,271	211,173
Any program[c]:	Ln(MW)	-0.010	0.007	-0.075
		(0.011)	(0.011)	(0.063)
	$\text{Ln}(MW_{t-1})$	-0.002	-0.006	0.051
		(0.016)	(0.014)	(0.076)
	$\text{Ln}(MW_{t-2})$	0.027	0.030**	-0.018
		(0.019)	(0.014)	(0.085)
χ^2 test $\beta_t + \beta_{t-1} + \beta_{t-2} = 0$ (p-value)		0.59 (0.44)	3.63 (0.06)	0.22 (0.64)
N		1,880,539	1,705,863	174,676

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects from weighted probit estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

Appendix Table 3. Robustness of Estimates of the Relationship between Minimum Wage Increases and Public Assistance Receipt Controlling for Higher-Order Polynomials for State-Specific Trends, CPS, 1979-2013

	State 4th-order polynomial time trends				State 5th-order polynomial time trends			
	Working age	*Workers*	*Non-workers*	*Non-workers (HH Adult=1[d])*	*Working age*	*Workers*	*Non-workers*	*Non-workers (HH Adult=1[d])*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SNAP/Food stamp	-0.009	-0.003	-0.019	-0.026	-0.010	-0.002	-0.026	-0.036
	(0.007)	(0.007)	(0.017)	(0.072)	(0.007)	(0.007)	(0.017)	(0.077)
N	3,798,071	2,943,160	854,911	110,365	3,798,071	2,943,160	854,911	110,365
Medicaid	-0.010	-0.006	0.004	-0.021	-0.010	-0.007	0.003	-0.026
	(0.010)	(0.008)	(0.019)	(0.059)	(0.009)	(0.007)	(0.019)	(0.054)
N	3,798,071	2,943,160	854,911	110,365	3,798,071	2,943,160	854,911	110,365
Housing assistance	0.000	-0.002	0.010	0.003	0.001	-0.002	0.012	-0.001
	(0.003)	(0.002)	(0.007)	(0.036)	(0.003)	(0.003)	(0.008)	(0.040)
N	3,798,071	2,943,160	854,911	110,365	3,798,071	2,943,160	854,911	110,365
AFDC/TANF[b]	-0.009	-0.004	-0.013	-0.023	-0.012**	-0.005	-0.024*	-0.069
	(0.006)	(0.006)	(0.012)	(0.069)	(0.003)	(0.006)	(0.012)	(0.074)
N	1,679,508	1,231,641	447,867	51,793	1,679,508	1,231,641	447,867	51,793
WIC[bc]	-0.006	0.002	-0.024	0.035	-0.007	0.001	-0.030	0.035
	(0.009)	(0.010)	(0.019)	(0.047)	(0.009)	(0.009)	(0.019)	(0.050)
N	777,444	566,271	211,173	23,872	777,444	566,271	211,173	23,872
Any program[c]	-0.014	-0.007	-0.023	-0.011	-0.014	-0.005	-0.047	-0.036
	(0.013)	(0.013)	(0.045)	(0.064)	(0.011)	(0.011)	(0.048)	(0.067)
N	1,880,539	1,705,863	174,676	88,952	1,880,539	1,705,863	174,676	88,952

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Weighted OLS estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2. Standard errors corrected for clustering on the state are in parentheses. Results are estimated via OLS because probit models fail to converge in most cases.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.

[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

Appendix Table 4. Minimum Wages and Family-Level Program Participation, CPS, 1979-2013

	Working age	Workers	Non-workers	Working age	Workers	Non-workers
	(1)	(2)	(3)	(4)	(5)	(6)
SNAP/Food stamp	-0.015	-0.006	-0.066	-0.028***	-0.009	-0.213***
	(0.013)	(0.008)	(0.050)	(0.008)	(0.006)	(0.043)
N	2,062,848	1,837,779	225,069	2,062,848	1,837,779	225,069
Medicaid	-0.036	-0.021	-0.084	-0.011	-0.001	-0.066
	(0.024)	(0.019)	(0.064)	(0.014)	(0.011)	(0.062)
N	2,062,848	1,837,779	225,069	2,062,848	1,837,779	225,069
Housing assistance	0.004**	0.002	0.030***	-0.004*	-0.003*	-0.017
	(0.002)	(0.001)	(0.011)	(0.002)	(0.002)	(0.018)
N	2,062,848	1,837,779	225,069	2,062,848	1,837,779	225,069
AFDC/TANF[a]	0.001	-0.000	0.050	-0.011***	-0.005**	-0.158***
	(0.004)	(0.002)	(0.067)	(0.003)	(0.002)	(0.055)
N	1,395,698	1,294,134	101,564	1,395,698	1,294,134	101,564
WIC[ab]	-0.006	-0.007	0.026	-0.005	-0.005	0.002
	(0.006)	(0.005)	(0.027)	(0.006)	(0.005)	(0.027)
N	642,611	592,567	50,044	642,611	592,567	50,044
Any program[c]	0.004	0.017	-0.039	-0.019	-0.002	-0.138**
	(0.014)	(0.012)	(0.052)	(0.013)	(0.011)	(0.061)
N	2,062,848	1,837,779	225,069	2,062,848	1,837,779	225,069
State linear time trend?	No	No	No	Yes	Yes	Yes
Division FE*Year FE?	No	No	No	Yes	Yes	Yes

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Marginal effects from weighted probit estimates are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. The dependent variable is an indicator for receipt of the public assistance program listed in the left column. Each regression includes a set of controls identical to those noted in Table 2. Standard errors corrected for clustering on the state are in parentheses.

[a] Sample is restricted to women ages 16 to 54.

[b] Data are only available the Current Population Survey March Supplements between 2001 and 2014.[c] Housing assistance receipt is estimated using retrospective minimum wage from the previous year. WIC is excluded because it is only available in the 2001-2014 March CPS.

[d] Sample is restricted to households with only one working-age adult age 18 or older.

Appendix Table 5. Robustness of Estimates of the Relationship between Minimum Wage Increases and Labor Force Participation, CPS and SIPP

	CPS (1979-2013)			SIPP (1996-2013)		
	Non-White	*Ages 16-29 without HS*	*Single Mothers without HS Ages 16-45*	*Non-White*	*Ages 16-29 without HS*	*Single Mothers without HS Ages 16-45*
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Earnings)[a]	0.136	0.366	1.887	-0.072	-0.410	0.107
	(0.247)	(0.301)	(1.166)	(0.117)	(0.247)	(0.726)
N	1,080,091	347,330	29,759	2,798,979	755,827	64,300
	[23,146.710]	[5,236.063]	[7,944.093]	[1,820.492]	[421.132]	[680.942]
Employed	0.014	0.038	0.241*	-0.018	-0.075*	0.015
	(0.029)	(0.046)	(0.135)	(0.014)	(0.041)	(0.110)
N	1,080,091	347,330	29,759	2,798,979	755,827	64,300
	[0.692]	[0.486]	[0.522]	[0.642]	[0.360]	[0.503]
Ln(Hours) | Employed=1	-0.002	-0.002	-0.173	-0.008	-0.250***	0.003
	(0.033)	(0.085)	(0.138)	(0.013)	(0.061)	(0.099)
N	750,173	170,024	15,523	1,594,349	241,956	29,479
	[38.330]	[29.002]	[35.424]	[38.331]	[29.081]	[35.038]
Ln(Weeks) | Employed=1	-0.005	-0.013	0.292	0.056***	0.046	0.054
	(0.019)	(0.052)	(0.220)	(0.020)	(0.033)	(0.052)
N	750,173	170,024	15,523	1,751,881	267,124	31,950
	[44.828]	[31.975]	[38.677]	[4.288]	[4.161]	[4.217]
State & year FE?	Yes	Yes	Yes	Yes	Yes	Yes
Month & individual FE?	No	No	No	Yes	Yes	Yes
Ind. & state controls?	Yes	Yes	Yes	Yes	Yes	Yes
State linear time trend?	Yes	Yes	Yes	No	No	No
Division FE*Year FE?	Yes	Yes	Yes	No	No	No

*** significant at 1% level ** significant at 5% level * significant at 10% level

Notes: Weighted OLS estimates in rows 1, 3 and 4, and marginal effects from weighted probit estimates in row 2 of columns (1) through (3) are obtained using data drawn from the Current Population Survey March Supplements between 1980 and 2014. Weighted OLS estimates in columns (4) through (6) are obtained using data drawn from the Survey of Income and Program Participation between 1996 and 2013. For the CPS estimates, each regression includes a set of controls identical to those noted in Table 2.

For the SIPP estimates, each regression includes a set of controls identical to those noted in Table 6. Standard errors corrected for clustering on the state are in parentheses and means in brackets.

In the CPS, earnings are measured as annual earnings; hours as weekly hours, and weeks as annual weeks. In the SIPP, earnings are measured as monthly earnings; hours as weekly hours, and weeks as monthly weeks.

[a] We take the natural log of 1 for individuals who report zero earnings.

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The Contribution of the Minimum Wage to US Wage Inequality over Three Decades: A Reassessment†

By David H. Autor, Alan Manning, and Christopher L. Smith*

We reassess the effect of minimum wages on US earnings inequality using additional decades of data and an IV strategy that addresses potential biases in prior work. We find that the minimum wage reduces inequality in the lower tail of the wage distribution, though by substantially less than previous estimates, suggesting that rising lower tail inequality after 1980 primarily reflects underlying wage structure changes rather than an unmasking of latent inequality. These wage effects extend to percentiles where the minimum is nominally nonbinding, implying spillovers. We are unable to reject that these spillovers are due to reporting artifacts, however. (*JEL* J22, J31, J38, K31)

The rapid expansion of earnings inequality throughout the US wage distribution during the 1980s catalyzed a rich and voluminous literature seeking to trace this rise to fundamental forces of labor supply, labor demand, and labor market institutions. A broad conclusion of the ensuing literature is that while no single factor was solely responsible for rising inequality, the largest contributors included: (i) a slowdown in the supply of new college graduates coupled with steadily rising demand for skills; (ii) falling union penetration, abetted by the sharp contraction of US manufacturing employment early in the decade; and (iii) a 30 log point erosion in the real value of the federal minimum wage between 1979 and 1988 (see overviews in Katz and Murphy 1992; Katz and Autor 1999; Card and DiNardo 2002; Autor, Katz, and Kearney 2008; Goldin and Katz 2008; Lemieux 2008; Acemoglu and Autor 2011).

An early and influential paper in this literature, Lee (1999), reached a markedly different conclusion. Exploiting cross-state variation in the gap between state median wages and the applicable federal or state minimum wage (the "effective minimum"), Lee estimated the share of the observed rise in wage inequality from

*Autor: Department of Economics, Massachusetts Institute of Technology, 50 Memorial Drive, E52-371, Cambridge, MA 02142, and National Bureau of Economic Research (NBER) (e-mail: dautor@mit.edu); Manning: Centre for Economic Performance and Department of Economics, London School of Economics, Houghton Street, London WC2A 2AE, United Kingdom (e-mail: a.manning@lse.ac.uk); Smith: Federal Reserve Board of Governors, 20th & C Street, NW, Washington DC 20551 (e-mail: christopher.l.smith@frb.gov). We thank Daron Acemoglu, Joshua Angrist, Lawrence Katz, David Lee, Thomas Lemieux, Christina Patterson, Emmanuel Saez, Gary Solon, Steve Pischke, and many seminar participants for valuable suggestions. We also thank David Lee and Arindrajit Dube for providing data on minimum wage laws by state. Any opinions and conclusions expressed herein are those of the authors and do not indicate concurrence with other members of the research staff of the Federal Reserve or the Board of Governors.

†Go to http://dx.doi.org/10.1257/app.20140073 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

1979 through 1988 that was due to the falling minimum rather than changes in underlying ("latent") wage inequality. Lee concluded that *more than* the entire rise of the 50/10 earnings differential between 1979 and 1988 was due to the falling federal minimum wage; had the minimum been constant throughout this period, observed wage inequality would have fallen rather than risen.[1] Lee's work built on the seminal analysis of DiNardo, Fortin, and Lemieux (1996, DFL hereafter), who highlighted the compressing effect of the minimum wage on the US wage distribution prior to the 1980s. Distinct from Lee, however, DFL (1996) concluded that the eroding minimum explained at most 40 to 65 percent of the rise in 50/10 earnings inequality between 1979 and 1988, leaving considerable room for other fundamental factors, most importantly supply and demand.[2]

Surprisingly, there has been little research on the impact of the minimum wage on wage inequality since DFL (1996) and Lee (1999), even though the data they use is now over 20 years old. One possible reason is that while lower tail wage inequality rose dramatically in the 1980s, it has not exhibited much of a trend since then (see Figure 1, panel A). But this does not make the last 20 years irrelevant; these extra years encompass 3 increases in the federal minimum wage and a much larger number of instances where state minimum wages exceeded the federal minimum wage. This additional variation will prove crucial in identifying the impact of minimum wages on wage inequality.

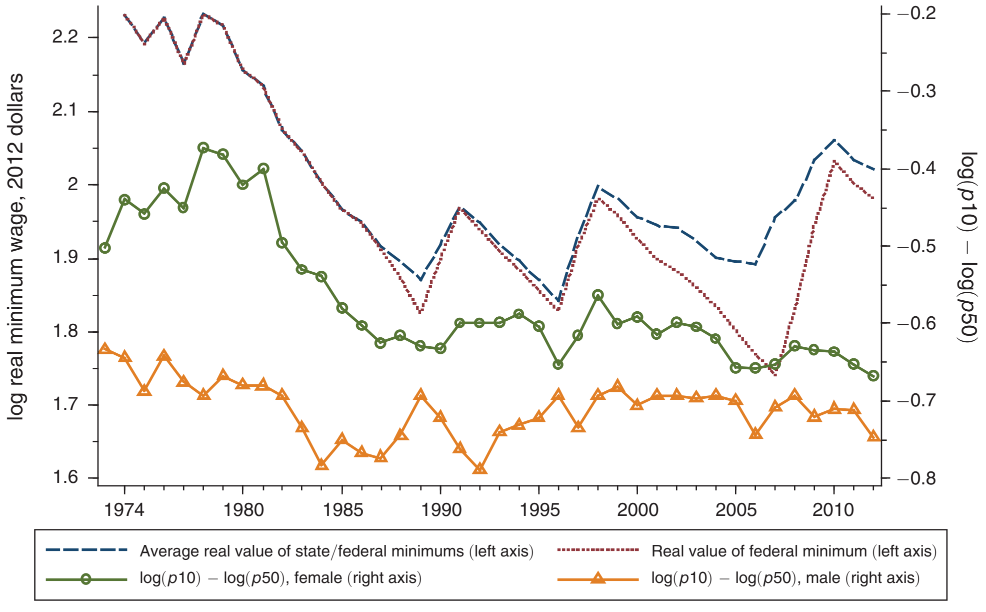
In this paper, we reassess the evidence on the minimum wage's impact on US wage inequality with three specific objectives in mind. A first is to quantify how the numerous changes in state and federal minimum wages enacted in the two decades since DFL (1996) and Lee's (1999) data window closed have shaped the evolution of inequality. A second is to understand *why* the minimum wage appears to compress 50/10 inequality despite the fact that the minimum generally binds well below the tenth percentile. A third is to resolve what we see as a fundamental open question in the literature that was raised by Lee (1999). This question is *not* whether the falling minimum wage contributed to rising inequality in the 1980s but whether underlying inequality was in fact rising *at all* absent the "unmasking" effect of the falling minimum. Lee (1999) answered this question in the negative. And despite the incompatibility of this conclusion with the rest of the literature, it has not drawn reanalysis.

We believe that the debate can now be cleanly resolved by combining a longer time window with a methodology that resolves first-order biases in existing literature. We begin by showing why OLS estimates of the impact of the "effective minimum" on wage inequality are likely to be biased by measurement errors and transitory shocks that simultaneously affect both the dependent and independent variables. Following the approach introduced by Durbin (1954), we purge these biases by instrumenting the effective minimum wage with the legislated minimum

[1] Using cross-region rather than cross-state variation in the "bindingness" of minimum wages, Teulings (2000 and 2003) reaches similar conclusions. Lemieux (2006) highlights the contribution of the minimum wage to the evolution of residual inequality. Mishel, Bernstein, and Allegretto (2007, chapter 3) also offer an assessment of the minimum wage's effect on wage inequality.

[2] See Tables III and V of DFL (1996).

Panel A. Minimum wages and log(p10) − log(p50)



Panel B. Minimum wages and log(p90) − log(p50)

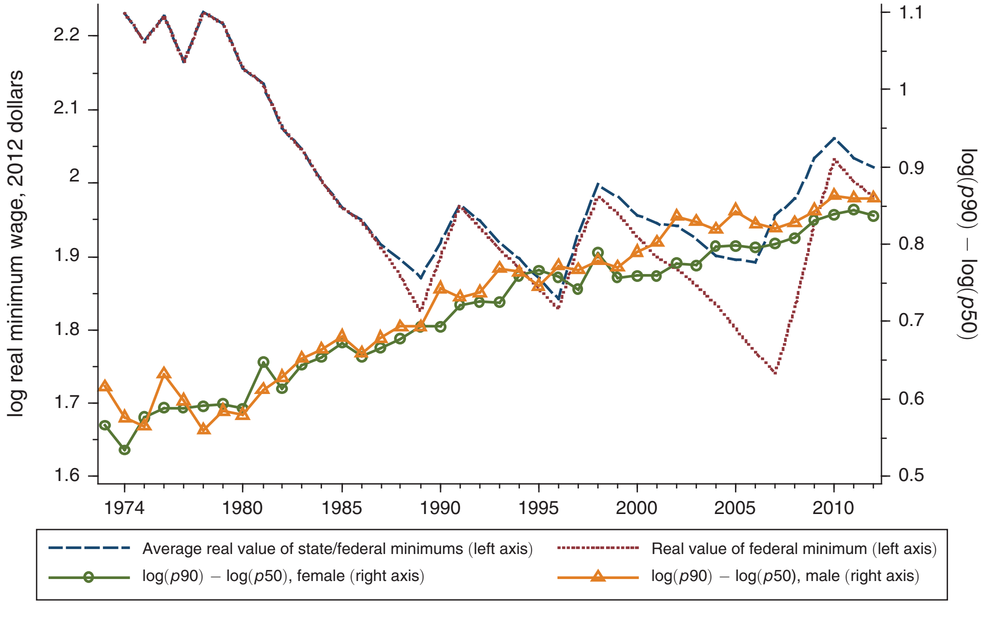


FIGURE 1. TRENDS IN STATE AND FEDERAL MINIMUM WAGES AND LOWER- AND UPPER-TAIL INEQUALITY

Notes: Data are annual averages. Minimum wages are in 2012 dollars.

(and its square), an idea pursued by Card, Katz, and Krueger (1993) when studying the impact of the minimum wage on employment (rather than inequality).

Our instrumental variables analysis finds that the impact of the minimum wage on inequality is economically consequential but substantially smaller than that reported by Lee (1999). The substantive difference comes from the estimation methodology. Additional years of data and state-level legislative variation in the minimum wage allow us to test (and reject) some of the identifying assumptions made by Lee (1999). In most specifications, we conclude that the decline in the real value of the minimum wage explains 30 to 40 percent of the rise in lower tail wage inequality in the 1980s. Holding the real minimum wage at its lowest (least binding) level throughout the 1980s, we estimate that female 50/10 inequality would have risen by 11–15 log points, male inequality by approximately 1 log point, and pooled gender inequality by 7–8 log points. In other words, there was a substantial increase in underlying wage inequality in the 1980s.

In revisiting Lee's estimates, we document that our instrumental variables strategy—which relies on variation in statutory minimum wages across states and over time—does not perform well when limited to data only from the 1980s period. This is because between 1979 and 1985, only one state aside from Alaska adopted a minimum wage in excess of the federal minimum; the ten additional state adoptions that occurred through 1989 all took place between 1986 and 1989 (Table 1). This provides insufficient variation to pin down a meaningful first-stage relationship between the legislated minimum wage and the effective minimum wage. By extending the estimation window to 1991 (as was also done by Lee 1999), we exploit the substantial federal minimum wage increase that took place between 1990 and 1991 to tighten these estimates; extending the sample further to 2012 lends additional precision. We show that it would have been infeasible using data prior to 1991 to successfully estimate the effect of the minimum wage on the wage distribution. It is only with subsequent data on comovements in state wage distributions and the minimum wage that meaningful estimates can be obtained. Thus, the causal effect estimate that Lee sought to identify was only barely estimable within the confines of his sample (though not with the methods used).

Our finding of a modest but meaningful effect of the minimum wage on 10/50 inequality leaves open a second puzzle: why did the minimum wage have any effect at all? Between 1979 and 2012, there is no year in which more than 10 percent of male hours or aggregate hours were paid at or below the federal or applicable state minimum wage (See Figure 2 and Tables 1A and 1B, columns 4 and 8), and only 5 years in which more than 10 percent of female hours were at or below the minimum wage. Thus, any impact of the minimum wage on 50/10 inequality among males or the pooled gender distribution *must* have arisen from spillovers, whereby the minimum wage must have raised the wages of workers earning above the minimum.[3]

[3] If there are disemployment effects, the minimum wage will have spillovers on the observed wage distribution even if no individual wage changes (see Lee 1999, for a discussion of this). The size of these spillovers will be related to the size of the disemployment effect. Although the employment impact of the minimum wage remains a contentious issue (see, for example, Card, Katz, and Krueger 1993; Card and Krueger 2000; Neumark and Wascher 2000; and more recently, see Allegretto, Dube, and Reich 2011 and Neumark, Salas, and Wascher 2014), most estimates are very small. For example, the recent Congressional Budget Office (2014) report on the likely consequences

Table 1A—Summary Statistics for Bindingness of State and Federal Minimum Wages

		A. Females			
	States with > federal minimum (1)	Minimum binding percentile (2)	Maximum binding percentile (3)	Share of hours at or below minimum (4)	Average $\log(p10) - \log(p50)$ (5)
1979	1	5.0	28.0	0.13	−0.38
1980	1	6.0	24.0	0.13	−0.40
1981	1	5.0	24.0	0.13	−0.41
1982	1	5.0	21.5	0.11	−0.48
1983	1	3.5	17.5	0.10	−0.51
1984	1	2.5	15.5	0.09	−0.54
1985	2	2.0	14.5	0.08	−0.56
1986	5	2.0	16.0	0.07	−0.59
1987	6	2.0	14.0	0.06	−0.60
1988	10	2.0	12.5	0.06	−0.60
1989	12	1.0	12.5	0.05	−0.61
1990	11	1.5	14.0	0.05	−0.58
1991	4	1.5	18.5	0.07	−0.58
1992	7	2.0	14.0	0.07	−0.58
1993	7	2.5	11.0	0.06	−0.59
1994	8	2.5	11.0	0.06	−0.61
1995	9	2.0	9.5	0.05	−0.61
1996	11	1.5	12.5	0.05	−0.61
1997	10	2.5	14.5	0.06	−0.60
1998	7	2.5	11.5	0.06	−0.58
1999	10	2.5	11.0	0.05	−0.58
2000	10	2.0	9.5	0.05	−0.59
2001	10	2.0	9.0	0.05	−0.59
2002	11	1.5	9.0	0.04	−0.60
2003	11	1.5	9.0	0.04	−0.61
2004	12	1.5	7.5	0.04	−0.63
2005	15	1.5	8.5	0.04	−0.64
2006	19	1.5	9.5	0.04	−0.64
2007	30	1.5	10.0	0.05	−0.63
2008	31	2.0	13.0	0.06	−0.64
2009	26	2.5	10.5	0.06	−0.64
2010	15	3.5	9.5	0.06	−0.64
2011	19	3.0	10.5	0.06	−0.65
2012	19	3.0	9.5	0.06	−0.66

Note: See text at bottom of Table 1B.

Such spillovers are a potentially important and little understood effect of minimum wage laws, and we seek to understand why they arise.

Distinct from prior literature, we explore a novel interpretation of these spillovers: measurement error. In particular, we assess whether the spillovers found in our samples, based on the Current Population Survey, may result from measurement

of a 25 percent rise in the federal minimum wage from \$7.25 to \$9.00 used a conventional labor demand approach but concluded job losses would represent less than 0.1 percent of employment. This would cause only a trivial spillover effect. In addition, we have explored how minimum wage related disemployment may affect our findings by limiting our sample to 25–64 year olds; because the studies that find disemployment effects generally find them concentrated among younger workers, focusing on older workers may limit the bias from disemployment. When we limit our sample in this way, we find that the effect of the minimum wage on lower tail inequality is somewhat smaller than for the full sample, consistent with a smaller fraction of the older sample earning at or below the minimum. However, using our preferred specification, the contribution of changes in the minimum wage to changes in inequality is qualitatively similar regardless of the sample.

Table 1B—Summary Statistics for Bindingness of State and Federal Minimum Wages

	B. Males				C. Males and females, pooled			
	Minimum binding percentile (1)	Maximum binding percentile (2)	Share of hours at or below minimum (3)	Average log(10) − log(50) (4)	Minimum binding percentile (5)	Maximum binding percentile (6)	Share of hours at or below minimum (7)	Average log(p10) − log(p50) (8)
1979	2.0	10.5	0.05	−0.64	3.5	17.0	0.08	−0.58
1980	2.5	10.0	0.06	−0.65	4.0	15.5	0.09	−0.59
1981	1.5	9.0	0.06	−0.68	2.5	14.5	0.09	−0.60
1982	2.0	8.0	0.05	−0.71	3.5	12.5	0.07	−0.63
1983	2.0	8.0	0.05	−0.73	3.0	11.5	0.07	−0.65
1984	1.5	7.5	0.04	−0.73	2.0	10.5	0.06	−0.67
1985	1.0	6.5	0.04	−0.74	1.5	9.5	0.06	−0.69
1986	1.0	6.5	0.03	−0.74	1.5	10.0	0.05	−0.70
1987	1.0	6.0	0.03	−0.73	1.5	9.0	0.04	−0.70
1988	1.0	6.0	0.03	−0.72	1.5	8.0	0.04	−0.69
1989	1.0	5.0	0.03	−0.72	1.0	7.0	0.04	−0.68
1990	0.5	6.0	0.03	−0.72	0.5	9.0	0.04	−0.67
1991	0.5	9.0	0.04	−0.71	1.0	12.5	0.05	−0.67
1992	1.0	6.5	0.04	−0.72	1.5	9.5	0.05	−0.67
1993	1.0	5.0	0.03	−0.73	1.5	7.5	0.04	−0.68
1994	1.0	4.5	0.03	−0.71	2.0	7.5	0.04	−0.69
1995	0.5	4.5	0.03	−0.71	1.5	6.0	0.04	−0.68
1996	1.0	7.0	0.03	−0.71	1.5	9.0	0.04	−0.67
1997	1.0	7.5	0.04	−0.69	1.5	10.0	0.05	−0.66
1998	1.0	7.0	0.04	−0.69	2.0	8.0	0.05	−0.65
1999	1.0	5.5	0.03	−0.69	2.0	7.0	0.04	−0.65
2000	1.0	6.0	0.03	−0.68	1.5	7.5	0.04	−0.65
2001	0.5	5.5	0.03	−0.68	1.5	7.0	0.04	−0.66
2002	1.0	6.0	0.03	−0.69	1.5	7.5	0.03	−0.65
2003	0.5	5.0	0.03	−0.69	1.5	6.5	0.03	−0.66
2004	1.0	5.0	0.03	−0.70	1.5	6.0	0.03	−0.68
2005	1.0	5.0	0.02	−0.71	1.5	6.5	0.03	−0.68
2006	0.5	6.0	0.02	−0.70	1.0	7.5	0.03	−0.68
2007	0.5	6.0	0.03	−0.70	1.5	7.5	0.04	−0.68
2008	1.0	6.5	0.04	−0.71	1.0	8.5	0.04	−0.69
2009	1.0	6.0	0.04	−0.74	2.0	8.0	0.05	−0.71
2010	2.0	6.5	0.04	−0.73	3.0	7.5	0.05	−0.70
2011	1.5	8.0	0.04	−0.72	2.5	9.0	0.05	−0.69
2012	1.5	7.0	0.04	−0.74	2.0	8.0	0.05	−0.71

Notes: Column 1 in Table 1A displays the number of states with a minimum that exceeds the federal minimum for at least six months of the year. Columns 2 and 3 of Table 1A, and columns 1, 2, 5, and 6 of Table 1B display estimates of the lowest and highest percentile at which the minimum wage binds across states (DC is excluded). The binding percentile is estimated as the highest percentile in the annual distribution of wages at which the minimum wage binds (rounded to the nearest half of a percentile), where the annual distribution includes only those months for which the minimum wage was equal to its modal value for the year. Column 4 of Table 1A and columns 3 and 7 of Table 1B display the share of hours worked for wages at or below the minimum wage. Column 5 of Table 1A and columns 4 and 8 of Table 1B display the weighted average value of the log(p10) − log(p50) for the male or female wage distributions across states.

artifacts. This can occur if a fraction of minimum wage workers report their wages inaccurately, leading to a hump in the wage distribution centered on the minimum wage rather than (or in addition to) a spike at the minimum. After bounding the potential magnitude of these measurement errors, we are unable to reject the hypothesis that the apparent spillover from the minimum wage to higher (noncovered) percentiles is spurious. That is, while the spillovers *are* present in the data, they may not be present in the distribution of wages actually paid. These results do not rule

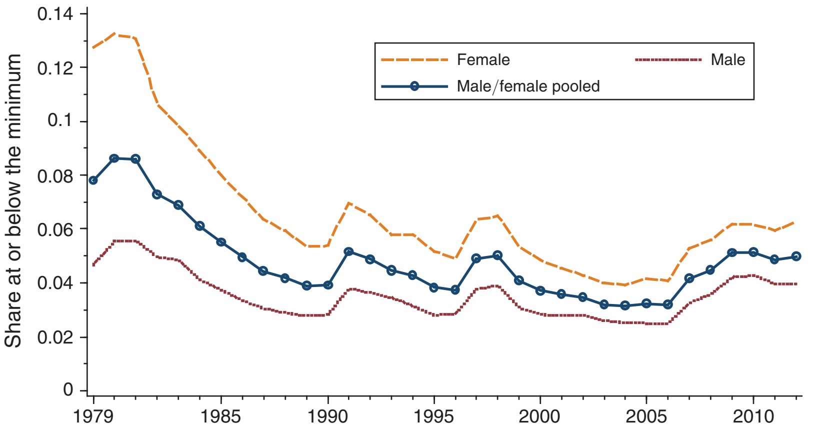


Figure 2. Share of Hours at or Below the Minimum Wage

Notes: The figure plots estimates of the share of hours worked for reported wages equal to or less than the applicable state or federal minimum wage, corresponding with data from columns 4 and 8 of Tables 1A and 1B.

out the possibility of true spillovers. But they underscore that spillovers estimated with conventional household survey data sources must be treated with caution since they cannot necessarily be distinguished from measurement artifacts with available precision.

The paper proceeds as follows. Section I discusses data and sources of identification. Section II presents the measurement framework and estimates a set of causal effects estimates models that, like Lee (1999), explicitly account for the bite of the minimum wage in estimating its effect on the wage distribution. We compare parameterized OLS and 2SLS models and document the pitfalls that arise in the OLS estimation. Section III uses point estimates from the main regression models to calculate counterfactual changes in wage inequality, holding the real minimum wage constant. Section IV analyzes the extent to which apparent spillovers may be due to measurement error. The final section concludes.

I. Changes in the Federal Minimum Wage and Variation in State Minimum Wages

In July of 2007, the real value of the US federal minimum wage fell to its lowest point in over three decades, reflecting a nearly continuous decline from a 1979 high point, including two decade-long spans in which the minimum wage remained fixed in nominal terms—1981 through 1990, and 1997 through 2007. Perhaps responding to federal inaction, numerous states have over the past two decades legislated state minimum wages that exceed the federal level. At the end of the 1980s, 12 states' minimum wages exceeded the federal level; by 2008, this number had reached 31 (subsequently reduced to 15 by the 2009 federal minimum wage increase).[4]

[4] Table 1 assigns each state the minimum wage that was in effect for the largest number of months in a calendar year. Because the 2009 federal minimum wage increase took effect in late July, it is not coded as exceeding most state minimums until 2010.

Consequently, the real value of the minimum wage applicable to the average worker in 2007 was not much lower than in 1997, and was significantly higher than if states had not enacted their own minimum wages. Moreover, the post-2007 federal increases brought the minimum wage faced by the average worker up to a real level not seen since the mid-1980s. An online Appendix table illustrates the extent of state minimum wage variation between 1979 and 2012.

These differences in legislated minimum wages across states and over time are one of two sources of variation that we use to identify the impact of the minimum wage on the wage distribution. The second source of variation we use, following Lee (1999), is variation in the "bindingness" of the minimum wage, stemming from the observation that a given legislated minimum wage should have a larger effect on the shape of the wage distribution in a state with a lower wage level. Table 1 provides examples. In each year, there is significant variation in the percentile of the state wage distribution where the state or federal minimum wage "binds." For instance, in 1979 the minimum wage bound at the twelfth percentile of the female wage distribution for the median state, but it bound at the fifth percentile in Alaska and the twenty-eighth percentile in Mississippi. This variation in the bite or bindingness of the minimum wage was due mainly to cross-state differences in wage levels in 1979, since only Alaska had a state minimum wage that exceeded the federal minimum. In later years, particularly during the 2000s, this variation was also due to differences in the value of state minimum wages.

A. *Sample and Variable Construction*

Our analysis uses the percentiles of states' annual wage distributions as the primary outcomes of interest. We form these samples by pooling all individual responses from the Current Population Survey Merged Outgoing Rotation Group (CPS MORG) for each year. We use the reported hourly wage for those who report being paid by the hour. Otherwise we calculate the hourly wage as weekly earnings divided by hours worked in the prior week. We limit the sample to individuals age 18 through 64, and we multiply top-coded values by 1.5. We exclude self-employed individuals and those with wages imputed by the BLS. To reduce the influence of outliers, we Winsorize the top two percentiles of the wage distribution in each state, year, and sex grouping (male, female, or pooled) by assigning the ninety-seventh percentile value to the ninety-eighth and ninety-ninth percentiles. Using these individual wage data, we calculate all percentiles of state wage distributions by sex for 1979–2012, weighting individual observations by their CPS sampling weight multiplied by their weekly hours worked.[5] For more details on our data construction, see the data Appendix.

Our primary analysis is performed at the state-year level, but minimum wages often change part way through the year. We address this issue by assigning the value of the minimum wage that was in effect for the longest time throughout the calendar

[5] Following the approach introduced by DFL (1996), now used widely in the wage inequality literature, we define percentiles based on the distribution of paid hours, thus giving equal weight to each paid hour worked. Our estimates are essentially unchanged if we weight by workers rather by worker hours.

year in a state and year. For those states and years in which more than one minimum wage was in effect for six months in the year, the maximum of the two is used. We have alternatively assigned the maximum of the minimum wage within a year as the applicable minimum wage. This leaves our conclusions unchanged.

II. Reduced Form Estimation of Minimum Wage Effects on the Wage Distribution

A. *General Specification and OLS Estimates*

The general model we estimate for the evolution of inequality at any point in the wage distribution (the difference between the log wage at the *p*th percentile and the log of the median) for state *s* in year *t* is of the form:

$$(1) \quad w_{st}(p) - w_{st}(50) = \beta_1(p)\left[w_{st}^m - w_{st}(50)\right] + \beta_2(p)\left[w_{st}^m - w_{st}(50)\right]^2 + \sigma_{s0}(p) + \sigma_{s1}(p) \times time_t + \gamma_t^{\sigma}(p) + \varepsilon_{st}^{\sigma}(p).$$

In this equation, $w_{st}(p)$ represents the log real wage at percentile p in state s at time t; time-invariant state effects are represented by $\sigma_{s0}(p)$; state-specific trends are represented by $\sigma_{s1}(p)$; time effects represented by $\gamma_t^{\sigma}(p)$; and transitory effects represented by $\varepsilon_{st}^{\sigma}(p)$, which we assume to be independent of the state and year effects and trends. Although our state effects and trends are likely to control for much of the economic fluctuations at state level, we also experimented with including the state-level unemployment rate as a control variable. This has virtually no impact on the estimated coefficients in equation (1) for any of our samples.

In equation (1), w_{st}^m is the log minimum wage for that state-year. We follow Lee (1999) in both defining the bindingness of the minimum wage to be the log difference between the minimum wage and the median (Lee refers to this as the effective minimum) and in modeling the impact of the minimum wage to be quadratic. The quadratic term is important to capture the idea that a change in the minimum wage is likely to have more impact on the wage distribution where it is more binding.[6] By differentiating (1) we have that the predicted impact of a change in the minimum wage on a percentile is given by $\beta_1(p) + 2\beta_2(p)\left[w_{st}^m - w_{st}(50)\right]$. Inspection of this expression shows how our specification captures the idea that the minimum wage will have a larger effect when it is high relative to the median.

Our preferred strategy for estimating (1) is to include state fixed effects and trends and to instrument the minimum wage.[7] But we start by presenting OLS estimates

[6] In this formulation, a more binding minimum wage is a minimum wage that is closer to the median, resulting in a higher (less negative) effective minimum wage. Since the log wage distribution has greater mass toward its center than at its tail, a 1 log point rise in the minimum wage affects a larger fraction of wages when the minimum lies at the fortieth percentile of the distribution than when it lies at the first percentile.

[7] Our primary specification does not control for other state-level controls. When we include state-year unemployment rates to proxy for heterogeneous shocks to a state's labor market, however, the coefficients on the minimum wage variables are essentially unchanged.

Table 2A—OLS and 2SLS Relationship between log(p) − log(p50) and log(min. wage) − log(p50), for Select Percentiles of Given Wage Distribution, 1979–2012

	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)	OLS Lee Spec. (5)
Panel A. Females					
p(5)	0.44***	0.54***	0.32***	0.39***	0.63***
	(0.03)	(0.05)	(0.04)	(0.05)	(0.04)
p(10)	0.27***	0.46***	0.22***	0.17***	0.52***
	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
p(20)	0.12***	0.29***	0.10**	0.07**	0.29***
	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
p(30)	0.07***	0.23***	0.02	0.04	0.15***
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)
p(40)	0.04**	0.17***	0.00	0.03	0.06***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)
p(75)	0.09***	0.23***	−0.03	0.00	−0.05**
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
p(90)	0.15***	0.34***	−0.02	0.04	−0.04
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Var. of log wage	0.07	0.04	−0.02	−0.09	−0.20***
	(0.04)	(0.05)	(0.08)	(0.07)	(0.03)
Panel B. Males					
p(5)	0.25***	0.43***	0.19***	0.16***	0.55***
	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)
p(10)	0.12***	0.34***	0.05	0.05*	0.38***
	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)
p(20)	0.06**	0.23***	0.02	0.02	0.21***
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
p(30)	0.04	0.19***	0.02	0.00	0.09***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
p(40)	0.06***	0.15***	0.05*	0.02	0.03***
	(0.01)	(0.02)	(0.02)	(0.04)	(0.01)
p(75)	0.14***	0.23***	0.00	0.02	0.09**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
p(90)	0.16***	0.30***	0.03	0.04	0.14**
	(0.03)	(0.03)	(0.03)	(0.04)	(0.07)
Var. of log wage	0.03	0.01	−0.07	−0.06	−0.12**
	(0.03)	(0.05)	(0.05)	(0.07)	(0.05)
Levels/first-differenced	Levels	FD	Levels	FD	Levels
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	No
State trends	Yes	No	Yes	No	No

Notes: See text at bottom of Table 2B.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

of (1).[8] Column 1 of Tables 2A and 2B reports estimates of this specification. We report the marginal effects of the effective minimum for selected percentiles when

[8] Strictly speaking our OLS estimates are weighted least squares and our IV estimates weighted two-stage least squares.

Table 2B—OLS and 2SLS Relationship between log(p) − log(p50) and log(min. wage) − log(p50), for Select Percentiles of Pooled Wage Distribution, 1979–2012

	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)	OLS Lee Spec. (5)
Panel C. Males and females pooled					
$p(5)$	0.35***	0.45***	0.29***	0.29***	0.62***
	(0.03)	(0.04)	(0.03)	(0.06)	(0.03)
$p(10)$	0.17***	0.35***	0.16***	0.17***	0.44***
	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)
$p(20)$	0.08***	0.23***	0.07***	0.04	0.25***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
$p(30)$	0.05***	0.19***	0.02	0.00	0.14***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
$p(40)$	0.04***	0.16***	0.02**	0.02	0.06***
	(0.01)	(0.02)	(0.01)	(0.03)	(0.01)
$p(75)$	0.11***	0.22***	0.00	0.01	0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
$p(90)$	0.15***	0.28***	0.01	0.02	0.04
	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)
Var. of log wage	0.04	0.02	−0.05	−0.07	−0.18***
	(0.02)	(0.03)	(0.05)	(0.05)	(0.04)
Levels/first-differenced	Levels	FD	Levels	FD	Levels
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	No
State trends	Yes	No	Yes	No	No

Notes: $N = 1{,}700$ for levels estimation, $N = 1{,}650$ for first-differenced estimation. Sample period is 1979–2012. For all but the last row, the dependent variable is log(p) − log(p50), where p is the indicated percentile. For the last row, the dependent variable is the variance of log wage. Estimates are the marginal effects of log(min. wage) − log(p50), evaluated at its hours-weighted average across states and years. The last row are estimates of the marginal effects of log(min. wage) − log(p50), evaluated at its hours-weighted average across states and years. Standard errors clustered at the state level are in parentheses. Regressions are weighted by the sum of individuals' reported weekly hours worked multiplied by CPS sampling weights. For 2SLS specifications, the effective minimum and its square are instrumented by the log of the minimum, the square of the log minimum, and the log minimum interacted with the average real log median for the state over the sample. For the first-differenced specification, the instruments are first-differenced equivalents.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

estimated at the weighted average of the effective minimum over all states and all years between 1979 and 2012. In the final row we also report an estimate of the effect on the variance, though the upper tail will heavily influence this estimate. Figure 3 provides a graphical representation of these estimated marginal effects for all percentiles. In all three samples (males, females, pooled), there is a significant estimated effect of the minimum wage on the lower tail, but, rather worryingly, there is also a large positive relationship between the effective minimum wage and *upper tail* inequality. This suggests there is some bias in these estimates. This problem also occurs when we estimate the model with first-differences in column 2.

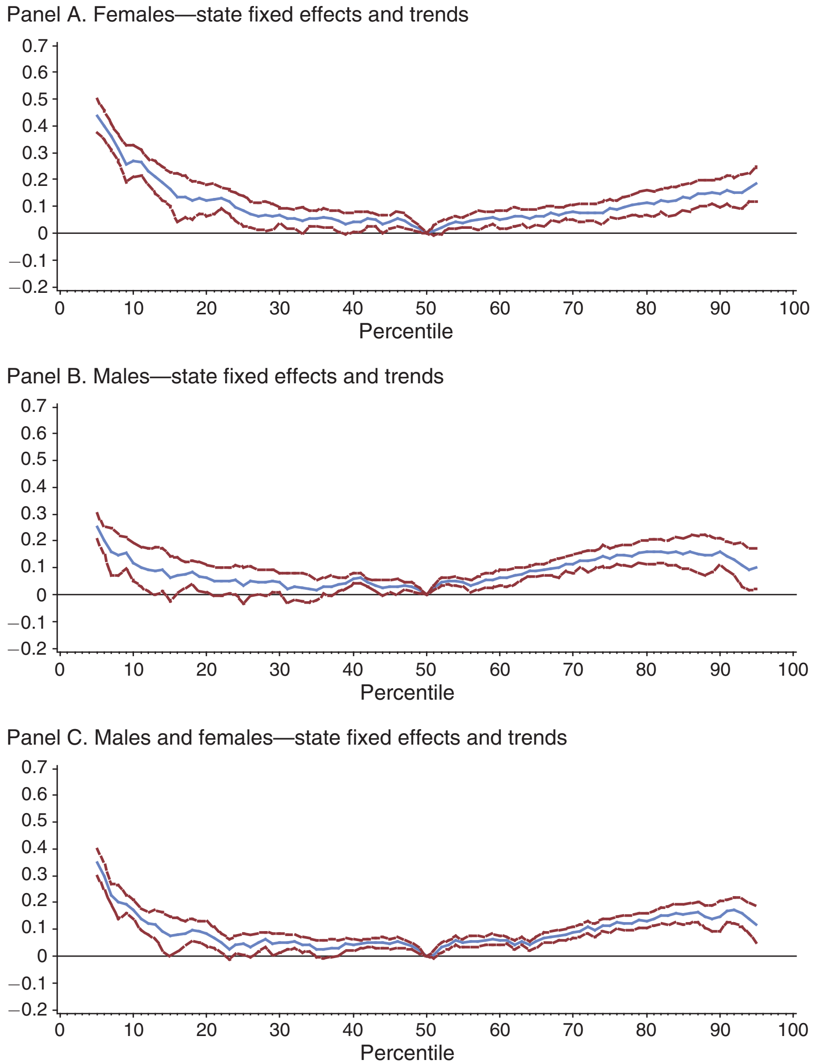


Figure 3. OLS Estimates of the Relationship between log(p) – log($p50$) and log(min) – log($p50$) and Its Square, 1979–2012

Notes: Estimates are the marginal effects of log(min. wage) – log($p50$), evaluated at the hours-weighted average of log(min. wage) – log($p50$) across states and years. Observations are state-year observations. Ninety-five percent confidence interval is represented by the dashed lines. Estimates correspond with column 1 of Tables 2A and 2B.

In discussing the possible causes of bias in estimates, it is helpful to consider the following model for the median log wage for state s in year t:

$$w_{st}(50) = \mu_{s0} + \mu_{s1} \times time_t + \gamma_t^{\mu} + \varepsilon_{st}^{\mu}. \tag{2}$$

Here, the median wage for the state is a function of a state effect, μ_{s0}; a state trend, μ_{s1}; a common year effect, γ_t^{μ}; and a transitory effect, ε_{st}^{μ}. With this setup, OLS estimation of (1) will be biased if $\mathrm{cov}(\varepsilon_{st}^{\mu}, \varepsilon_{st}^{\sigma}(p))$ is nonzero because the median is used in the construction of the effective minimum; that is, transitory fluctuations in state wage medians are correlated with the gap between the state wage median and other wage percentiles. Is this bias likely to be present in practice? One would naturally expect that transitory shocks to the median do not translate one-for-one to other percentiles. If, plausibly, the effects dissipate as one moves further from the median, this would generate bias due to the nonzero correlation between shocks to the median wage and measured inequality throughout the distribution. This implies that we would expect $\text{cov}\left(\varepsilon_{st}^{\mu}, \varepsilon_{st}^{\sigma}(p)\right) < 0$ and that this covariance would attenuate as one considers percentiles further from the median.

How does this covariance affect estimates of equation (1)? This depends on the covariance of the effective minimum wage terms with the errors in the equation. The natural assumption is that $\text{cov}\left(w_{st}^{m} - w_{st}(50), w_{st}(50)\right) < 0$, that is, even after allowing for the fact that high-wage states may have a state minimum higher than the federal minimum, the minimum wage is less binding in high-wage states. Combining this with the assumption that $\text{cov}\left(\varepsilon_{st}^{\mu}, \varepsilon_{st}^{\sigma}(p)\right) < 0$ leads to the prediction that OLS estimation of (1) leads to upward bias in the estimate of the impact of minimum wages on inequality in both the lower and upper tail.

We will address this problem by applying instrumental variables to purge biases caused by measurement error and other transitory shocks, following the approach introduced by Durbin (1954). We instrument the observed effective minimum and its square using an instrument set that consists of: (i) the log of the real statutory minimum wage, (ii) the square of the log of the real minimum wage, and (iii) the interaction between the log minimum wage and average log median real wage for the state over the sample period. In this IV specification, identification in (1) for the linear term in the effective minimum wage comes entirely from the variation in the statutory minimum wage, and identification for the quadratic term comes from the inclusion of the square of the log statutory minimum wage and the interaction term.[9] As there are always time effects included in our estimation, all the identifying variation in the statutory minimum comes from the state-specific minimum wages, which we assume to be exogenous to state wage levels of inequality.[10] Our second instrument is the square of the predicted value for the effective minimum from the regression outlined above, and relies on the same identifying assumptions (exogeneity of the statutory minimum wage).

Column 3 of Tables 2A and 2B report the estimates when we instrument the effective minimum in the way we have described. The first-stages for these regressions are reported in Appendix Table A1. For all samples, the three instruments are

[9] To see why the interaction is important to include, expand the square of the effective minimum wage, $\log(\text{min}) - \log(p50)$, which yields three terms, one of which is the interaction of $\log(\text{min})$ and $\log(p50)$. We have also tried replacing the square and interaction terms with the square of the predicted value for the effective minimum, where the predicted value is derived from a regression of the effective minimum on the log statutory minimum, state and time fixed effects, and state trends (similar to an approach suggested by Wooldridge 2002; section 9.5.2). 2SLS results using this alternative instrument are virtually identical to the strategy outlined in the main text. In general, using the statutory minimum as an instrument is similar in spirit to the approach taken by Card, Katz, and Krueger (1993) in their analysis of the employment effects of the minimum wage.

[10] We follow almost all of the existing literature and assume the state level minimum wages are exogenous to other factors affecting the state-level wage distribution once we have controlled for state fixed effects and trends. A priori, any bias is unclear, e.g., rising inequality might generate a demand for higher minimum wages as might economic conditions favorable to minimum wage workers. The long lags in the political process surrounding rises in the minimum wages makes it unlikely that there is much response to contemporaneous economic conditions.

jointly highly significant and pass standard diagnostic tests for weak instruments (e.g., Stock, Wright, and Yogo 2002). Compared to column 1, the estimated impacts of the minimum wage in the lower tail are reduced, especially above the tenth percentile. This is consistent with what we have argued is the most plausible direction of bias in the OLS estimate in column 1. And, for all three samples, the estimated effect in the upper tail is now small and insignificantly different from zero, again consistent with the IV strategy reducing bias in the predicted direction.[11]

For robustness, we also estimate these models in first differences. Column 4 shows the results from first-differenced regressions that include state and year fixed effects, instrumenting the endogenous differenced variables using differenced analogues to the instruments described above.[12] Figure 4A shows the results for all percentiles from the level IV specifications; Figure 4B shows the results from the first-differenced IV specifications. Qualitatively, the first-differenced regressions are quite similar to the levels regressions, although they imply slightly larger effects of the minimum wage at the bottom of the wage distribution.

Our 2SLS estimates find that the minimum wage affects lower tail inequality up through the twenty-fifth percentile for women, up through the tenth percentile for men, and up through approximately the fifteenth percentile for the pooled wage distribution. A 10 log point increase in the effective minimum wage reduces 50/10 inequality by approximately 2 log points for women, by no more than 0.5 log points for men, and by roughly 1.5 log points for the pooled distribution. These estimates are less than half as large as those found by the baseline OLS specification, and are considerably smaller than those reported by Lee (1999). What accounts for this qualitative difference in findings? The dissimilarity could stem either from differences in specification and estimation or from the additional years of data available for our analysis. We consider both factors in turn, and show that the first—differences in specification and estimation—is fundamental.

B. *Reconciling with Literature: Methods or Time Period?*

Lee (1999) estimates equation (1) by OLS and his preferred specification excludes the state fixed effects and trends that we have included.[13] Column 5 of Tables 2A and 2B, and Figure 5, shows what happens when we estimate this model on our longer sample period. Similar to Lee, we find large and statistically significant effects of the minimum wage on the lower percentiles of the wage distribution that extend throughout *all* percentiles below the median for the male, female, and pooled wage distributions, and are much larger than the effects in our preferred specifications. Also note that, with the exception of the male estimates, the upper tail "effects" are small and insignificantly different from zero, which might be considered a necessary

[11] These findings are essentially unchanged if we use higher order state time trends.

[12] The instruments for the first-differenced analogue are Δw_{st}^m and $\Delta(w_{st}^m - \tilde{w}(50)_{st})^2$, where Δw_{st}^m represents the annual change in the log of the legislated minimum wage, and $\Delta(w_{st}^m - \tilde{w}(50)_{st})^2$ represents the change in the square of the predicted value for the effective minimum wage.

[13] We include time effects in all of our estimation, as does Lee (1999). We estimate the model separately for each p (from 1 to 99), and impose no restrictions on the coefficients or error structure across equations.

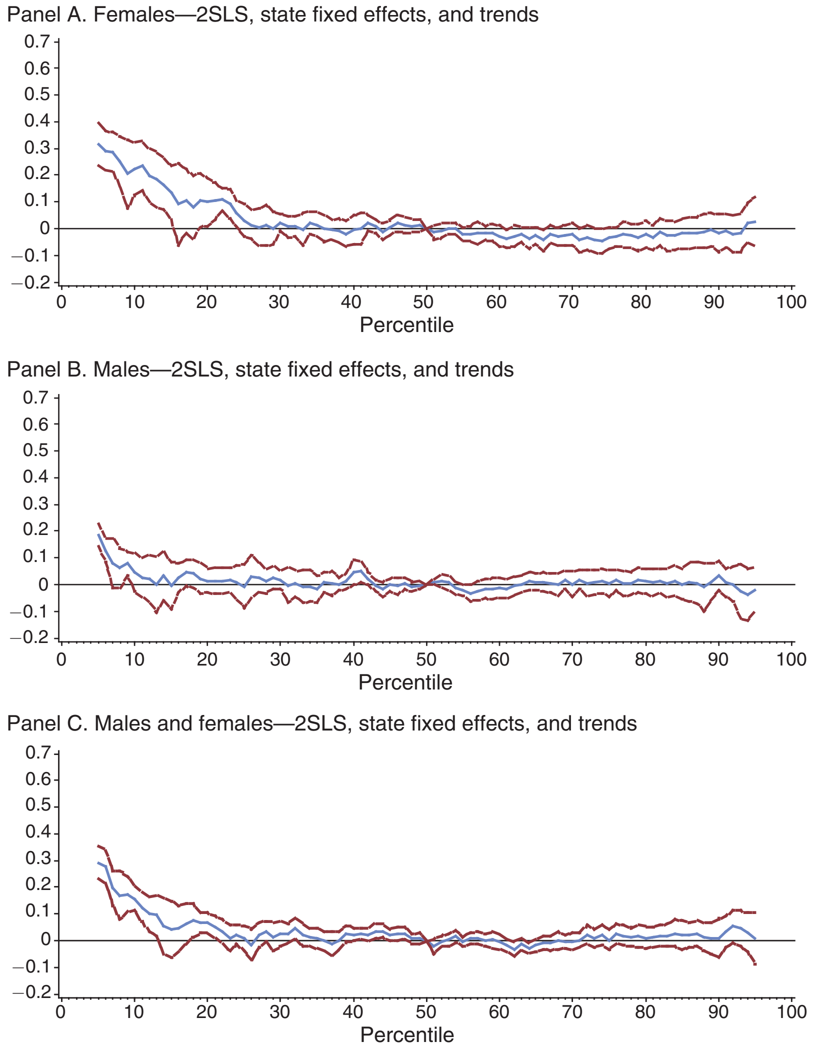


FIGURE 4A. 2SLS ESTIMATES OF THE RELATIONSHIP BETWEEN LOG(p) – LOG($p50$) AND LOG(MIN) – LOG($p50$) AND ITS SQUARE, 1979–2012

Notes: Estimates are the marginal effects of log(min. wage) – log($p50$), evaluated at the hours-weighted average of log(min. wage) – log($p50$) across states and years. Observations are state-year observations. Ninety-five percent confidence interval is represented by the dashed lines. Estimates correspond with column 3 of Tables 2A and 2B.

condition for the results to be credible estimates of the impact of the minimum wage on wage inequality at any point in the distribution.

These estimates are likely to suffer from serious biases, however. If state fixed effects and trends are omitted from the specification of (1), estimates of minimum wage effects on wage inequality will be biased if $(\sigma_{s0}(p), \sigma_{s1}(p))$ is correlated with (μ_{s0}, μ_{s1}), that is, state log median wage levels and latent state log wage inequality are correlated. Lee (1999) is very clear that his specification relies on the assumption of a zero correlation between the level of median wages and inequality. This assumption can be tested if one has a measure of inequality that is unlikely to be

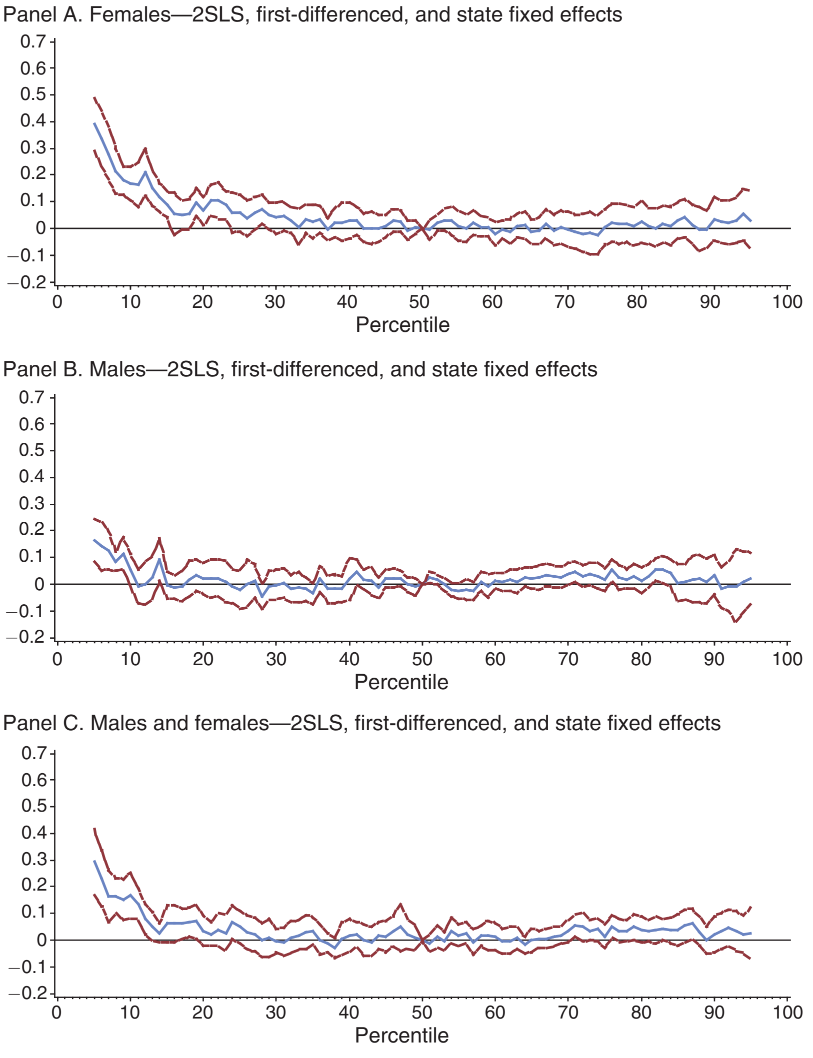


Figure 4B. 2SLS Estimates of the Relationship between $\log(p) - \log(p50)$ and $\log(\text{min}) - \log(p50)$ in First-Differences, 1979–2012

Notes: Estimates are the marginal effects of log(min. wage) − log(p50), evaluated at the hours-weighted average of log(min. wage) − log(p50) across states and years. Observations are state-year observations. Ninety-five percent confidence interval is represented by the dashed lines. Estimates correspond with column 4 of Tables 2A and 2B.

affected by the level of the minimum wage. For this purpose we use 60/40 inequality, that is, the difference in the log of the sixtieth and fortieth percentiles. Given that the minimum wage never binds very far above the tenth percentile of the wage distribution over our sample period, we feel comfortable assuming that the minimum wage has no impact on percentiles 40 through 60. Under this maintained hypothesis, 60/40 inequality serves as a valid proxy for the underlying inequality of a state's wage distribution.

To assess whether either the level or trend of state latent inequality is correlated with average state wage levels or their trends, we estimate state-level regressions

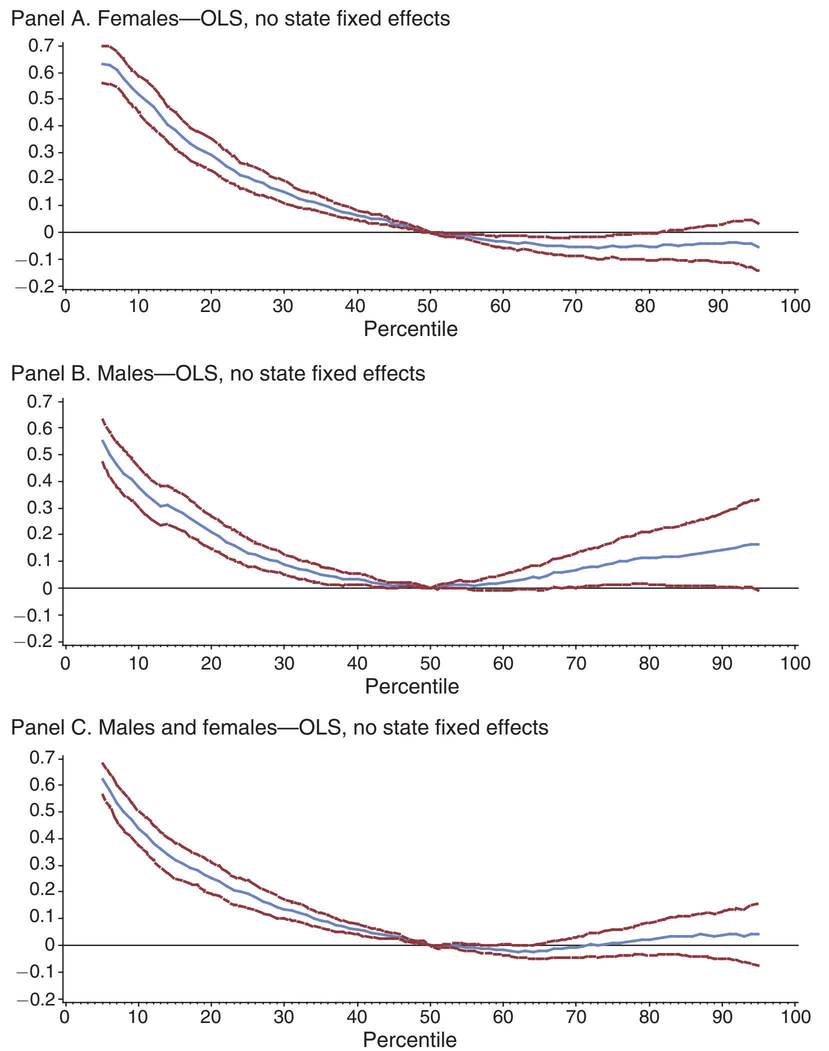


FIGURE 5. 2SLS ESTIMATES OF THE RELATIONSHIP BETWEEN LOG(p) – LOG(p50) AND LOG(MIN) – LOG(p50) AND ITS SQUARE USING LEE (1999) SPECIFICATION, 1979–2012

Notes: Estimates are the marginal effects of log(min. wage) – log(p50), evaluated at the hours-weighted average of log(min. wage) – log(p50) across states and years. Observations are state-year observations. Ninety-five percent confidence interval is represented by the dashed lines. Estimates correspond with column 5 of Tables 2A and 2B.

of average 60/40 inequality and estimated trends in 60/40 inequality on average median wages and trends in median wages. Figures 6A and 6B depict scatter plots of these regressions, with regression results reported in Appendix Table A1. Figure 6A depicts the cross-state relationship between the average log(p60)–log(p40) and the average log(p50) for each of our three samples. Figure 6B depicts the cross-state relationship between the trends in the two measures. In all cases but the male trends plot (panel B of Figure 6B), there is a strong, positive visual relationship between the two—and, even for the male trend scatter, there is, in fact, a statistically significant positive relationship between the trends in the log(p60)–log(p40) and log(p50).

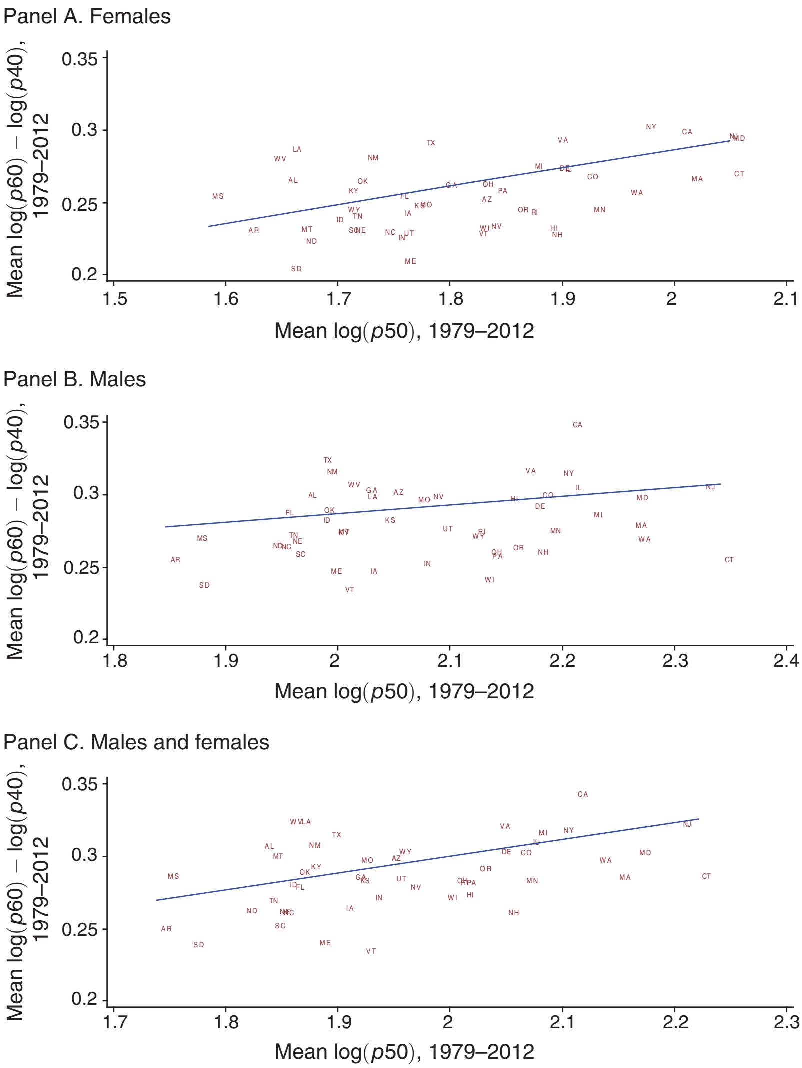


Figure 6A. OLS Estimates of the Relationship between Mean log(p60) – log(p40) and Mean log(p50), 1979–2012

Notes: Estimates correspond with regressions from Appendix Table A2. The figures show the cross-state relationship between the average log(p60) – log(p40) and log(p50) between 1979 and 2012. Alaska, which tends to be an outlier, is dropped for visual clarity, though this does not materially affect the slope of the line (Appendix Table A2 includes Alaska).

The finding of a positive correlation between underlying inequality and the state median implies there is likely to be omitted variable bias from the exclusion of state fixed effects and trends—specifically, an *upward* bias to the estimated minimum wage effect in the lower tail and, simultaneously, a *downward* bias in the upper tail. To see why, note that higher wage states have lower (more negative) effective minimum wages (defined as the log gap between the legislated minimum and the state median), and the results from Table 2 imply that these states also have higher levels of latent

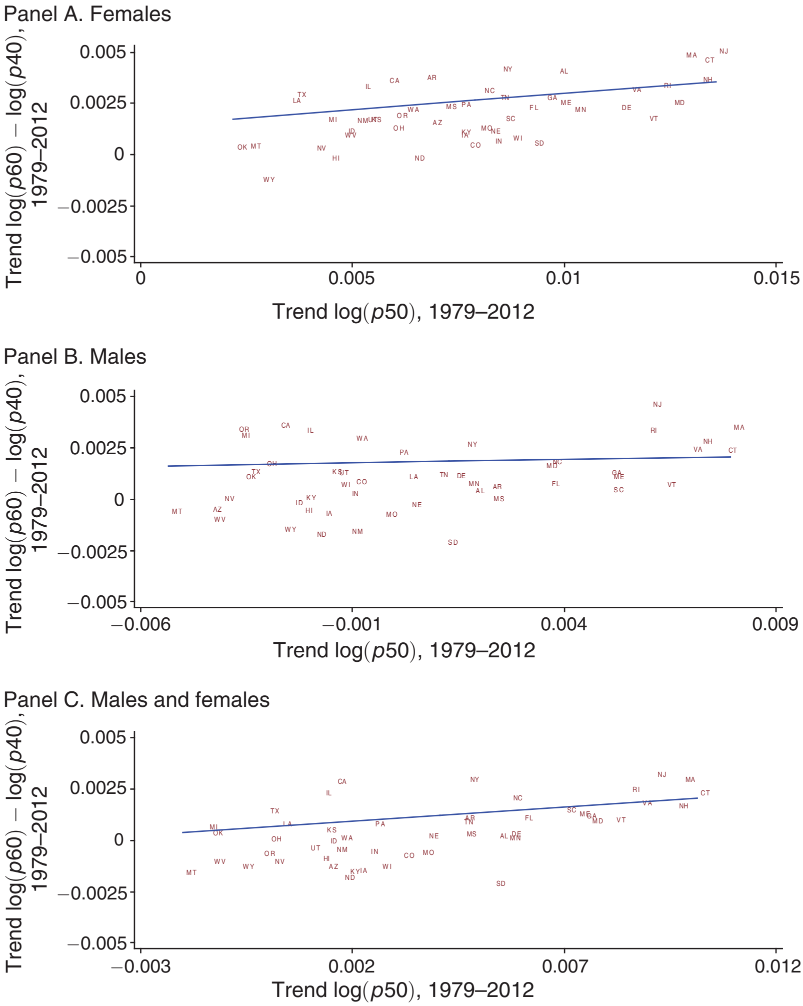


Figure 6B. OLS Estimates of the Relationship between Trend $\log(p60) - \log(p40)$ and Trend $\log(p50)$, 1979–2012

Notes: Estimates correspond with regressions from Appendix Table A2. The figures show the cross-state relationship between the trend in $\log(p60) - \log(p40)$ and the trend in $\log(p50)$ between 1979 and 2012. Alaska, which tends to be an outlier, is dropped for visual clarity, though this does not materially affect the slope of the line (Appendix Table A2 includes Alaska).

inequality; thus they will have a more negative value of the left-hand side variable in our main estimating equation (1) for percentiles below the median, and a more positive value for percentiles above the median. Since the state median enters the right-hand side expression for the effective minimum wage with a negative sign, estimates of the relationship between the effective minimum and wage inequality will be *upward-biased* in the lower tail and *downward-biased* in the upper tail.

Combined with our discussion above on potential biases stemming from the correlation between the transitory error components on both sides of equation (1),

Table 3—Actual and Counterfactual Changes in log($p50/10$) between Selected Years: Changes in log Points (100 × log Change)

		2SLS counterfactuals		OLS counterfactuals	
				Levels, No FE	
	Observed change	Levels FE 1979–2012	First diffs 1979–2012	1979–2012	1979–1991
	(1)	(2)	(3)	(4)	(5)
Panel A. 1979–1989					
Females	24.6	11.3***	15.1***	2.9	4.3***
		(3.7)	(1.8)	(2.0)	(1.3)
Males	2.5	1.2	1.4	−6.5***	−5.3***
		(1.4)	(0.9)	(1.4)	(1.6)
Pooled	11.8	6.7***	8.1***	−1.2	0.0
		(1.8)	(0.8)	(1.3)	(1.2)
Panel B. 1979–2012					
Females	28.5	14.8***	18.6***	6.4***	7.4***
		(3.7)	(1.8)	(1.9)	(1.3)
Males	7.9	7.0***	7.1***	3.1***	3.6***
		(1.0)	(0.7)	(1.0)	(0.9)
Pooled	11.4	6.9***	7.9***	1.1	1.8**
		(1.5)	(0.7)	(1.2)	(0.8)

Notes: Estimates represent changes in actual and counterfactual log($p50$) − log($p10$) between 1979 and 1989, and 1979 and 2012, measured in log points (100 × log change). Counterfactual wage changes in panel A represent counterfactual changes in the 50/10 had the effective minimum wage in 1979 equaled the effective minimum wage in 1989 for each state. Counterfactual wage changes in panel B represent changes had the effective minima in 1979 and 2012 equaled the effective minimum in 1989. The 2SLS counterfactuals (using point estimates from the 1979–2012 period) are formed using coefficients from estimations reported in columns 3 and 4 of Tables 2A and 2B. The OLS counterfactual estimates (using point estimates from the 1979–2012 period) are formed using coefficients from estimations reported in column 5 of Table 2. Counterfactuals using point estimates from the 1979–1991 period are formed using coefficients from analogous regressions for the shorter sample period. Marginal effects are bootstrapped as described in the text; the standard deviation associated with the estimates is reported in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

which leads to an *upward* bias on the coefficient on the effective minimum wage in both lower and upper tails, we infer that these two sources of bias reinforce each other in the lower tail, likely leading to an *overestimate* of the impact of the minimum wage on lower tail inequality. Simultaneously, they have countervailing effects on the upper tail. Thus, our finding in the fifth column of Table 2 of a relatively weak relationship between the effective minimum wage and upper tail inequality (for the female and pooled samples) may arise because these two countervailing sources of bias largely offset one another for upper tail estimates. But since these biases are reinforcing in the lower tail of the distribution, the absence of an upper tail correlation is not sufficient evidence for the absence of lower tail bias, implying that Lee's (1999) preferred specification may suffer from upward bias.

The original work assessing the impact of the minimum wage on rising US wage inequality—including DFL (1996), Lee (1999), and Teulings (2000, 2003)—used data from 1979 through the late 1980s or early 1990s. Our primary estimates exploit an additional 21 years of data. Does this longer sample frame make a substantive difference? Figure 7 answers this question by plotting estimates of marginal

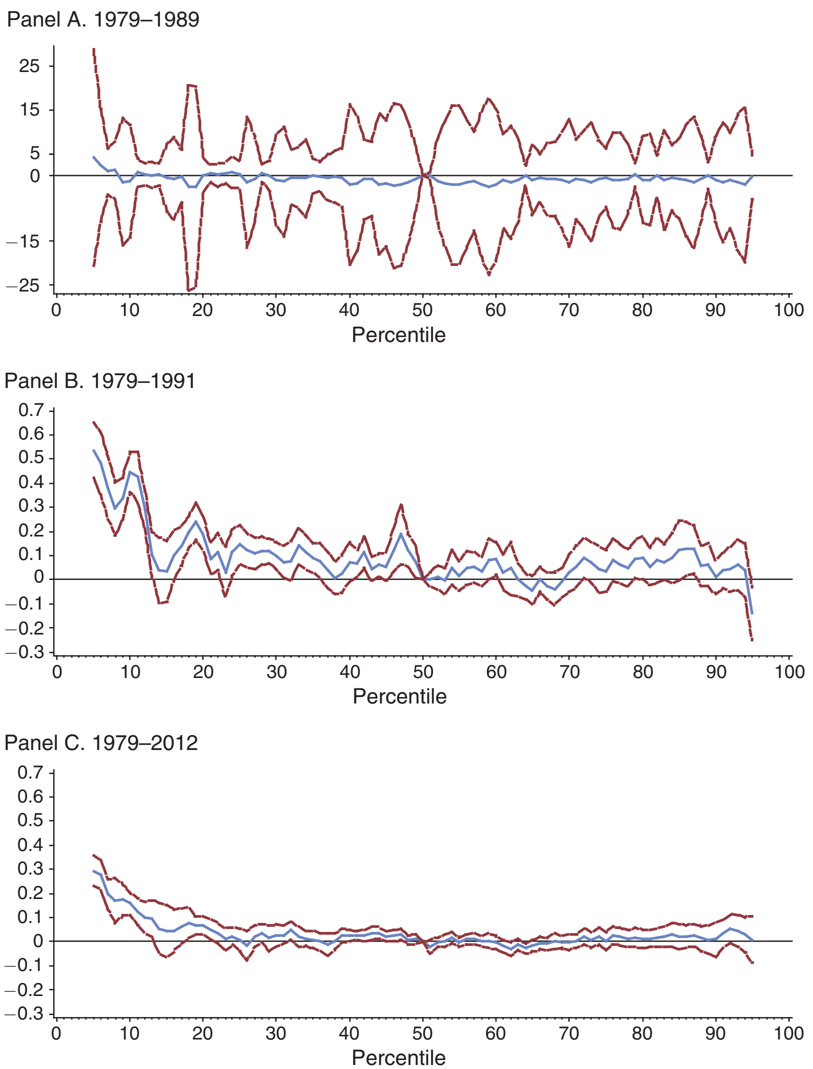


Figure 7. 2SLS Estimates of the Relationship between log(p) − log(p50) and log(min) − log(p50) over Various Time Periods (*males and females*)

Notes: Estimates are the marginal effects of log(min. wage) − log(p50), evaluated at the hours-weighted average of log(min. wage) − log(p50) across states and years. Observations are state-year observations. The 95 percent confidence interval is represented by the dashed lines. Estimates correspond with column 3 of Tables 2A and 2B.

effects of the effect of minimum wage on percentiles of the pooled male and female wage distribution (as per column 3 of Table 2) for each of three time periods: 1979–1989, when there was little state-level variation in the minimum wage; 1979–1991, incorporating an additional two years in which numerous states raised their minimum wage; and 1979–2012. Panel A of Figure 7 reveals that our IV strategy—which relies on variation in statutory minimum wages across states and over time—does *not* perform well when limited to data only from the 1980s period: the point estimates are enormous relative to both OLS estimates and 2SLS estimates; and the confidence bands are extremely large (note that the scale in the figure runs from −25 to 25, more than an order of magnitude larger than even the largest point estimates in Table 2). This lack of statistical significance is not surprising in light of the small number of policy changes in this period: between 1979 and 1985, only one state aside from Alaska adopted a minimum wage in excess of the federal minimum; the ten additional adoptions through 1989 all occurred between 1986 and 1989 (Table 1). Consequently, when calculating counterfactuals below, we apply marginal effects estimates obtained using additional years of data.

By extending the estimation window to 1991 in panel B of Figure 7 (as was also done by Lee 1999), we exploit the substantial federal minimum wage increase that took place between 1990 and 1991. This federal increase generated numerous cross-state contrasts since nine states had by 1989 raised their minimums above the 1989 federal level and below the 1991 federal level (and an additional three raised their minimum to $4.25, which would be the level of the 1991 federal minimum wage). As panel B underscores, including these two additional years of data dramatically reduces the standard errors around our estimates, though the estimated marginal effects on a particular percentile are still quite noisy. Adding data for the full sample through 2012 (panel C of Figure 7) reduces the standard errors further and helps smooth out estimated marginal effects across percentiles.

Comparing across the three panels of Figure 7 reveals that it would have been infeasible using data prior to 1991 to successfully estimate the effect of the minimum wage on the wage distribution. It is only with subsequent data on comovements in state wage distributions and the minimum wage that more accurate estimates can be obtained. For this reason, our primary counterfactual estimates of changes in inequality rely on coefficient estimates from the full sample. We also discuss below the robustness of our substantive findings to the use of shorter sample windows (1979–1989 and 1979–1991).

III. Counterfactual Estimates of Changes in Inequality

How much of the expansion in lower tail wage inequality since 1979 can be explained by the declining minimum wage? Following Lee (1999), we present reduced form counterfactual estimates of the change in *latent* wage inequality absent the decline in the minimum wage—that is, the change in wage inequality that would have been observed had the minimum wage been held at a constant real benchmark. These reduced form counterfactual estimates do not distinguish between mechanical and spillover effects of the minimum wage, a topic that we analyze next. We consider counterfactual changes over two periods: 1979–1989 (which captures the large widening of lower-tail income inequality over the 1980s) and 1979–2012.

To estimate changes in latent wage inequality, Lee (1999) proposes the following simple procedure. For each observation in the dataset, calculate its rank in its respective state-year wage distribution. Then, adjust each wage by the quantity:

$$\Delta w_{st}(p) = \hat{\beta}_1(p)\left(\tilde{m}_{s,\tau_0} - \tilde{m}_{s,\tau_1}\right) + \hat{\beta}_2(p)\left(\tilde{m}^2_{s,\tau_0} - \tilde{m}^2_{s,\tau_1}\right), \tag{3}$$

where $\tilde{m}_{s,\tau 1}$ is the observed end-of period effective minimum in state s in some year $\tau 1$, $\tilde{m}_{s,\tau 0}$ is the corresponding beginning-of-period effective minimum in $\tau 0$, and $\hat{\beta}_1(p)$, $\hat{\beta}_2(p)$ are point estimates from the OLS and 2SLS estimates in Table 2 (columns 1, 4, or 5).[14] We pool these adjusted wage observations to form a counterfactual national wage distribution, and we compare changes in inequality in the simulated distribution to those in the observed distribution.[15] We compute standard errors by bootstrapping the estimates within the state-year panel.[16]

The first column of the upper panel of Table 3 shows that between 1979 and 1989, the female 50/10 log wage ratio increased by nearly 25 log points. Applying the coefficient estimates on the effective minimum and its square obtained using the 2SLS model fit to the female wage data for 1979 through 2012 (column 2 of panel A), we calculate that had the minimum wage been constant at its real 1989 level throughout this period, female 50/10 inequality would counterfactually have risen by 11.3 log points. Using the first differences specification (column 3), we estimate a counterfactual rise of 15.1 log points. Thus, the minimum wage can explain between 40 and 55 percent of the observed rise in equality, with the complement due to a rise in underlying inequality. These are nontrivial effects, of course, and they confirm, in accordance with the visual evidence in Figure 1, that the falling minimum wage contributed meaningfully to rising female lower-tail inequality during the 1980s and early 1990s.

The OLS estimates preferred by Lee (1999) find a substantially larger role for the minimum wage, however. Using the OLS model fit to the female wage data for 1979 through 2012 (column 4 of panel A), we calculate that female 50/10 inequality would counterfactually have risen by only 2.9 log points. Applying the coefficient estimates for only the 1979–1991 period (column 5), female 50/10 inequality would have risen by 4.3 log points. Thus, consistent with Lee (1999), the OLS estimate implies that the decline in the real minimum wage can account for the bulk (all but 3 to 4 of 25 log points) of the expansion of lower tail female wage inequality in this period.

The second and third rows of Table 3 calculate the effect of the minimum wage on male and pooled gender inequality. Here, the discrepancy between IV and OLS-based counterfactuals is even more pronounced. 2SLS models indicate that the minimum wage makes a modest contribution to the rise in male wage inequality and explains only about 30 to 40 percent of the rise in pooled gender inequality. By contrast, OLS estimates imply that the minimum wage *more than* fully explains both

[14] So, for example, taking $\tau_0 = 1979$ and $\tau_1 = 1989$, and subtracting Δw_{st}^{p} from each observed wage in 1979 would adjust the 1979 distribution to its counterfactual under the realized effective minima in 1989.

[15] We use states' observed median wages when calculating $\tilde{m}$ rather than the national median deflated by the price index as was done by Lee (1999). This choice has no substantive effect on the results but appears most consistent with the identifying assumptions.

[16] Our bootstrap takes states as the sampling unit, and thus we start by drawing 50 states with replacement. We next estimate the models in Tables 2A and 2B for the selected states using the percentile estimates and sample weights from the full dataset and, finally, apply the coefficients to the full CPS individual-level sample to calculate the counterfactual in equation (3). Table 3 reports the mean and standard deviation of 1,000 replications of this counterfactual exercise.

the rise in male 50/10 inequality and the rise in pooled 50/10 inequality between 1979 and 1989.

Despite their substantial discrepancy with the OLS models, the 2SLS estimates appear highly plausible. Figure 2 shows that the minimum wage was nominally *nonbinding* for males throughout the sample period, with fewer than 6 percent of all male wages falling at or below the relevant minimum wage in any given year. For the pooled gender distribution, the minimum wage had somewhat more bite, with a bit more than 8 percent of all hours paid at or below the minimum in the first few years of the sample. But this is modest relative to its position in the female distribution, where 9 to 13 percent of wages were at or below the relevant minimum in the first 5 years of the sample. Consistent with these facts, 2SLS estimates indicate that the falling minimum wage generated a sizable increase in female wage inequality, a modest increase in pooled gender inequality, and a minimal increase in male wage inequality.

Panel B of Table 3 calculates counterfactual (minimum wage constant) changes in inequality over the full sample interval of 1979–2012. In all cases, the contribution of the minimum wage to rising inequality is smaller when estimated using 2SLS in place of OLS models, and its impacts are substantial for females, modest for the pooled distribution, and negligible for males.

Figure 8 and the top panel of Figure 9 provide a visual comparison of observed and counterfactual changes in male, female, and pooled-gender wage inequality during the critical period of 1979 through 1989, during which time the minimum wage remained nominally fixed, while lower tail inequality rose rapidly for all groups. As per Lee (1999), the OLS counterfactuals depicted in these plots suggest that the minimum wage explains essentially all (or more than all) of the rise in 50/10 inequality in the female, male, and pooled-gender distributions during this period. The 2SLS counterfactuals place this contribution at a far more modest level. The counterfactual series for males, for example, is indistinguishable from the observed series, implying that the minimum wage made almost no contribution to the rise in male inequality in this period. We see a similarly pronounced discrepancy between OLS and 2SLS models in the lower panel of Figure 9, which plots observed and counterfactual wage changes in the pooled gender distribution for the full sample period of 1979 through 2012 (again holding the minimum wage at its 1988 value).[17]

Consistent with earlier literature, our estimates confirm that the falling minimum wage contributed to the growth of lower tail inequality growth during the 1980s. But while prior work, most notably Lee (1999), finds that the falling minimum fully accounts for this growth, this result appears strongly upward biased by violation of the identifying assumptions on which it rests. Purging this bias, we find that the minimum wage can explain at most half—and generally less than half—of the growth of lower tail inequality during the 1980s. Over the full three decades between 1979 and 2012, at least 60 percent of the growth of pooled 50/10 inequality, 50 percent of

[17] We have repeated these counterfactuals using coefficient estimates from years 1979 through 1991 (using the additional cross-state identification offered by the increases in the federal minimum wage over this period) rather than the full 1979–2012 sample period. The counterfactual estimates from this exercise are somewhat smaller but largely consistent with the full sample, both during the critical period of 1979 through 1989 and during other intervals.

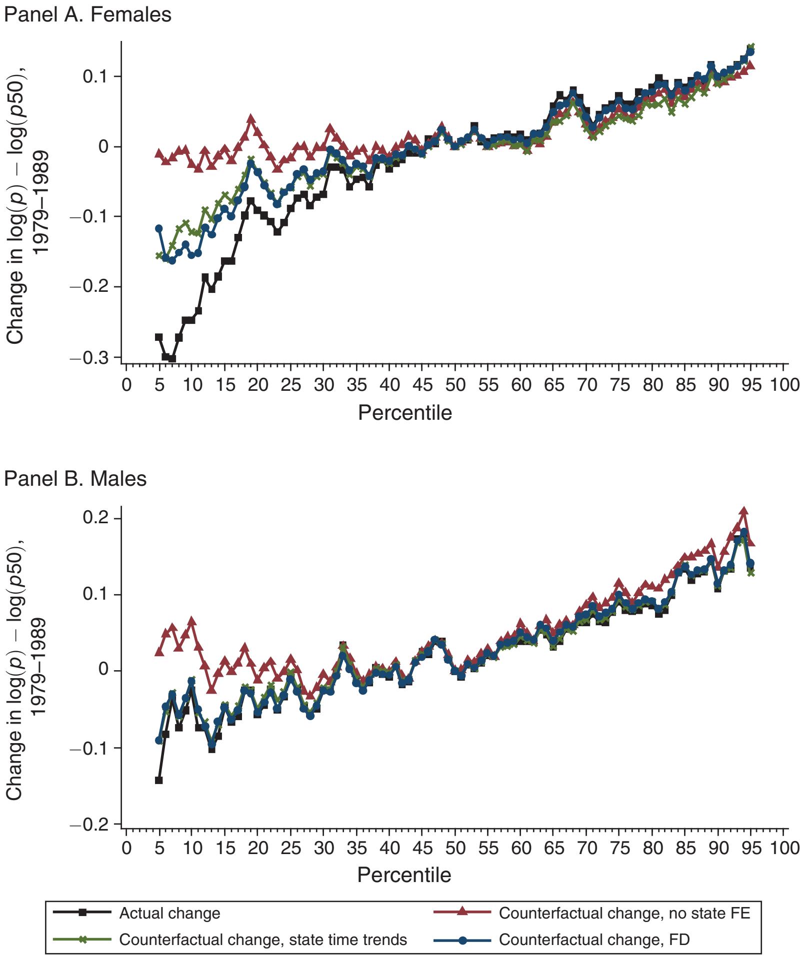


FIGURE 8. ACTUAL AND COUNTERFACTUAL CHANGE IN LOG(p) – LOG$(p50)$ DISTRIBUTION BY SEX

Notes: Plots represent the actual and counterfactual changes in the fifth through ninety-fifth percentiles of the male wage distribution. Counterfactual changes are calculated by adjusting the 1979 wage distributions by the value of states' effective minima in 1989 using coefficients from OLS regressions without state fixed effects (column 5 of Table 2) and 2SLS regressions with state fixed effects and time trends or first-differenced 2SLS regressions with state fixed effects (columns 3 and 4 of Table 2).

female 50/10 inequality, and 90 percent of male 50/10 inequality is due to changes in the underlying wage structure.

IV. The Limits of Inference: Distinguishing Spillovers from Measurement Error

Federal and state minimum wages were nominally nonbinding at the tenth percentile of the wage distribution throughout most of the sample (Figure 2); in fact, there is only one 3-year interval (1979 to 1983), when more than 10 percent of hours paid were at or below the minimum wage (Table 1)—and this was only the case for females. Yet our main estimates imply that the minimum wage modestly

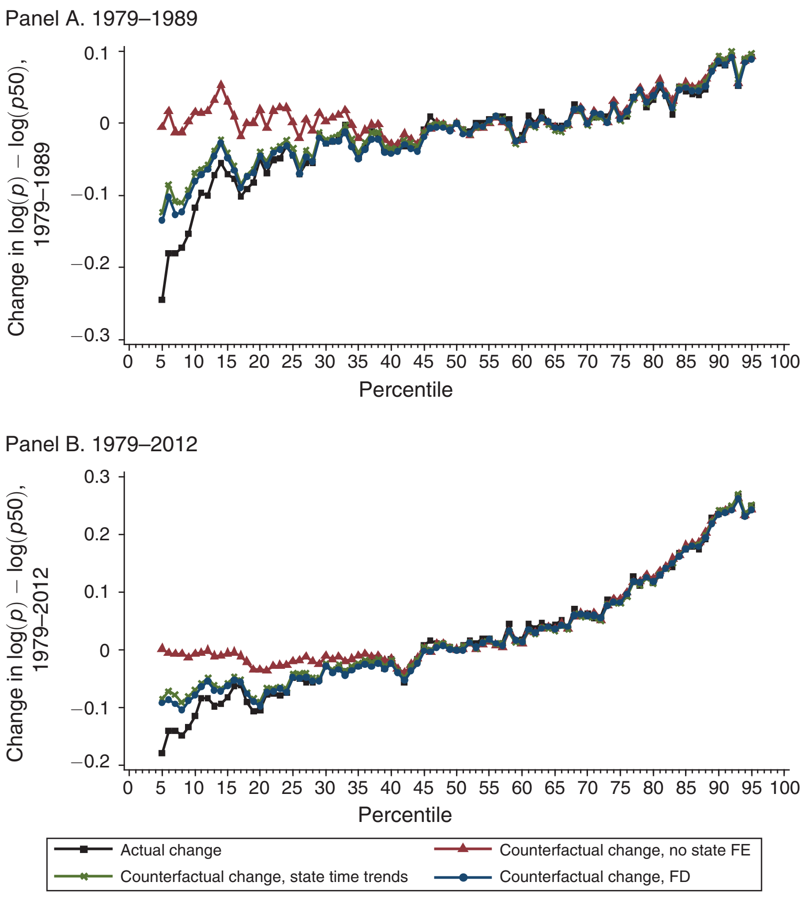


Figure 9. Actual and Counterfactual Change in $\log(p)$–$\log(p50)$ Male and Female Pooled Distribution

Notes: Plots represent the actual and counterfactual changes in the fifth through ninety-fifth percentiles of the male and female pooled wage distribution. Counterfactual changes in panel A are calculated by adjusting the 1979 wage distributions by the value of states' effective minima in 1989 using coefficients from OLS regressions (column 5 of Table 2) and 2SLS regressions (columns 3 and 4 of Table 2). Counterfactual changes in panel B are calculated by adjusting both the 1979 and 2012 wage distributions by the value of states' effective minima in 1989 using coefficients from OLS regressions (column 5 of Table 2) and 2SLS regressions (columns 3 and 4 of Table 2).

compressed both male and pooled-gender 50/10 wage inequality during the 1980s. This implies that the minimum wage had spillover effects on percentiles above where it binds.

While these spillovers might arise from several economic forces, such as tournament wage structures or positional income concerns, a mundane but nonetheless plausible alternative explanation is measurement error. To see why, consider a case where the minimum wage is set at the fifth percentile of the latent wage distribution and has no spillover effects. However, due to misreporting, the spike in the wage distribution at the true minimum wage is surrounded by a measurement error cloud that extends from the first through the ninth percentiles. If the legislated minimum wage were to rise to the ninth percentile and measurement error were to remain constant, the rise in the minimum wage would compress the *measured* wage distribution up to the thirteenth percentile, thus reducing the measured 50/10 wage gap. This apparent spillover would be a feature of the data, but it would not be a feature of the true wage distribution.[18]

In this final section of the paper, we quantify the possible bias wrought by these measurement spillovers. Specifically, we ask whether we can reject the null hypothesis that the minimum wage *only* affects the earnings of those earning at or below the minimum—in which case, the apparent spillovers would be consistent with measurement error.[19] Since this analysis relies in part on some strong assumption, it should be thought of as an illustrative exercise designed to give some idea of magnitudes rather than a dispositive test.

A. *General Setup*

We use a simple measurement error model to test the hypothesis.[20] Denote by p^* a percentile of the latent wage distribution (i.e., the percentile absent measurement error and without a minimum wage), and write the latent wage associated with it as $w^*(p^*)$. Assuming that there are only direct effects of the minimum wage (i.e., no true spillovers and no disemployment effects), then the true wage at percentile p^* will be given by

$$w(p^*) \equiv \max\left[w^m, w^*(p)\right], \tag{4}$$

where $w^*(p)$ is the true latent log wage percentile and w^m the log of the minimum wage.

Now, allow for the possibility of measurement error, so that for a worker at true wage percentile p^*, we observe:

$$w_i = w(p^*) + \varepsilon_i, \tag{5}$$

where ε_i is an error term with density function $g(\varepsilon)$, which we assume to be independent of the true wage. We will make use of the following result proved in section B of the Appendix:

Result 1: Under the null hypothesis of no actual spillovers, no disemployment and measurement error independent of the true wage, the elasticity of wages at an

[18] This argument holds in reverse for a decline in the minimum wage.

[19] Note that we are not testing whether an apparent spillover for a particular percentile, for a particular state/year, is attributable to measurement error—we are testing whether, on average, *all* of the observed spillovers could be attributable to measurement error.

[20] In the following discussion, it will be useful to distinguish between three distinct wage distributions: (i) the latent wage distribution, which is the wage distribution in the absence of a minimum wage and measurement error; (ii) the true wage distribution, which is the wage distribution in the absence of measurement error but allowing for minimum wage effects; and (iii) the observed wage distribution, which is the wage distribution allowing for measurement error and a minimum wage (i.e., what is measured from CPS data).

observed percentile with respect to the minimum wage is equal to the fraction of people at that observed percentile whose true wage is equal to the minimum.

The intuition for this result is straightforward: if the minimum wage rises by 10 percent, and 10 percent of workers at a given percentile are paid the minimum wage, and only these have their wage affected, the observed wage at that percentile will rise by 1 percent.

This result has a simple corollary (proved in section C of the Appendix) that we also use in the estimation below:

Result 2: Under the null hypothesis of no true spillovers, the elasticity of the *overall* mean log wage with respect to the minimum wage is equal to the fraction of the wage distribution that is truly paid the minimum wage—that is, the size of the true spike.

This result follows from the fact that all individuals who are truly paid the minimum wage must appear *somewhere* in the observed wage distribution. And of course, changes at any point in the distribution also change the mean. Thus, if the true spike at the minimum wage comprises 10 percent of the mass of the true wage distribution, a 10 percent rise in the minimum will increase the *true and observed* mean wage by 1 percent. Note that no distributional assumptions about measurement error are needed for either Result 1 or Result 2 other than the assumption that the measurement error distribution is independent of wage levels.

The practical value of Result 2 is that we can readily estimate the effect of changes in the minimum wage on the mean using the methods developed above. In practice, we estimate a version of equation (1), using as the dependent variable the average log real wage. On the right-hand side, we include the effective minimum wage and its square as endogenous regressors (and instrument for them using the same instruments as in the earlier analysis), state and year fixed effects, state time trends, and the log of the median (to control for shocks to the wage level of the state that are unrelated to the minimum wage, assuming that any spillovers do not extend through the median). We plot these estimates in the three panels of Figure 10, corresponding to females, males, and the pooled wage distribution. The dashed line in each panel represents the marginal effect of the minimum on the mean by year, taking the weighted average across all states for each year. Under the null hypothesis of no true spillovers, this estimate of the effect the minimum on the mean is an estimate of the size of the true spike. Under the alternative hypothesis that true spillovers are present, the marginal effect on the mean will exceed the size of the true spike. To distinguish these alternatives requires a second, independent estimate of the size of the true spike.

B. *Estimating Measurement Error*

We develop a second estimate of the magnitude of the true spike by directly estimating a model of measurement error and using this estimate to infer the size of the spike absent this error. We exploit the fact that under the assumption of full

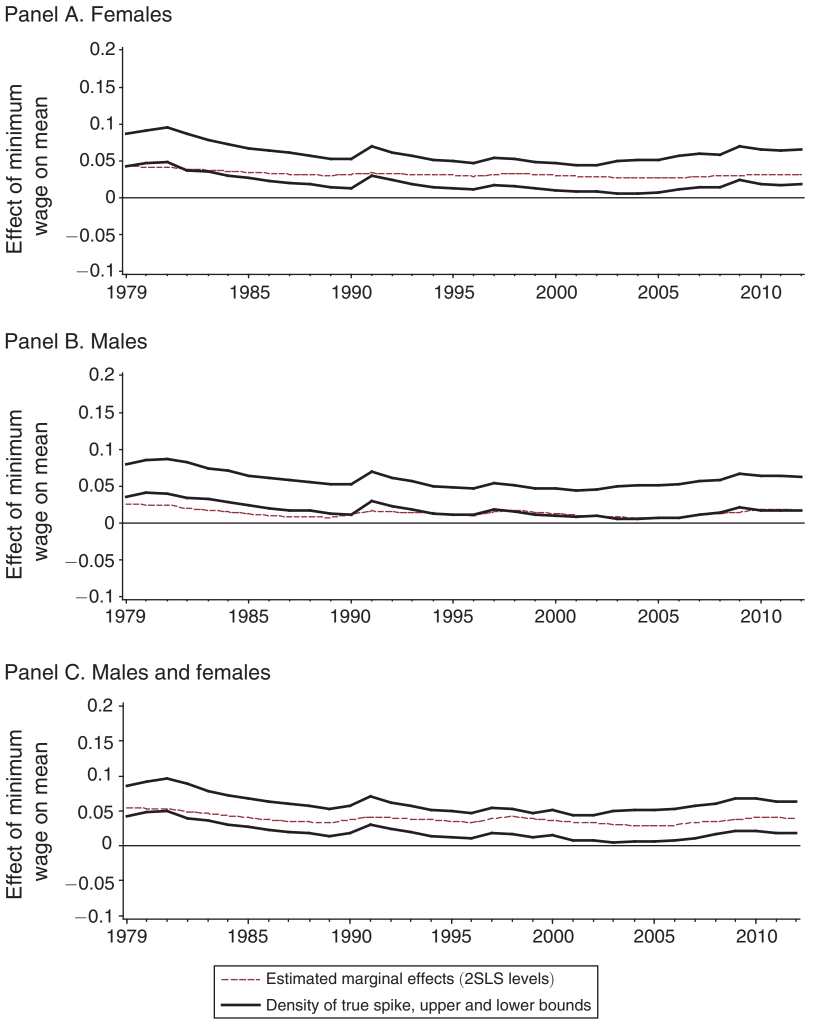


Figure 10. Comparison of Estimated Effects of the Minimum Wage on the Mean and Density at the True Spike

Notes: Mean effects represent the average marginal effects of the minimum wage (weighted across states), estimated from 2SLS regressions of log(mean) – log($p50$) on the effective minimum and its square, year and state fixed effects, state time trends, and the log median, where the effective minimum and its square are instrumented as in the earlier analysis. The bounds for the density of the true spike are estimated from a maximum likelihood procedure described in section D of the Appendix.

compliance with the minimum wage, all observations found below the minimum wage must be observations with measurement error.[21] Of course, wage observations

[21] There are surely some individuals who report sub-minimum wage wages and *actually* receive sub-minimum wages. The largest (but not the only) group is probably tipped workers, who in many states can legally receive a subminimum hourly wage as long as tips push their total hourly income above the minimum. For instance, in 2009, about 55 percent of those who reported their primary occupation as waiter or waitress reported an hourly wage less than the applicable minimum wage for their state, and about 17 percent of all observed subminimum wages were from waiters and waitresses. If we treat the wages of these individuals as measurement error, we will clearly

below the minimum can only provide information on individuals with *negative* measurement error, since minimum wage earners with positive measurement error must have an observed wage above the minimum. Thus, a key identifying assumption is that the measurement error is symmetric, that is $g(\varepsilon) = g(-\varepsilon)$.

In what follows, we use maximum likelihood to estimate the distribution of wages below the minimum and the fraction of workers at and above the minimum (for the sample of non-tipped workers as described in footnote 21). We assume that the "true" wage distribution only has a mass point at the minimum wage so that $w^*(p^*)$ has a continuous derivative. We also assume that the measurement error distribution only has a mass point at zero so that there is a nonzero probability of observing the "true" wage. (Without this assumption, we would be unable to rationalize the existence of a spike in the observed wage distribution at the minimum wage.) Denote the probability that the wage is correctly reported as γ. For those who report an error-ridden wage, we will use, in a slight departure from previous notation, $g(\varepsilon)$ to denote the distribution of the error.

With these assumptions, the size of the spike in the *observed* wage distribution at the minimum wage, which we denote by $\tilde{p}$, is equal to the true spike times the probability that the wage is correctly reported:

$$\tilde{p} = \gamma \hat{p}. \tag{6}$$

Hence, using an estimate of γ, we can estimate the magnitude of the true spike as $\hat{p} \approx \tilde{p}/\tilde{\gamma}$.[22]

C. *Finding: Spillovers Cannot Be Distinguished from Measurement Error*

We use the following two-step procedure to estimate γ. Under the assumption that the latent log wage distribution is normal with mean μ and variance σ_w^2 and that the measurement error distribution is normal with mean zero and variance σ_ε^2, we use observations from the top part of the wage distribution—which we assume are unaffected by changes to the minimum wage—to estimate the median and variance of the observed latent wage distribution, allowing for variation across state and time.[23] Equipped with these estimates, we use the observed fraction of workers who are paid below the minimum for each state and year to estimate $(\sigma_\varepsilon^2, \gamma)$ by maximum likelihood. We assume that $(\sigma_\varepsilon^2, \gamma)$ vary over time but not across states. Exact details of our procedure can be found in section D of the Appendix. As previously noted, we perform this analysis on a sample that excludes individuals from lower-paying occupations that tend to earn tips or commission.

over-state the extent of misreporting. We circumvent this problem by conducting the measurement/spillover analysis on a sample that *excludes* employees in low-paying occupations that commonly receive tips or commission. These are: food service jobs, barbers and hairdressers, retail salespersons, and telemarketers.

[22] The assumption on the absence of mass points in the true wage distribution and the error distribution mean that the group of workers who are not paid the minimum but, by chance, have an error that makes them appear to be paid the minimum is of measure zero and so can be ignored.

[23] This procedure does not account for the type of measurement error induced by heaping of observations around whole numbers (e.g., \$5.50), so the estimates that follow should be treated as suggestive.

Estimates of γ for males, females, and the pooled sample (not shown) generally find that the probability of correct reporting is around 80 percent, and mostly varies from between 70 to 90 percent over time (though is estimated to be around 65 to 70 percent in the early 1980s for females and the pooled distribution). We combine this estimate with the observed spike to get an estimate of the "true" spike in each period, though this will be an estimate of the size of the true spike only for the estimation sample of workers in non-tipped occupations.

This leaves us in need of an estimate of the "true" spike for the tipped occupations. Given the complexity of the state laws surrounding the minimum wage for tipped employees, we do not attempt to model these subminimum wages. Rather we simply note that the spike for tipped employees must be between zero and one, and we use this observation to bound the "true" spike for the entire workforce. Because the fraction of workers in tipped occupations is small, these bounds are relatively tight.

Figure 10 compares these bounds with the earlier estimates of the "true" spike based on the elasticity of the mean with respect to the minimum in each year. Under the null hypothesis that the minimum wage has no true spillovers, the effect on the mean should equal the size of the "true" spike. And indeed, the estimated mean effect lies within the bounds of the estimated "true" spike in almost all years. We are accordingly unable to reject the hypothesis that the apparent effect of the minimum wage on percentiles above the minimum is a measurement error spillover rather than a true spillover.

This analysis rests on some strong assumptions and so should not be regarded as definitive. But if we tentatively accept this null, it has the important implication that changes in the minimum wage may only affect those who are paid the minimum and the apparent effects further up the wage distribution are the consequences of measurement error. A conclusive answer would require better wage data, ideally administrative payroll data.[24]

V. Conclusion

This paper offers a reassessment of the impact of the minimum wage on the wage distribution by using a longer panel than was available to previous studies, incorporating many additional years of data and including significantly more variation in state minimum wages, and using an econometric approach that purges confounding correlations between state wage levels and wage variances that we find bias earlier estimates. Under our preferred model specification and estimation sample, we estimate that between 1979 and 1989, the decline in the real value of the minimum wage is responsible for 30 to 55 percent of the growth of lower tail inequality in the female, male, and pooled wage distributions (as measured by the differential between the log of the fiftieth and tenth percentiles). Similarly, calculations indicate that during the full sample period of 1979–2012, the declining minimum wage made

[24] In Dube, Guiliano, and Leonard's (2015) study of the impact of wage increases on employment and quit behavior at a large retail firm, the authors note that this firm implemented sizable wage spillovers as a matter of corporate policy—with minimum wage increases automatically leading to raises among workers earning as much as 15 percent above the new minimum.

a meaningful contribution to female inequality, a modest contribution to pooled gender inequality, and a negligible contribution to male lower tail inequality. In net, these estimates indicate a substantially smaller role for the US minimum in the rise of inequality than suggested by earlier work, which had attributed 85 percent to 110 percent of this rise to the falling minimum.

Despite these modest total effects, we estimate that the effect of the minimum wage extends further up the wage distribution than would be predicted if the minimum wage had a purely mechanical effect on wages (i.e., raising the wage of all who earned below it). One interpretation of these significant spillovers is that they represent a true wage effect for workers initially earning above the minimum. An alternative explanation is that wages for low-wage workers are mismeasured or misreported. If a significant share of minimum wage earners report wages in excess of the minimum wage, and this measurement error persists in response to changes in the minimum, then we would observe changes in percentiles above where the minimum wage directly binds in response to changes in the minimum wage. Our investigation of this hypothesis in Section IV is unable to reject the null hypothesis that all of the apparent effect of the minimum wage on percentiles above the minimum is the consequence of measurement error. Accepting this null, the implied effect of the minimum wage on the *actual* wage distribution is even smaller than the effect of the minimum wage on the *measured* wage distribution.

In net, our analysis suggests that there was a significant expansion in latent lower tail inequality over the 1980s, mirroring the expansion of inequality in the upper tail. While the minimum wage was certainly a contributing factor to widening lower tail inequality—particularly for females—it was not the primary one.

Appendix

A. *Data Appendix*

As described in Section I, our primary data comes from individual responses from the Current Population Survey Merged Outgoing Rotation Group (CPS MORG) for each year. For each year, we pool the monthly observations. We use CPS variables (e.g., weekly and hourly wages) as cleaned by the Unicon Research Corporation. Specifically, the hourly wage variable is ERNHR, the weekly wage variable is WKUSERN, ERNWKC, or ERNWK (depending on the year), and the weekly hours variable is HOURS. The respondent weight variable that we use is ERNWGT. As mentioned in the text, in our calculations we weight by a respondent's earnings weight (ERNWGT) multiplied by hours worked (HOURS), although our findings are roughly unchanged if we use ERNWGT instead.

The primary outcome we construct from CPS data is a respondent's wage. For those who report being paid by the hour we take their hourly wage to be their reported hourly wage; otherwise we calculate the hourly wage as weekly earnings divided by hours worked in the prior week. We multiply top-coded values by 1.5. When computing percentiles within a state, we Winsorize the top two percentiles of the wage distribution in each state, year, and sex grouping (male, female, or pooled) by assigning

Table A1—First-Stage Estimates for Specifications That Include Year Fixed Effects and State Time Trends

	A. Females		B. Males		C. Males and females	
	LHS: log(min) − log(p50) (1)	LHS: square of log(min) − log(p50) (2)	LHS: log(min) − log(p50) (3)	LHS: square of log(min) − log(p50) (4)	LHS: log(min) − log(p50) (5)	LHS: square of log(min) − log(p50) (6)
log(min)	0.30	0.61	1.79***	−2.31***	1.15***	−0.92
	(0.41)	(0.58)	(0.44)	(0.80)	(0.40)	(0.61)
Square of log(min)	−0.34*	1.32***	−0.82***	2.59***	−0.61***	2.01***
	(0.18)	(0.27)	(0.24)	(0.45)	(0.20)	(0.32)
log(min) × avg. of state median	0.79***	−2.76***	0.49*	−2.56***	0.61***	−2.69***
	(0.14)	(0.15)	(0.26)	(0.46)	(0.21)	(0.29)
F-statistics	251***	362***	370***	394***	587***	684***
p-value	0.00	0.00	0.00	0.00	0.00	0.00

Notes: $N = 1{,}700$. Sample period is 1979–2012. For columns 1, 3, and 5, the dependent variable is the "effective minimum," that is, log(min) − log(p50). For columns 2, 4, and 6, the dependent variable is the square of the effective minimum. The RHS variables included are the three instruments: the log of the minimum wage, the square of the log min, and the interaction between the log of the min multiplied by the average median for the state over the sample. Also included in the regression are year fixed effects, and state-specific trends. The F-statistic for testing whether the three instruments are jointly significant, and associated p-value, are presented at the bottom. Standard errors clustered at the state level are in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table A2—OLS Relationship between Mean and Trends in log(p60) − log(p40) and log(p50)

	A. Females		B. Males		C. Males and females	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent variable: mean log(p60) − log(p40), 1979–2012						
Mean log(p50), 1979–2012	0.12***		0.06		0.11***	
	(0.03)		(0.05)		(0.04)	
Panel B. Dependent variable: trend log(p60) − log(p40), 1979–2012						
Trend log(p50), 1979–2012		0.16**		0.03		0.14**
		(0.07)		(0.08)		(0.06)

Notes: $N = 50$ (one observation per state). Observations are weighted by the average hours worked per state. Robust standard errors are in parentheses. The dependent variable in the top panel is the mean log(p60) − log(p40) for the state, over the 1979–2012 period. The dependent variable in the bottom panel is the linear trend in the log(p60) − log(p40) for the state, over the 1979–2012 period. Regressions correspond to plots in Figures 6A and 6B.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

the ninety-seventh percentile value to the ninety-eighth and ninety-ninth percentiles. Our sample includes individuals age 18–64, and we exclude self-employed individuals as well as respondents with wage data imputed by the BLS.

B. *Proof of Result* 1

The density of wages among workers whose true percentile p^* is given by $g\left(w - w(p^*)\right)$. The density of observed wages is simply the average of $g(\cdot)$ across true percentiles:

$$f(w) = \int_0^1 g\left(w - w(p^*)\right) dp^*. \tag{B1}$$

And the cumulative density function for observed wages is given by

$$F(w) = \int_{-\infty}^{w} \int_0^1 g\left(w - w(p^*)\right) dp^*\, dx. \tag{B2}$$

This can be inverted to give an implicit equation for the wage at observed percentile p, $w(p)$:

$$p = \int_{-\infty}^{w(p)} \int_0^1 g\left(w - w(p^*)\right) dp^*\, dx. \tag{B3}$$

By differentiating this expression with respect to the minimum wage, we obtain the following key result:

$$\left[\int_0^1 g\left[w(p) - w(p^*)\right] dp^*\right] \frac{\partial w(p)}{\partial w^m} + \int_{-\infty}^{w(p)} \int_0^1 \frac{\partial g\left[x - w(p^*)\right]}{\partial w^m} dp^*\, dx = 0. \tag{B4}$$

Now we have that

$$\frac{\partial g\left[x - w(p^*)\right]}{\partial w^m} = -g\left[x - w(p^*)\right] \frac{\partial w(p^*)}{\partial w^m}. \tag{B5}$$

Which, from (B1), is

$$\frac{\partial g\left[x - w(p^*)\right]}{\partial w^m} = \begin{array}{ll} -g\left[x - w^m\right] & if \;\; p^* \leq \hat{p}(w^m) \\ 0 & if \;\; p^* > \hat{p}(w^m) \end{array}. \tag{B6}$$

Substituting (B1) and (B6) into (B4) and re-arranging, we have that

$$\frac{\partial w(p)}{\partial w^m} = \frac{\hat{p} g\left[w(p) - w^m\right]}{f\left[w(p)\right]}. \tag{B7}$$

The numerator is the fraction of workers who are really paid the minimum wage but are observed with wage $w(p)$ because they have measurement error equal to $\left[w(p) - w^m\right]$. Hence, the numerator divided by the denominator is the fraction of workers observed at wage $w(p)$ who are really paid the minimum wage.

C. *Proof of Result* 2

One implication of (B7) is the following. Suppose we are interested in the effect of minimum wages on the mean log wage, $\bar{w}(p)$. We have that

$$\text{(C1)} \qquad \frac{\partial \bar{w}}{\partial w^m} = \int_0^1 \frac{\partial w(p)}{\partial w^m} dp = \int_0^1 \frac{\hat{p} g[w(p) - w^m]}{f[w(p)]} dp.$$

Change the variable of integration to $w(p)$. We will have

$$\text{(C2)} \qquad dw = w'(p)\, dp = \frac{1}{f[w(p)]} dp.$$

Hence, (C1) becomes

$$\text{(C3)} \qquad \frac{\partial \bar{w}}{\partial w^m} = \hat{p} \int_{-\infty}^{\infty} g[w - w^m] dw = \hat{p}.$$

That is, the elasticity of average log wages with respect to the log minimum is just the size of the true spike.

D. *Estimation Procedure for the Measurement Error Model*

We first derive the proportion of workers reporting subminimum wages, which we denote by Z. Assuming full compliance with the minimum wage statute, all of these subminimum wages will represent negative measurement error. We therefore have

$$\text{(D1)} \qquad Z = (1 - \gamma) \times \left[0.5\hat{p} + \int_{\hat{p}}^1 G\big(w^m - w^*(p^*)\big)\, dp^*\right].$$

The symmetry assumption implies that half of those at the true spike who report wages with error will report wages below the minimum, and this is reflected as the first term in the bracketed expression $(0.5\hat{p})$. In addition, for workers paid above the minimum, some subset will report with sufficiently negative error that their reported wage will fall below the minimum, thus also contributing to the mass below the statutory minimum. This contributor to Z is captured by the second term in the bracketed expression.

Our assumption is that the true latent log wage is normally distributed according to

$$\text{(D2)} \qquad w^* \sim N(\mu, \sigma_w^2).$$

To keep notation to a minimum we suppress variation across states and time, though this is incorporated into the estimation. The true wage is given by

$$\text{(D3)} \qquad w = \max(w^m, w^*).$$

And the observed wage is given by

$$v = w + D\varepsilon, \tag{D4}$$

where D is a binary variable taking the value 0 if the true wage is observed and 1 if it is not. We assume that

$$\Pr(D = 1) = 1 - \gamma. \tag{D5}$$

We assume that ε is normally distributed according to

$$\varepsilon \sim N\left(0, \frac{1 - \rho^2}{\rho^2} \sigma_w^2\right). \tag{D6}$$

We choose to parameterize the variance of the error process as proportional to the variance of the true latent wage distribution as this will be convenient later. We later show that ρ is the correlation coefficient between the true latent wage and the observed latent wage when misreported—a lower value of ρ implies more measurement error so leads to a lower correlation between the true and observed wage. We assume that (w^*, D, ε) are all mutually independent.

Our estimation procedure uses maximum likelihood to estimate the parameters of the measurement error model. There are three types of entries in the likelihood function:

- those with an observed wage equal to the minimum wage
- those with an observed wage above the minimum wage
- those with an observed wage below the minimum wage

Let us consider the contribution to the likelihood function for these three groups in turn.

Those Observed to Be Paid the Minimum Wage.—With the assumptions made above, the "true" size of the spike is given by

$$\hat{p} = \Phi\left(\frac{w^m - \mu}{\sigma_w}\right). \tag{D7}$$

And the size of the observed "spike" is given by

$$\tilde{p} = \gamma \Phi\left(\frac{w^m - \mu}{\sigma_w}\right). \tag{D8}$$

This is the contribution to the likelihood function for those paid the minimum wage.

Those Observed to Be Paid Below the Minimum Wage.—Now let us consider the contribution to the likelihood function for those who report being paid below the minimum wage. We need to work out the density function of actual observed wages w, where $w < w^m$. None of those who report their correct wages (i.e., have $D = 0$) will report a subminimum wage, so we need only consider those who misreport their wage (i.e., those with $D = 1$). Some of these will have a true wage equal to the minimum and some will have a true wage above the minimum. Those who are truly paid the minimum will have measurement error equal to $(w - w^m)$, so, using (D6) and (D7), the contribution to the likelihood function will be

$$\text{(D9)} \qquad (1-\gamma)\frac{\rho}{\sigma_w\sqrt{1-\rho^2}}\,\phi\!\left(\frac{\rho(w-w^m)}{\sigma_w\sqrt{1-\rho^2}}\right)\Phi\!\left(\frac{w^m-\mu}{\sigma_w}\right).$$

Now, consider those whose true wage is above the minimum but have a measurement error that pushes their observed wage below the minimum. For this group, their observed wage is below the minimum and their latent wage is above the minimum. The fraction of those who misreport who are in this category is, with some abuse of the concept of probability,

$$\text{(D10)} \qquad \Pr(v = w, w^* > w^m).$$

Define

$$\text{(D11)} \qquad v^* = w^* + \varepsilon,$$

which is what the observed wage would be if there was no minimum wage and they misreport i.e., $D = 1$.

From (D2) and (D4),

$$\text{(D12)} \qquad \begin{aligned} \begin{pmatrix} v^* \\ w^* \end{pmatrix} &\sim N\left[\begin{pmatrix} \mu \\ \mu \end{pmatrix}, \begin{pmatrix} \sigma_w^2 + \sigma_\varepsilon^2 & \sigma_w^2 \\ \sigma_w^2 & \sigma_w^2 \end{pmatrix}\right] \\ \begin{pmatrix} v^* \\ w^* \end{pmatrix} &\sim N\left[\begin{pmatrix} \mu \\ \mu \end{pmatrix}, \sigma_w^2\begin{pmatrix} 1/\rho^2 & 1 \\ 1 & 1 \end{pmatrix}\right] \end{aligned}.$$

This implies the following:

$$\text{(D13)} \qquad \begin{pmatrix} v^* \\ w^* - \rho^2 v^* \end{pmatrix} \sim N\left[\begin{pmatrix} \mu \\ \mu(1-\rho^2) \end{pmatrix}, \sigma_w^2\begin{pmatrix} 1/\rho^2 & 1 \\ 1 & 1-\rho^2 \end{pmatrix}\right],$$

which is an orthogonalization that will be convenient.

Now for those paid above the minimum but whose wage is misreported, the true wage is w^* and the observed wage is v^*. So,

$$\text{(D14)}\quad \begin{aligned}\Pr(v = w,\, w^* > w^m) &= \Pr(v^* = w, w^* > w^m)\\ &= \Pr(v^* = w, w^* - \rho^2 v^* > w^m - \rho^2 w)\\ &= \Pr(v^* = w)\Pr(w^* - \rho^2 v^* > w^m - \rho^2 w)\\ &= \frac{\rho}{\sigma_w}\phi\left(\frac{\rho(w-\mu)}{\sigma_w}\right)\left[1 - \Phi\left(\frac{\left[(w^m - \mu) - \rho^2(w-\mu)\right]}{\sigma_w\sqrt{1-\rho^2}}\right)\right],\end{aligned}$$

where the third line uses the independence of (D13).

Putting together (D9) and (D14), the fraction of the population observed to be paid at a wage w below the minimum is given by

$$\text{(D15)}\quad L = (1-\gamma)\cdot\left[\left[\frac{\rho}{\sigma_w\sqrt{1-\rho^2}}\phi\left(\frac{\rho(w-w^m)}{\sigma_w\sqrt{1-\rho^2}}\right)\right]\Phi\left(\frac{w^m-\mu}{\sigma_w}\right)\right.$$
$$\left. + \frac{\rho}{\sigma_w}\phi\left(\frac{\rho(w-\mu)}{\sigma_w}\right)\left[1 - \Phi\left(\frac{\left[(w^m-\mu) - \rho^2(w-\mu)\right]}{\sigma_w\sqrt{1-\rho^2}}\right)\right]\right].$$

Those Observed to Be Paid above the Minimum Wage.—Now let us consider the fraction observed above the minimum wage. These workers might be one of three types:

- Those really paid the minimum wage who misreport a wage above the minimum.
- Those really paid above the minimum wage who do not misreport.
- Those really paid above the minimum wage, who do misreport, but do not report a subminimum wage.

For those who are truly paid the minimum wage and have a misreported wage, a half will be above, so the fraction of those who report a wage above the minimum is:

$$\text{(D16)}\qquad \frac{1}{2}(1-\gamma)\Phi\left(\frac{w^m-\mu}{\sigma_w}\right).$$

Those who do not misreport and truly have a wage above the minimum will be

$$\text{(D17)}\qquad \gamma\left(1 - \Phi\left(\frac{w^m-\mu}{\sigma_w}\right)\right).$$

Now, consider those whose true wage is above the minimum but who misreport. For this group we know their observed latent wage is above the minimum and that their true latent wage is above the minimum. The fraction who are in this category is

$$\Pr(w^* > w^m, v^* > w^m). \tag{D18}$$

Now,

$$\begin{aligned} \Pr(w^* > w^m, v^* > w^m) &= 1 - \Pr(w^* < w^m) - \Pr(v^* < w^m) + \Pr(w^* < w^m, v^* < w^m) \\ &= 1 - \Phi\left(\frac{w^m - \mu}{\sigma_w}\right) - \Phi\left(\frac{\rho(w^m - \mu)}{\sigma_w}\right) \\ &\quad + \Phi\left(\frac{w^m - \mu}{\sigma_w}, \frac{\rho(w^m - \mu)}{\sigma_w}, \rho\right), \end{aligned} \tag{D19}$$

where the final term is the cumulative density function of the bivariate normal distribution. Putting together (D16), (D17), and (D19), the fraction of the population observed to be paid above the minimum is given by

$$\begin{aligned} (1 - \gamma) \cdot \Bigg[&\frac{1}{2}\Phi\left(\frac{w^m - \mu}{\sigma_w}\right) + 1 - \Phi\left(\frac{w^m - \mu}{\sigma_w}\right) - \Phi\left(\frac{\rho(w^m - \mu)}{\sigma_w}\right) \\ &+ \Phi\left(\frac{w^m - \mu}{\sigma_w}, \frac{\rho(w^m - \mu)}{\sigma_w}, \rho\right)\Bigg] + \gamma\left(1 - \Phi\left(\frac{w^m - \mu}{\sigma_w}\right)\right). \end{aligned} \tag{D20}$$

This is the contribution to the likelihood function for those paid above the minimum.

There are three parameters in this model (σ_w, γ, ρ). These parameters may vary with state or time. In the paper we have already documented how the variance in observed wages varies across state and time, so it is important to allow for this variation. Nevertheless, for ease of computation our estimates assume that (γ, ρ) only vary across time and are constant across states.

We estimate the parameters in two steps. We first use the information on the shape of the wage distribution above the median to obtain an estimate of the median and variance of the latent observed wage distribution for each state/year.[25] This

[25] To estimate this, we assume that the latent wage distribution for each state/year is log normal and can be summarized by its median and variance, so that $w^*_{st}(p) = \mu_{st} + \sigma_{st} F^{-1}(p)$, where μ_{st} is the log median and σ_{st} is the variance. We then assume that the minimum wage has no effect on the shape of the wage distribution above the median, so that upper-tail percentiles are estimates of the latent distribution. To estimate μ_{st} and σ_{st}, we pool the fiftieth through seventy-fifth log wage percentiles, regress the log value of the percentile on the inverse CDF of the

assumes that the latent distribution above the median is unaffected by the minimum wage. It also assumes that latent observed wage distribution is normal, which is not consistent with our measurement error model (recall our model assumes that the latent observed wage distribution is a mixture of two distributions, i.e., those who report their wage correctly and those who do not). This does not affect the estimate of the median but does affect the interpretation of the variance. Here we show how to map between this estimate of the variance and the parameters of our measurement error model.

Our measurement error model implies that the log wage at percentile p, $w(p)$ satisfies the following equation:

$$p = \gamma \Phi\left(\frac{w(p) - \mu}{\sigma_w}\right) + (1 - \gamma)\Phi\left(\frac{\rho(w(p) - \mu)}{\sigma_w}\right). \tag{D21}$$

Differentiating this, we obtain the following equation for $w'(p)$:

$$1 = \left[\gamma\left(\frac{1}{\sigma_w}\right)\phi\left(\frac{w(p) - \mu}{\sigma_w}\right) + (1 - \gamma)\left(\frac{\rho}{\sigma_w}\right)\phi\left(\frac{\rho(w(p) - \mu)}{\sigma_w}\right)\right] w'(p). \tag{D22}$$

Our estimated model, which assumes a single normal distribution instead uses the equation

$$1 = \left(\frac{1}{\sigma}\right)\phi\left(\frac{w(p) - \mu}{\sigma}\right) w'(p). \tag{D23}$$

And our estimation procedure provides an estimate of σ. Equating the two terms we have the following expression for the relationship between σ_w and σ:

$$\sigma_w = \sigma \frac{\left[\gamma\phi\left(\frac{w(p) - \mu}{\sigma_w}\right) + \rho(1 - \gamma)\phi\left(\frac{\rho(w(p) - \mu)}{\sigma_w}\right)\right]}{\phi\left(\frac{w(p) - \mu}{\sigma}\right)}. \tag{D24}$$

If the values of the density functions are similar, then one can approximate this relationship by

$$\sigma_w = \sigma[\gamma + \rho(1 - \gamma)]. \tag{D25}$$

standard normal distribution, and allowing the intercept (μ_{st}) and coefficient (σ_{st}) to vary by state and year (and including state-specific time trends in both the intercept and coefficient). Since we assume the wage distribution is unaffected by the minimum wage between the fiftieth and seventy-fifth percentiles, the distribution between the fiftieth and seventy-fifth percentiles, combined with our parametric assumptions, allows us to infer the shape of the wage distribution for lower percentiles. We have experimented with the percentiles used to estimate the latent wage distribution and the results are not very sensitive to the choices made.

This is an approximation, but simulation of the model for the parameters we estimate suggest it is a good approximation. This implies that we can write all elements of the likelihood function as functions of

$$z_{st} = \left(\frac{w_{st}^m - \mu_{st}}{\sigma_{st}}\right). \tag{D26}$$

That is, z_{st} is the standardized deviation of the minimum from the median using the estimate of the observed variance obtained as described above from step 1 of the estimation procedure.

In the second step, we estimate the parameters (ρ, γ) using maximum-likelihood, with the elements of the likelihood function described above.

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The Impact of a City-Level Minimum Wage Policy on Supermarket Food Prices by Food Quality Metrics: A Two-Year Follow Up Study

James Buszkiewicz [1], Cathy House [2], Anju Aggarwal [1], Mark Long [3], Adam Drewnowski [1] and Jennifer J. Otten [4,*]

1 Epidemiology, Center for Public Health Nutrition, University of Washington, Seattle, WA 98195, USA; buszkiew@uw.edu (J.B.); anjuagg@uw.edu (A.A.); adamdrew@uw.edu (A.D.)
2 Nutritional Sciences Program, University of Washington, Seattle, WA 98195, USA; catherineehouse@gmail.com
3 Daniel J. Evans School of Public Policy and Governance, University of Washington, Seattle, WA 98195, USA; marklong@uw.edu
4 Environmental and Occupational Health Sciences, Center for Public Health Nutrition, University of Washington, Seattle, WA 98195, USA
* Correspondence: jotten@uw.edu

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Abstract: *Objective*: To examine the effects of increasing minimum wage on supermarket food prices in Seattle over 2 years of policy implementation, overall and differentially across food quality metrics. *Methods:* Prices for the UW Center for Public Health Nutrition (CPHN) market basket of 106 foods were obtained for 6 large supermarket chain stores in Seattle ("intervention") and for the same chain stores in King County ("control") at four time points: 1-month pre- (March 2015), 1-month post- (May 2015), 1-year post- (May 2016), and 2-years post-policy implementation (May 2017). Prices for all food items were standardized and converted to price per 100 kcal. Food quality metrics were used to explore potential differential price increases by (a) food groups, as defined by US Department of Agriculture; (b) NOVA food processing categories, and (c) nutrient density quartiles, based on the Nutrient Rich Foods Index 9.3. Separate difference-in-differences linear regression models with robust standard errors, examined price differences per 100 kcal overall, clustered by store chain, and stratified by each food quality metric. *Results:* There were no overall market basket price changes attributable to Seattle's minimum wage policy. Moreover, no minimum wage effect was detected by USDA food group, food processing, or nutrient density categories. *Conclusions:* Local area supermarket food prices were not impacted by Seattle's minimum wage policy 2 years into policy implementation and after the first increase to $15/h overall or by sub-classification. Low-income workers may be able to afford higher quality diets if wages increase yet supermarket prices stay the same.

Keywords: minimum wage; market basket; food cost; supermarkets; food price

1. Introduction

The federal minimum wage rate in the United States has not kept pace with inflation since 1968 contributing to growing wage inequities and a decline in purchasing power among low- and minimum-wage earning workers and their families [1,2]. Recently, in an effort to improve the economic environment of workers and increase the economic security and well-being of workers and their families, an increasing number of municipalities and counties have adopted wage rates above that of their state [3]. In 2003, Santa Fe, New Mexico and San Francisco, California became the first cities to adopt such local minimum wage ordinances [4] and, in 2014, Seattle became the first city to adopt a $15/h minimum wage [5]. On January 1st, 2017, many low wage workers in the City of Seattle saw their first minimum wage increase to $15/h [5].

While proponents of higher minimum wages argue that such increases are necessary to improve purchasing power and combat stagnating wages, there is concern that increased labor costs, which comprise, on average, one-third of total costs, will lead to a rise in the cost of goods, potentially offsetting these benefits for low-wage workers [6–8]. Increases in the cost of food are of particular concern given that one-third of all low-wage workers are employed in the food system and because low- and minimum wage workers spend a larger share of their income on food [9].

Limited evidence is available on the effect of minimum wage policies on food prices, particularly grocery store prices, and even fewer studies perform analyses to assess if there might be differential price increases on more nutritious food items [7,10–14]. Evaluations of minimum wage effects on food prices in the restaurant and fast food industries demonstrate pass-through effects ranging from 0.56% to 4% based on the timing and magnitude of the minimum wage increase (10% to 33%) [7,10–12]. However, our prior research on the early effect of a local minimum wage ordinance has found little effect on grocery store food prices at 1-month (16% increase, $9.47 to $11) or 1-year (37% increase, $11 to $13) post-policy implementation either overall, by food group, or by level of food processing [13,14].

Understanding the potentially differential price effects of minimum wage increases is important given that higher cost of healthy foods has been proposed as one of the underlying barriers to healthy eating, particularly among lower socioeconomic groups [15–19]. If increased labor costs due to minimum wage do not lead to an increase in food prices, then minimum wage policies may help low-wage workers achieve higher quality diets through greater food purchasing power. By contrast, if higher labor costs due to minimum wage increases cause a rise in food prices overall or differentially by nutritional content (e.g., more nutritious or nutrient-dense foods rise in cost faster than less nutritious foods), then low-wage workers may experience a decline in purchasing power for healthy foods, ultimately leading to poorer health. Low- or minimum-wage earning shoppers are significantly more price sensitive than their higher income counterparts [14,19,20]. Extant research has shown that policies or interventions that tax, such as recent city-level soda taxes [21], or target discounts on certain healthy foods, such as the United States Department of Agriculture's Healthy Incentives Pilot [22], can influence shopping behavior and consumption.

In this analysis, we examine differential price effects across three food quality metrics: food group, level of food processing, and nutrient density. These metrics were chosen because they have been associated with health outcomes and because they exhibit heterogeneity by food price when comparing across metrics [17,19,21,23–31]. The Centers for Disease Control and Prevention efforts to improve diet have focused on increasing the consumption of fruits and vegetables [25]. However, fruits and vegetables have been shown to be more expensive than other food groups. Thus, researchers and policymakers have explored pricing strategies, either through discounts or taxes, to encourage (e.g., fruit) or discourage (e.g. sugar-sweetened beverages) the consumption of certain food groups [21,26]. Consumption of ultra-processed food items have been linked to increased risk of certain chronic diseases, such as obesity, as well as increased consumption of added sugars [27–30]. Moreover, the convenience and pricing of processed and ultra-processed foods at supermarkets has been shown to influence their consumption [31]. Several studies have provided evidence that prices vary across the nutrient density profiles of food items with foods low in key micronutrients and high in energy (e.g., sugar sweetened beverages) being among the least expensive choices available to consumers [19]. Consumption of these low-nutrient foods has been linked to higher risk of obesity and there is concern that more price-sensitive populations, such as low-wage earners and their families, may consume such food items more frequently and thus may be at greater risk of developing diet-related chronic disease [17,23,24].

The present study has several strengths and builds on our prior work in several key ways [13,14]. Previously, we provided an illustration of methods for the application of a market basket data collection tool to examine changes in food prices for localities interested in tracking and comparing the price effects of similar policy changes. As with this paper, our prior work evaluated potential price effects overall as well as by food group and level of food processing, however, our prior work evaluated impacts at fewer time points and prior to fuller implementation of the Seattle Minimum Wage Ordinance. In the current analysis, we build upon these insights by adding a new food quality metric to access the potential for differential food price increases by nutrient density using the Nutrient Rich Foods Index 9.3 [32]. In addition, rather than standardizing food prices by weight or volume, as we have in our previous publication, we evaluate food price per 100 kcal. This standardized price measure allows us to evaluate changes across food group, food processing, and nutrient density strata in a way that is more reflective of human behavior and choices at the point of purchase. That is, when low-wage-earning shoppers make food purchasing decisions they are more likely to have incentive to obtain the greatest number of calories for the lowest price rather than the greatest weight of food. Finally, this analysis evaluates the effect of both an additional year of exposure to the Seattle Minimum Wage Ordinance as well as an additional increase in the policy phase-in wage rate, from $12.50–$13 per hour to $13.50–$15 per hour. This is of key importance as this paper is the first evaluation of the effect of a minimum wage increase to $15 per hour on local area supermarket food prices.

2. Methods

2.1. Study Design

In June 2014, the Seattle City Council adopted legislation which phased the City of Seattle's minimum wage rate to $15/h variably between 2015 and 2019–2021 [5,33]. The phase-in schedule is driven by both the size of the business and whether the business contributes to health insurance benefits for its workers [5,34]. As of its initial phase-in on 1 April 2015, the city's minimum wage increased from $9.47/h to $11/h for most large ($\geq$ 500 employees nationally) and some small businesses (<500 employees) and $10/h for other small businesses. On 1 January 2016, the city's minimum wage increased from $10–$11/h to $12.50–$13/h for large businesses and $10.50–$12/h for small businesses (<500 employees). On 1 January 2017, the city's minimum wage increased again to $13.50–$15/h for large businesses or $11–$13/h for small businesses. For the purposes of this analysis, all supermarket store chains sampled follow the large employer schedule (see Table A1).

The specifics of store selection, data collection, and the data collection instruments for this project have been described elsewhere [13,14,33,35]. In brief, data was collected from six supermarkets in Seattle and affected by the ordinance ("intervention"), and six same-chain supermarkets located in King County, but outside of Seattle and unaffected by the ordinance ("control"). We selected store chains that were the most frequently patronized by a representative sample of Seattle-King County residents, had locations in both Seattle and King County, and represented a range of prices from low to high. The six chains included in the sample were representative of 50 out of the 78 individual Seattle stores impacted by the ordinance at the time of its initial implementation in April 2015. Specific store locations were selected based on their proximity to low-income neighborhoods, based on American Community Survey data [36].

Data on 106 food and beverage items were collected using the University of Washington's (UW) Center for Public Health Nutrition (CPHN) market basket. The UW CPHN market basket, developed in 2009, is a combined and condensed version of the Consumer Price Index and United States Department of Agriculture's Thrifty Food Plan market baskets and has been enhanced to contain commonly consumed items and nutrient-rich foods and beverages [13,14,33,35,36]. This market basket tool has been used and validated in Seattle stores in a number of prior studies and is described in detail in Otten et. al (2017); it has the advantage of containing more healthy food items needed for a high quality diet [13,14,33,35,36]. Stores were visited at four times points: phase 1 (prior to policy implementation, March 2015), phase 2 (1-month post policy implementation, May 2015), phase 3 (after the phase-in to $13/h and seasonally matched to phase 2, May 2016) and phase 4 (after the phase-in to $15/h and seasonally matched to phase 2 and 3, May 2017) (see Table A1). At each visit, a trained researcher recorded the lowest non-sale price, in United States dollars (USD), for the identical or comparable item from prior visits. When multiple sizes were available for the same food item, the medium-sized item was selected. Following data collection, prices were rechecked for any anomalies, and any missing or anomalous items were rechecked at the store.

All prices were standardized to the price per 100 grams and then price per 100 kilocalories (kcal) using the United States Department of Agriculture (USDA) food database [37]. This standardization was used as it was thought to better reflect the purchasing logic of lower income shoppers, obtaining the adequate calories for an affordable price, rather than a focus on the volume of a product. This is particularly relevant when thinking about the quantity of certain low kcal, high cost items such as fresh produce. For example, 100 g of fresh cantaloupe is a very different quantity than 100 kcal. Other methods for standardizing prices include price per 100 grams and price per serving [38], among others; however, these other standardization methods are beyond the scope of this analysis.

2.2. Food Categorizations

Once food price data were collected and processed, each food and beverage item was assigned to a food group, a food processing category, and a Nutrient Rich Food Index 9.3 ($NRF_{9.3}$) quartile. Food group categorizations were based on seven food groups defined by the USDA and included "cereals and grains", "fruits", "vegetables", "dairy", "meats, beans, and proteins", "fats and oils", "sugars" and "other beverages" [39]. However, the category "other beverages" was excluded from this analysis as 1) this category contained few items, 2) since each item (coffee, malt beverages, wine) in this category had a high price to kcal ratio, these items represented outliers, and 3) these items are not covered by government programs for which many low-income families would be eligible, such as the Supplemental Nutrition Assistance Program and Women, Infants, and Children.

Food processing level categories were based on the degree to which foods remained in their natural state or underwent alteration to increase shelf life, improve flavor, or make them readily consumable. We used the NOVA food processing classification scheme described in Martínez Steele, et al. (2016), which classifies food items as unprocessed or minimally processed, processed culinary ingredients, processed foods, and ultra-processed food (see Table A2) [27]. Two researchers independently classified each food item according to this classification scheme and had agreement on ninety-three (90%) of the total 103 food items. A third researcher helped to classify the remaining ten (10%) food items.

Nutrient quality was assessed using the $NRF_{9.3}$ per 100 kcal, which calculates a nutrient density score for a food item based on the proportion of certain positive and negative macro- and micronutrients contained within [32,40,41]. To calculate the $NRF_{9.3}$, we first obtained the nutrient composition per 100 grams for each food item by linking the CPHN market basket to the Minnesota Nutrition Composition and the USDA SR 28 databases [42]. This included information on total calories along with information on nine nutrients to encourage (protein, fiber, vitamins A, C, and D, calcium, iron, potassium, and magnesium) and three nutrients to limit (added sugars, saturated fat, sodium) in a healthy diet (see Table A3) [32,40,41]. The $NRF_{9.3}$ utilizes daily value reference quantities established by the Food and Drug Administration and the USDA's Healthy Eating Index [32,40,41]. For each item in the market basket, the $NRF_{9.3}$ score was calculated using the formula [32,40,41]:

$$NRF_{9.3} = \left(\sum_{1-9} \frac{\frac{Nutrient_i}{DV_i}}{ED} - \sum_{1-3} \frac{\frac{Li}{MRV_i}}{ED} \right) \times 100 \quad (1)$$

where each of the nine nutrients to encourage (*Nutrient*) and three nutrients to limit (*Li*), per 100 kcal, is divided by the daily reference value to obtain the percent daily value (*DV*) or maximum recommended values (*MRV*), respectively. Each estimate is then divided by the energy density (*ED*) of the given food item, summed with category (encourage, *Nutrient*, or limit, *Li*), and then estimate for nutrients to limited are subtracted from those to encourage for each food item. This value is then multiplied by 100% to obtain the $NRF_{9.3}$ score. To avoid overly inflated $NRF_{9.3}$ scores for low energy density items with extremely high relative amounts of a single nutrient (e.g. green peppers), individual nutrient %DVs that exceeded 100% were top-coded to 100%. Food items were then assigned to one of four quartiles based on $NRF_{9.3}$ score quartiles with quartile 1 comprising the least nutrient dense food items and quartile 4 comprising highly nutrient dense food items (see Table A3).

2.3. Statistical Analysis

We used a difference-in-differences (DD) linear regression model to estimate the mean difference in price per 100 kcal between Seattle chain supermarkets and King County chain supermarkets over time. This approach provides the average treatment effect on the treated (Seattle) stores and assumes that the trends for food prices overtime in both treated and control stores in the pre-policy period were parallel. The percentage of missing food items over time was low (1.5% or 75 out of 4944) and therefore, we chose to conduct a complete case analysis, excluding those items differentially missing across the period of observation. We also exclude three items that had outlier prices per 100 kcal (wine, coffee, and malt beverages), due to the low caloric value relative to price, leaving 103 items for the final analysis. We used the formula:

$$\begin{aligned} Price_{ijkt} = \beta_0 + \ \beta_1 Seattle_k + \gamma_1 Post1_t + \gamma_2 Post2_t + \gamma_3 Post3_t + \ \delta_1 Post1_t \times Seattle_k \\ + \delta_2 Post2_t \times Seattle_k + \delta_3 Post3_t \times Seattle_k + \varepsilon_{ijkt} \end{aligned} \tag{2}$$

where $Price_{ijkt}$ is the estimated mean price for item i at store j in region k (1 = Seattle, "intervention", and 0 = King County, "control"), at time t. β_0 is the intercept and average price for King County stores at baseline, i.e. prior to policy implementation. $Seattle_k$ is an indicator variable that equals 1 for Seattle stores and 0 for King County store, and β_1 captures the mean difference in item-level price between Seattle and King County stores at baseline. $Post1_t$, $Post2_t$, and $Post3_t$ are indicator variables which equal 1 for each seasonally-matched data collection period: follow-up 1 (May 2015), follow-up 2 (May 2016), follow-up 3 (May 2017), respectively, and 0 if otherwise, with the reference period being the pre-policy implementation baseline. γ_1, γ_2, γ_3 are the differences in mean item-level price for each follow-up period post-policy implementation relative to the pre-policy baseline period. $Post1_t \times Seattle_k$, $Post2_t \times Seattle_k$, and $Post3_t \times Seattle_k$ equals 1 for Seattle stores in follow-up period 1, 2, and 3, respectively, relative to baseline, and δ_1, δ_2, δ_3 provide the estimated average policy treatment effect on the treated (Seattle) stores are the coefficients of interest. More specifically, δ_1, δ_2, δ_3 provide the mean difference in item-level price attributable to the minimum wage ordinance in each follow-up period, relative to baseline. ε_{ijkt} is the residual error term. To examine potential modification of the effect of minimum wage on price by food group, food processing, and nutrient density, separate DD linear regression models (Equation (2), above) were run overall and within each food quality strata—food group, food processing category, and nutrient density quartiles. We did not include additional model controls due the small number of sampled stores; however, region and time fixed effects as well as same-chain store matching across regions likely account for some unobserved confounding. Robust standard errors were clustered to account for food price correlation within stores and an α level of 0.05 was used to determine statistical significance. All analyses were conducted using Stata version 14 [43].

3. Results

Figure 1 displays the average total market basket price for Seattle and King County locations, standardized across locations such that each item had the same weight (in grams) or volume (in ounces) across stores, rather than by kcal, at baseline and follow-up periods 1 through 3. While King County stores had a slightly lower average market basket price than Seattle stores, this difference remained relatively constant overtime. This is also reflected in the non-significant, mean difference item-level price per kcal at 1-month (−\$0.01 per 100 kcal, SE = 0.026, P = 0.670), 1-year (\$0.00 per

100 kcal, SE = 0.019, P = 0.928), and 2-years ($0.00 per 100 kcal, SE = 0.024, P = 0.861) post-policy implementation between Seattle and King County locations (Table 1).

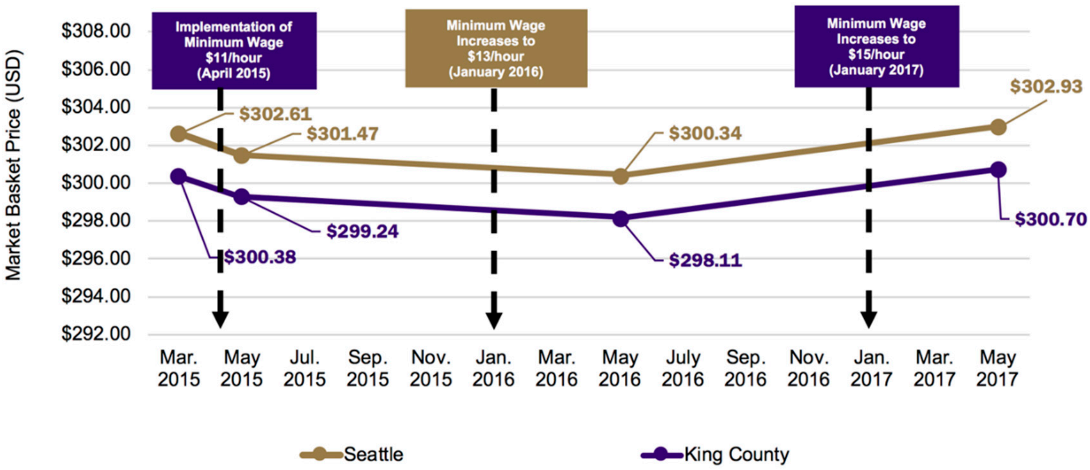

Figure 1. Average market basket price at one-month pre- and at 1-month, 1-year, and 2-years post-implementation of Seattle's minimum wage in intervention (Seattle) and control (King County) supermarkets. Notes: The minimum wage increase schedule highlighted in the figure follows that for firms with >500 employees worldwide and who do not provide health benefits for their employees.

Food group stratified, multi-level DD regression model results are shown in Table 1. While there were significant temporal changes from baseline to follow-up 3 for cereals & grains, and vegetables, as with the overall results, there was no significant effect on food prices per 100 kcal overall or by food group attributable to the Seattle Minimum Wage Ordinance. The largest observed price change by food group was for fruit, which experienced a nonsignificant decline of $0.05 per 100 kcal (SE = 0.09) from baseline to follow-up 3 in Seattle supermarkets relative to King County supermarkets. Fruit, sugar and sweets, and vegetables from baseline to follow-up and sugar and sweets from baseline to follow-up 2 experienced the next largest changes with nonsignificant declines of $0.03 per 100 kcal.

Table 1. Overall and food group stratified linear difference-in-differences model results for the mean change in item-level price per 100 kcal across Seattle ('intervention') and King County ('control') stores and over time from March 2015 to May 2017, before and during implementation of Seattle's minimum wage ordinance.

Mean Difference in Price Estimates Price per 100 kcal (in USD) (Robust Standard Errors)	Overall	Food Group						
		Cereals & Grains	Dairy	Fats & Oils	Fruits	Meats, Beans, Eggs, & Nuts	Sugar & Sweets	Vegetables
Seattle (relative to King County)	0.02 (0.082)	0.00 (0.020)	0.00 (0.039)	0.00 (0.018)	0.05 (0.139)	−0.01 (0.065)	0.03 (0.140)	0.05 (0.174)
Follow-up 1 (relative to baseline)	0.00 (0.017)	0.00 (0.009)	−0.01 (0.006)	0.00 (0.003)	−0.04 (0.057)	−0.01 (0.006)	−0.08 (0.075)	0.07 (0.043)
Follow-up 2 (relative to baseline)	−0.01 (0.014)	0.00 (0.013)	−0.01 (0.010)	0.00 (0.005)	0.01 (0.062)	−0.01 (0.016)	−0.10 (0.086)	0.02 (0.026)
Follow-up 3 (relative to baseline)	0.03 (0.019)	−0.02 * (0.011)	−0.03 (0.016)	0.00 (0.005)	0.05 (0.069)	0.00 (0.023)	−0.12 (0.087)	0.16 *** (0.034)
Seattle × Follow-up 1	−0.01 (0.026)	0.00 (0.011)	0.01 (0.012)	0.00 (0.004)	−0.03 (0.070)	0.00 (0.008)	−0.03 (0.132)	−0.03 (0.063)
Seattle × Follow-up 2	0.00 (0.019)	0.00 (0.019)	0.00 (0.014)	0.00 (0.007)	−0.02 (0.082)	0.00 (0.022)	−0.03 (0.147)	0.00 (0.036)

Table 1. *Cont.*

Mean Difference in Price Estimates Price per 100 kcal (in USD) (Robust Standard Errors)	Overall	Food Group						
		Cereals & Grains	Dairy	Fats & Oils	Fruits	Meats, Beans, Eggs, & Nuts	Sugar & Sweets	Vegetables
Seattle × Follow-up 3	0.00 (0.024)	−0.01 (0.016)	−0.01 (0.024)	0.01 (0.007)	−0.05 (0.087)	0.02 (0.031)	−0.02 (0.149)	−0.01 (0.060)
Observations	4869	665	573	185	605	1424	286	1131
Number of stores	12	12	12	12	12	12	12	12
R^2 within	0.0003	0.0031	0.0067	0.0139	0.0010	0.0006	0.0271	0.0018
R^2 between	0.0025	0.2010	0.0000	0.0036	0.0069	0.0000	0.0000	0.0054
R^2 overall	0.0003	0.0032	0.0055	0.0036	0.0012	0.0006	0.0253	0.0019

Notes: Baseline, March 2015 (1-month pre-policy enactment); follow-up 1, May 2015 (1-month post-policy enactment); follow-up 2, May 2016 (1-year post-policy enactment); follow-up 3 (2-years post-policy enactment). Robust standard errors are clustered by store. kcal = kilocalories. USD = United States dollars. p Values come from Wald tests. *** $p < 0.001$, * $p < 0.05$.

Table 2 displays DD regression results for mean difference in price estimates over time between Seattle and King County supermarkets by level of food processing. There was no significant minimum wage effect on mean difference in price per 100 kcal across any food processing category strata. The largest observed price change by level of food processing in Seattle supermarkets relative to King County supermarkets was for was for unprocessed or minimally processed foods, which experienced a nonsignificant decline of $0.02 per 100 kcal (SE = 0.04) from baseline to follow-up 1.

Table 2. Food processing category stratified linear difference-in-differences model results for the mean change in item-level price per 100 kcal across Seattle ('intervention') and King County ('control') stores and over time from March 2015 to May 2017, before and during implementation of Seattle's minimum wage ordinance.

Mean Difference in Price Estimates Price per 100 kcal (in USD) (Robust Standard Errors)	Food Processing Category			
	Unprocessed or Minimally Processed Foods	Processed Culinary Ingredients	Processed Foods	Ultra-Processed Foods
Seattle (relative to King County)	0.03 (0.107)	−0.00 (0.010)	0.01 (0.096)	0.01 (0.054)
Follow-up 1 (relative to baseline)	0.02 (0.027)	0.00 (0.003)	−0.03 * (0.011)	−0.02 (0.015)
Follow-up 2 (relative to baseline)	−0.01 (0.017)	0.01 *** (0.002)	−0.01 (0.033)	−0.01 (0.024)
Follow-up 3 (relative to baseline)	0.08 *** (0.022)	0.00 (0.002)	−0.04 (0.038)	−0.04 (0.024)
Seattle × Follow-up 1	−0.02 (0.037)	0.00 (0.003)	0.01 (0.018)	0.00 (0.029)
Seattle × Follow-up 2	0.01 (0.021)	0.00 (0.003)	−0.01 (0.049)	−0.01 (0.038)
Seattle × Follow-up 3	−0.01 (0.034)	0.00 (0.003)	0.01 (0.045)	0.00 (0.039)
Observations	2,778	323	480	1,288
Number of stores	12	12	12	12
R^2 within	0.0008	0.0107	0.0011	0.0049
R^2 between	0.0052	0.0070	0.0006	0.0011
R^2 overall	0.0009	0.0094	0.0010	0.0043

Notes: Baseline, March 2015 (1-month pre-policy enactment); follow-up 1, May 2015 (1-month post-policy enactment); follow-up 2, May 2016 (1-year post-policy enactment); follow-up 3 (2-years post-policy enactment). Robust standard errors are clustered by store. kcal = kilocalories. USD = United States dollars. p Values come from Wald tests. *** $p < 0.001$, * $p < 0.05$.

DD regression results for mean difference in price estimates over time between Seattle and King County supermarkets by nutrient density score, as defined by $NRF_{9.3}$ quartiles, are shown in Table 3. Similar to the results of the analysis of effects across food groups and level of food processing, there was no significant minimum wage effect on the mean difference in price per 100 kcal in any $NRF_{9.3}$ quartiles. The largest observed price change by $NRF_{9.3}$ quartiles in Seattle supermarkets relative to King County supermarkets was for $NRF_{9.3}$ quartile 4: "highly nutrient dense foods", which experienced a nonsignificant decline of $0.04 per 100 kcal (SE = 0.07) from baseline to follow-up 3.

Table 3. Nutrient rich food index 9.3 (NRF 9.3) quartile stratified linear difference-in-differences model results for the mean change in item-level price per 100 kcal across Seattle ('intervention') and King County ('control') stores and time, from March 2015 to May 2017, following implementation of Seattle's minimum wage ordinance.

Mean Difference in Price Estimates Price per 100 kcal (in USD) (Robust Standard Errors)	**NRF 9.3 Quartile**			
	Quartile 1: Least Nutrient Dense Foods	**Quartile 2: Moderately Nutrient Dense Foods**	**Quartile 3: Nutrient Dense Foods**	**Quartile 4: Highly Nutrient Dense Foods**
Seattle (relative to King County)	0.01 (0.051)	−0.00 (0.033)	0.01 (0.099)	0.06 (0.169)
Follow-up 1 (relative to baseline)	−0.02 (0.016)	0.01 (0.006)	−0.02 (0.029)	0.05 (0.039)
Follow-up 2 (relative to baseline)	−0.02 (0.021)	−0.01 (0.009)	0.00 (0.025)	0.01 (0.030)
Follow-up 3 (relative to baseline)	−0.04 (0.021)	0.00 (0.014)	−0.01 (0.039)	0.18 *** (0.032)
Seattle × Follow-up 1	−0.00 (0.030)	−0.00 (0.007)	−0.01 (0.040)	−0.03 (0.061)
Seattle × Follow-up 2	−0.01 (0.035)	0.01 (0.017)	0.00 (0.034)	0.00 (0.037)
Seattle × Follow-up 3	0.00 (0.035)	0.00 (0.020)	0.01 (0.047)	−0.04 (0.067)
Observations	1236	1194	1266	1173
Number of stores	12	12	12	12
R^2 within	0.0048	0.0003	0.0001	0.0030
R^2 between	0.0090	0.0002	0.0003	0.0071
R^2 overall	0.0050	0.0002	0.0001	0.0032

Notes: Baseline, March 2015 (1-month pre-policy enactment); follow-up 1, May 2015 (1-month post-policy enactment); follow-up 2, May 2016 (1-year post-policy enactment); follow-up 3 (2-years post-policy enactment). Robust standard errors are clustered by store. kcal = kilocalories. USD = United States dollars. *p* Values come from Wald tests. *** $p < 0.001$.

4. Discussion

This analysis expands on our prior work and contributes to the growing body of work evaluating the effect of minimum wage policies on food prices in four key ways. First, we evaluate changes in food prices per 100 kcal which tie changes in food prices more closely to how food purchasing decisions are likely to be made by price-sensitive individuals. Second, we evaluate an additional year of exposure to the Seattle Minimum Wage Ordinance as well as an additional increase in minimum wage. Third, this analysis presents the first evaluation of a $15 per hour minimum wage on local area supermarket food prices (Table A1). Fourth, we add an additional metric, the Nutrient Rich Food Index 9.3, as a way to further evaluate differential changes in food prices which are relevant to health. The results of this study lead us to conclude that local minimum wage increases did not result in changes in prices of food items sold at policy-affected supermarkets. There was also no evidence for differential effects of minimum wage increases on prices studied using multiple food quality metrics. These findings are important for two reasons. First low-wage earners and their families spend a larger share of their income on food [44]. Second, lower incomes have been linked to increased consumption of lower cost, energy dense foods of minimal nutritional value, and to poor health outcomes [24,36,45,46].

A net increase in wages could be used to purchase more nutritious food options, provided that the price of food stays constant. However, a recent, cross-sectional evaluation of the effect of the Seattle Minimum Wage Ordinance on low-wage jobs [47] found that the increase to $13/h reduced hours among low-wage jobs by 6–7% while hourly wages increase by 3%, leading to an overall reduction in income, on average. A follow-up, longitudinal analysis of low-wage workers [48] found that the most experienced half of workers (above median experience) saw earnings gains of approximately $19 per week while those with less experience (below median) saw little to no change, on average.

The present work adds to a limited but growing body of evidence evaluating the effect of minimum wage increases on grocery store prices in the United States [13,14,49–51]. The results of this evaluation and our prior studies [13,14] most closely align with the work of Ganapati and Weaver (2017) who found minimal evidence of pass-through effects on grocery store prices using national Nielsen retail price scanner data from 2005 to 2015 [49]. However, Leung (2018) also used national Nielsen retail price scanner data from 2006 to 2015 to evaluate the effect of federal, state, and local minimum wage increases and found that a 10% increase in minimum wage raised grocery store prices by 0.6–0.8%; however, there was a wide degree of effect heterogeneity by county-level and household-level socioeconomic status [50]. The estimated pass-through effect in poorer counties (defined by being below median Kaitz index) was greater than that of their rich counterparts and poorer households also reduced the intensity of their shopping following a minimum wage increase [50]. Another study by Renkin, et al (2017) found that a 20% increase in effective minimum wage led to a 0.4% increase in grocery store food prices in the first three months following the increase [51] in contrast to our results which did not observe any such immediate increase 1-month following Seattle's minimum wage increase [13,14].

Further inference can be garnered from the evaluation of minimum wage increases and food prices in the fast-food and larger restaurant industries [10,11,52–56]. Seminal work by Katz and Kreuger (1992) and Card and Kreuger (1994) in examining federal minimum increases on fast-food restaurant food prices observed little effect that could be attributed to higher minimum wages [10,52]. MacDonald and Aaronson (2006) reported fast food price increases of 1.56% for a 10% increase in minimum wage, but also that item selection for price increases was related to other factors, such as a recent price reduction of the item [57]. Powers (2009) assessed the response of fast food prices to a 2004 state-level minimum wage increase in Illinois and found mixed evidence of an impact on fast food prices, however, the strongest price effects were observed in entrées [53]. Basker and Khan (2016) priced three menu items at New York City fast food restaurants throughout 1993–2014 to analyze price changes in response to five federal minimum wage increases that were implemented over the study period [54]. Their study found that a federal minimum wage increase of 33% (from $7.25/h to $10.10/h) would raise in fast food prices by 3%, an average increase of $0.10 per item [54]. However, the authors note that such an increase would likely be wholly offset by the corresponding increase in individual earnings among low-wage workers [54]. With respect to local minimum wage ordinances, Allegretto and Reich (2018) found that restaurant food prices in San Jose increased by 1.45% in response to a 25% increase in local minimum wage while Dube (2007) found a significant increase in prices of 6.2% at small and midsize fast food restaurants with table service in response to a 26% increase in minimum wage.

There are several reasons for the null findings we observed in our analyses. First, supermarkets may be using alternative channels to offset their increased costs, which are not consumer-facing. A report on the labor market effects of the Seattle minimum wage ordinance found that although minimum-wage workers experienced an increase in wages, a reduction in hours of approximately 35–50 min per week per worker was also observed following the implementation of the policy [47]. Moreover, in a survey of Seattle employers, reducing hours or headcount was the second most common channel of adjustment across all for-profit industries following raising prices or adding fees [58]. Second, supermarkets may be raising the price of food items that were not included in the CPHN market basket, such as in-house prepared meals, salads, or bakery items [59]. Of the six supermarket chains surveyed, five had large prepared foods sections. Moreover, these items would require more labor by in-house employees. Third, supermarket prices may be more sensitive to national rather than local changes, particularly for large chain stores such as those included in this study. A market basket survey of Seattle supermarkets from 2002 to 2014 found that local-area food prices track closely with changes in the Consumer Price Index [38]. Another study found that within-chain price rigidity attenuated much of the observed effect of local minimum wage increases on retail prices [50]. Fourth, it is possible that employees at the sampled supermarket chains were earning at or above $15 per hour prior to the implementation of the ordinance and therefore the minimum wage increases implemented by the ordinance may not have been as binding for workers in this industry. Four out of the six supermarket chains were unionized [13,14] and larger firms, those with 500 or more employees globally, have been shown to be early adopters of mandated minimum wage increases [58]. However, we have previously shown [13,14], using administrative data, that twice as many grocery store jobs in Seattle and three times as many grocery store jobs (NAICS code 445110) in King County paid $11 an hour or less in the year prior to the passage of the Seattle minimum wage ordinance as compared with jobs in all other industries. Therefore, we believe it is likely that the minimum wage increases would have still lead to increased labor costs for these supermarket chains.

This study, however, was not without its limitations. First, this study did not collect data on food items prepared in-house, which requires more labor from supermarket employees. Second, prices reflect the lowest, non-discounted price and we are therefore unable to evaluate any sale or pricing strategies supermarkets may be employing to sway consumer choice. Third, while we attempted to evaluate food items by attributes most relevant to diet quality and, therefore, health, these results do not capture food purchasing habits of consumers nor do they speak to the diets or health of consumers. Fourth, Seattle's unique economic conditions may limit the generalizability of our findings to other local minimum wage initiatives. Fifth, the timing of the implementation of the Seattle Minimum Wage Ordinance relative to the start of this funded evaluation did not allow for the collection of multiple pre-policy time points; therefore, we cannot determine with certainty that the food prices between Seattle and King County meet the parallel trends condition for difference-in-differences analysis. Future studies should consider evaluating the potential moderating role of region or other area-level socio-demographics on the effect of minimum wage on food prices. In addition, studies should consider using sales data to evaluate changes in consumer behavior as well as including prepared food items in their evaluation.

5. Conclusions

We find no evidence of changes in food prices overall or by food group, level of food processing, or nutrient density score, attributable to two years of exposure to the Seattle minimum wage ordinance and an increase in hourly wage to $13.50–$15.00 per hour. These null findings are encouraging as higher wages without a corresponding increase in the price of food may provide low-wage households with greater purchasing power for more healthy, nutritious foods. Given the growing trends of cities and states adopting higher minimum wages, other jurisdictions may find this market basket tool, as well as the categorization of food items, useful in evaluating the effect of wage increases on food prices in their area.

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Appendix A

Table A1. Timeline of Seattle's minimum wage increase during data collection.

Date of Data Collection	Minimum Wage Rate for Large Employers Not Paying Towards Employee Medical Benefits (in USD)	Minimum Wage Rate for Large Employers Paying Towards Employee Medical Benefits (in USD)	Time Point
March 2015	$9.47/h	$9.47/h	1-month pre-enactment
May 2015	$11.00/h	$11.00/h	1-month post-enactment
May 2016	$13.00/h	$12.50/h	1-year post-enactment
May 2017	$15.00/h	$13.50/h	2-year post-enactment

Notes: USD = United States dollars. Large employers are defined as 501 or more employees internationally. Two other phase-in schedules are possible for employers with 500 or fewer employees based on whether they contribute to employee benefits. For more information, please visit: https://www.seattle.gove/laborstandards/ordinances/minimum-wage.

Table A2. Food processing categorization based on the level of processing.

Food Processing Category	Defined as	Market Basket Examples
Unprocessed or minimally processed foods	Foods taken directly from nature; minimally processed to clean, pasteurize, freeze, or other processes that do not alter the composition	Rice, milk, apples, frozen turkey, broccoli (*n* = 59)
Processed culinary ingredients	Ingredients that can be added to unprocessed or minimally processed foods for flavor or seasoning used in the cooking process	Flour, butter, shortening, sugar (*n* = 12)
Processed foods	Unprocessed or minimally processed food that are processed or further processed, often with salt or oil, with the intent of extending shelf-life or altering palatability	Tortillas, tofu, canned salmon, canned corn (*n* = 10)
Ultra-processed foods	Foods that are highly processed with the intent of convenience and ready-to-eat/drink	Cookies, ice cream, salad dressing, sausages, cola, potato chips (*n* = 27)

Notes: NOVA food processing classification scheme taken from Martínez Steele, et al. (2016) [27]. Processed foods also include fermented alcoholic beverages; however, these were excluded from the present analysis.

Table A3. Nutrient density quartiles based on $NRF_{9.3}$ score.

Nutrient Density Quartile	Mean $NRF_{9.3}$ Score ±SD	$NRF_{9.3}$ Score Range	Example Foods
Quartile 1—Least nutrient dense foods	−13.6 ± 17.4	−51.6–8.8	Butter, cookies, bologna, potato chips, cola, cheese (*n* = 26)
Quartile 2—Moderately nutrient dense foods	20.2 ± 5.7	9.26–30.2	Potatoes, Turkey Eggs, steak, rice, bread (*n* = 25)
Quartile 3—Nutrient dense foods	56.8 ± 22.8	30.3–112.5	Peaches, chicken breast, beans, grapes, cereal, salmon, bananas (*n* = 27)
Quartile 4—Highly nutrient dense foods	232.3 ± 90.1	117.8–479.5	Green beans, spinach, grapefruit, sweet peppers, carrots, cantaloupe, asparagus, sweet potatoes (*n* = 25)

Notes: NRF = Nutrient Rich Foods index.

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How Effective Is the Minimum Wage at Supporting the Poor?

Author(s): Thomas MaCurdy

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How Effective Is the Minimum Wage at Supporting the Poor?

Thomas MaCurdy

Stanford University

This study investigates the antipoverty efficacy of minimum wage policies. Proponents of these policies contend that employment impacts are negligible and suggest that consumers pay for higher labor costs through imperceptible increases in goods prices. Adopting this empirical scenario, the analysis demonstrates that an increase in the national minimum wage produces a value-added tax effect on consumer prices that is more regressive than a typical state sales tax and allocates benefits as higher earnings nearly evenly across the income distribution. These income-transfer outcomes sharply contradict portraying an increase in the minimum wage as an antipoverty initiative.

I. Introduction

The widespread popularity of raising the minimum wage draws heavily on its appeal as an antipoverty policy, which relies on two beliefs: first, raising the minimum wage will increase the incomes of poor families, and second, the minimum wage imposes little or no public or social costs. Indeed, in 2006 a group of more than 650 economists signed a widely distributed statement issued by the Economic Policy Institute expressing these sentiments in support of legislation calling for a 40 percent increase in the federal minimum wage. This support along with broad

Aspects of the arguments and approach in this study have appeared in several non-peer-reviewed reports and working papers written by me (and different coauthors) since the late 1990s. These reports/papers greatly benefited from discussion, comments, and expert research assistance from Frank McIntyre, Peggy O'Brien-Strain, and Selen Opcin. For this updated paper and newly produced empirical results, I gratefully acknowledge many useful contributions from Kevin Mumford.

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acceptance of these beliefs encouraged policy makers in Washington, DC, to raise the minimum wage from $5.15 in 2007 to $7.25 in 2009.

The policy debate over the minimum wage principally revolves around its effectiveness as an antipoverty program. A popular image used by both sides of the debate consists of families with breadwinners who earn low wages to support their children. Policies that raise the wages of these workers increase their earnings and contribute to their escaping poverty. As a counterbalance to this impact, opponents of the minimum wage argue that wage regulation causes some low-wage workers to lose their jobs and they will suffer income drops. The issue, then, becomes a trade-off: some low-income breadwinners will gain and others will lose. Promoters of the minimum wage retort that employment losses are quite small, and consequently, the workers who gain far exceed those who lose.

In addition to potential adverse employment effects, opponents of minimum wages further counter the belief that the minimum wage assists poor families by documenting that many minimum wage workers are not breadwinners of low-income families. They are, instead, often teenagers, single heads of household with no children, or not even members of low-income families. Promoters of the minimum wage admit that some of these groups may also benefit from the wage increase, but since few workers lose jobs, they contend that the minimum wage still benefits low-income families with children.

The notion that the minimum wage can be increased with little or no economic cost underlies many advocates' assessments of the effectiveness of the minimum wage in its antipoverty role. Most economists agree that imposing wage controls on labor will not raise total income in an economy; indeed, elementary economics dictates that such market distortions lead to reduced total income, implying fewer overall benefits than costs. If, however, one presumes that employment losses do not occur and total income does not fall, then the minimum wage debate becomes a disagreement over how it redistributes income. The efficacy of a minimum wage hike as an antipoverty program depends on who benefits from the increase in earnings and who pays for these higher earnings. Whereas a number of studies have documented who benefits, who pays is earnings far less obvious. But someone must pay for the higher earnings received by the low-wage workers.

At the most simplistic level, the employer pays for the increase. However, businesses do not actually pay, for they are merely conduits for transactions among individuals. Businesses have three possible responses to the higher labor costs imposed by the minimum wage. First, they can reduce employment or adjust other aspects of the employment relationship (e.g., fewer fringe benefits or training opportunities), in which case some low-wage workers pay themselves through loss of their jobs or by receiving fewer nonsalary benefits; second, firms can lose profits, in which case owners pay; and, third, employers can increase prices, wherein consumers pay.

Of these three sources, entertaining that low-wage workers bear any cost of the minimum wage has been largely dismissed by proponents in recent years on the basis of several (albeit much disputed) studies that found little or no job loss following historical increases in federal and state minimum wages. While the extra resources needed to cover higher labor costs could theoretically come out of profits, several factors suggest that this source is the least likely to bear costs. Capital and entrepreneurship are highly mobile and will eventually leave any industry that does not yield a return comparable to that earned elsewhere. This means that capital and entrepreneurship, and hence profits, will not bear any significant portion of a "tax" imposed on a particular factor of production. Stated differently, employers in low-wage industries are typically in highly competitive industries such as restaurants and retail stores, and the only option for these low–profit margin industries becomes lowering exposure to low-wage labor or raising prices. With jobs presumed to be unaffected, this leaves higher prices as the most likely candidate for covering minimum wage costs. In fact, supporters of minimum and living wage initiatives often admit that slight price increases pay for higher labor costs following minimum wage hikes.

To evaluate, then, the redistributive effects of the minimum wage adopting the view implicitly held by its advocates, this study examines the antipoverty effectiveness of this policy presuming that firms raise prices to cover the full amount of their higher labor costs induced by the rise in wages. In particular, the analysis simulates the economy taking into account both who benefits and who pays for a minimum wage increase assuming that its costs are all passed on solely in the form of higher consumer prices. The families bearing the costs of these higher prices are those consumers who purchase the goods and services produced with minimum wage labor. In actuality, most economists expect that some of these consumers would respond to the higher prices by purchasing less, but such behaviors directly contradict the assertion of no employment effects since lower purchases mean that fewer workers would be needed to satisfy demand. Consequently, to keep faith with the view held by proponents, the simulations carried out in this study assume that consumers do not alter their purchases of the products and services produced by low-wage labor and they bear the full cost of the minimum wage rise. This approach, then, maintains the assumption of a steady level of employment, the "best-case" scenario asserted by minimum wage proponents. Although highly stylized and probably unrealistic, the following analysis demonstrates that the minimum wage can have unintended and unattractive distributional effects, even in the absence of the employment losses predicted by economic theory.

To evaluate the distributional impacts of an increase in the minimum wage, this study investigates the circumstances applicable in the 1990s when the federal minimum wage increased from \$4.25 in 1996 to \$5.15 in 1997.[1] To identify families supported by low-wage workers and to measure the effects on their earnings and income, this analysis uses data from waves 1–3 of the 1996 Survey of Income and Program Participation (SIPP). To translate the higher earnings paid to low-wage workers into the costs of the goods and services produced by them, this study relies on national input-output tables constructed by the Minnesota Impact Analysis for Planning (IMPLAN) Group, matched to a time period comparable with SIPP's. To ascertain which families purchase the goods and services produced by low-wage workers and how much more they pay when prices rise to pay for minimum wage increases, this study uses data from the Consumer Expenditure Survey (CES), again matched to the same time period as SIPP's. The contribution of this study is not to estimate the distribution of benefits of the minimum wage, nor is it to estimate the effect on prices; both of these impacts have already been examined in the literature. Instead, the goal of this paper is to put the benefit and cost sides together to infer the net distributional impacts of the minimum wage on different categories of families and to translate this impact into a format readily accessible to economists and policy makers.

To provide an economic setting for evaluating the distributional measures presented here, this study develops a general equilibrium (GE) framework incorporating minimum wages. This model consists of a two-sector economy with the two goods produced by three factors of production: low-wage labor, high-wage labor, and capital. A particular specification of this GE model justifies the computations performed in the analysis, and entertaining alterations in its behavioral elements permits an assessment of how results might change with alternative economic assumptions. The model proposed here goes well beyond what is currently available in the literature, which essentially relies on a Heckscher-Ohlin approach with fixed endowments (supplies) of labor and capital inputs. In contrast, the GE model formulated in this study admits flexible elasticities for both input supplies and consumer demand, as well as a wide range of other economic factors.

Seven sections make up the remainder of this paper. Section II reviews the economics literature on the responses available to employers to pay for the higher labor costs imposed by the minimum wage, and it relates these survey findings to the simulation method used in this paper. Section III overviews the methodology and data used to carry out the simulations of minimum wage impacts. Section IV characterizes who ben-

[1] This increase was done in two steps: an increase from \$4.25 to \$4.75 on October 1, 1996, and then to \$5.15 on September 1, 1997. Adjusting for inflation, the \$5.15 minimum wage in 1997 was worth about \$7.00 in 2010.

efits from an increase in the national minimum wage, and Section V describes who pays for this increase. Section VI calculates the net distributional effects of a rise in the minimum wage. Section VII discusses limitations of the analytical approach used here within a coherent GE model of the distributional impacts of the minimum wage. Finally, Section VIII summarizes the findings.

II. Paying for the Minimum Wage

This section reviews the economics literature on how employers respond to the higher labor costs imposed by the minimum wage and relates the findings from this literature to the simulation method used in this paper. The distributional effects of a minimum wage increase depend in part on who pays the costs of the policy change. The literature has focused on three possible responses (not mutually exclusive): first, employers could respond by reducing the hours of work or the number of employees (workers pay); second, firms could increase prices (consumers pay); and/or third, businesses could not respond at all, which would leave them with lower profits (owners pay). The first three subsections below discuss the economic reasoning and evidence for each of these responses, and the last subsection specifies the assumptions maintained in the following simulation analysis.

A. *Reducing Employment*

Economics research on the minimum wage has predominantly focused on the issue of employment losses. This focus draws on a fundamental tenet of economic theory: all else being equal, agents purchase less of a good as its price rises. According to this theory, not only will employers reduce their employment to mitigate costs associated with a minimum wage hike, they will also tend to reduce output and/or increase the utilization of other factors of production. For each potential employee, the firm decides whether having additional hours will increase the firm's revenue sufficiently to justify that worker's wage. For some firms, the extra revenue generated by the least productive workers becomes insufficient to justify their wages, so employment falls. In this scenario, low-wage workers bear part of the cost of an increase in the minimum wage through reduction in employment and hours of work (also possibly through reductions in forms of compensation other than earnings).[2] The

[2] In addition to reducing fringe benefits and training, minimum wage employers can also presumably demand greater effort (i.e., higher productivity) from the minimum wage workers who remain employed. Given the limited fringe benefits and training in these jobs, increased effort may well present a more important margin of adjustment. Moreover, higher wages may lower employment costs through reduced turnover. However, as in the

vast majority of the debate over the minimum wage revolves around measuring the rate at which a rise in the minimum wage affects employment.

Prior to the 1990s, economists widely held the view that minimum wage increases primarily adversely affect the employment of young workers under age 25. In their survey of 25 time-series studies of youth employment published between 1970 and 1981, Brown, Gilroy, and Kohen (1982) conclude that a 10 percent increase in the minimum wage can be expected to reduce teenage employment by 1–3 percent according to existing empirical evidence; in their review of a smaller number of cross-section studies, the estimated decrease in teenage employment ranged from zero to over 3 percent for a 10 percent increase in the minimum wage. The accumulated research of this era generally maintains that young adults beyond the teenage years experience notably smaller negative employment impacts than their teenage counterparts.

Research in the 1990s onward challenged this conventional wisdom through a series of studies that exploited variation in state-specific minimum wages above the federal level as a primary source of data to measure the impacts of the minimum wage. This literature, comprising more than 100 papers written over the past two decades, has become known as the "new minimum wage research." The most influential work in this literature finds no negative employment effects, and some studies even suggest that employment increases in reaction to minimum wage hikes. Card and Krueger's 1995 book *Myth and Measurement* compiles some of the most prominent work in this area. Card and Krueger (1994) examine fast-food employment in New Jersey and Pennsylvania before and after the 1992 increase in New Jersey's minimum wage. With point estimates suggesting a positive employment effect, Card and Krueger conclude, "we believe that, on average, the employment effects of a minimum-wage increase are close to zero" (383). Other studies discussed in *Myth and Measurement,* including Card (1992a, 1992b) and Katz and Krueger (1992), further support this conclusion. More recent studies by Card and Krueger (2000), Zavodny (2000), Dube, Naidu, and Reich (2007), Dube, Lester, and Reich (2010), and Allegretto, Dube, and Reich (2011) produce similar findings. As economic rationales for explaining their empirical findings, this line of research predominantly cites two characterizations of labor markets: a monopsonistic labor market of the sort discussed by Stigler (1946) and bilateral search models with heterogeneous workers of the sort proposed in Lang and Kahn (1998).

This challenge of the conventional wisdom about minimum wage impacts has not gone unanswered in the literature. Several studies directly

case of greater effort, optimal selection of such counterbalancing factors is already available to employers through voluntarily raising wages, and thus, mandated minimum wages can be expected to raise unit labor costs overall, which must be paid for by some source(s).

critique the approaches used to derive the "new" conclusions (e.g., Deere, Murphy, and Welch 1995; Kim and Taylor 1995; Welch 1995; Burkhauser, Couch, and Wittenburg 2000; Neumark and Wascher 2000). Others studies confirm the consensus view of the 1980s and find negative employment effects primarily concentrated among younger workers (e.g., Currie and Fallick 1996; Neumark 2001; Williams and Mills 2001; Neumark and Wascher 2002; Neumark, Schweitzer, and Wascher 2004).[3] Further, the surveys of Brown (1999) and Neumark and Wascher (2007) point out that much of the empirical work in the "new" research actually estimates small and negative employment responses to increases in minimum wages.

Nevertheless, the widely held view today in the economics profession maintains that relatively modest increases in the minimum wage exert negligible impacts on employment. In particular, according to a survey of senior faculty from the top research universities in the United States conducted by the Initiative on Global Markets, only 40 percent (confidence weighted) believe that raising the federal minimum wage would make it noticeably harder for low-skilled workers to find employment.[4] Advocates of the minimum wage often cite such consensus when arguing that impacts on employment can be ignored.

B. *Raising Prices*

A cost of the minimum wage commonly acknowledged by its advocates concerns its impacts on prices. The labor demand curve, which leads to the basic conclusions about employment effects, assumes that product prices are held constant. This is a reasonable assumption for firms that compete with other firms that are not affected by the minimum wage increase, such as overseas or high-tech firms that employ higher-wage workers. However, many of the industries that employ minimum wage workers do not compete in such markets. These include the types of service industries that make up the largest share of low-wage employers: eating and drinking establishments and retail trade. For these industries, an increase in the minimum wage principally represents an industrywide increase in costs. Therefore, prices for low-wage goods will rise. (Output could also fall, depending on the price sensitivity of consumers, but this reaction is often presumed not to occur to avoid the implications for re-

[3] The book entitled *Minimum Wages* published in 2010 by Neumark and Wascher summarizes the findings of these studies and many others.

[4] More precisely, 40 percent agree that raising the minimum wage would adversely affect the employment of low-wage workers, 38 percent disagreed, and 22 percent are uncertain. Only 16 percent do not favor indexing the minimum wage to inflation as a desirable antipoverty policy (see http://www.igmchicago.org/igm-economic-experts-panel/poll-results?SurveyID=SV_br0IEq5a9E77NMV).

duction in employment.) In this price increase scenario, some of the burden of the minimum wage increase falls on the consumers of low-wage products.

Although rigorous research on the subject is somewhat limited, a body of work has developed examining the impact of a minimum wage on prices. The basic theoretical predictions were first noted by Stigler (1946) and have been further described by Hamermesh (1993) and Aaronson and French (2007). Lemos (2008) surveys the empirical literature in this area and presents evidence supporting the claim that prices rise as a result of minimum wage increases. Synthesizing the findings of nearly 30 studies, this survey assesses estimated price elasticities in response to minimum wage increases equal up to 0.4 for food prices and up to 0.04 for overall prices.

One set of studies directly estimates price impacts (e.g., Card and Krueger 1995; Aaronson 2001; Lemos 2006; MacDonald and Aaronson 2006; Aaronson, French, and MacDonald 2008). Aaronson (2001), for example, explores the effects of increasing the minimum wage on restaurant prices using a competitive market model. From several data sources on restaurant prices in the United States and Canada, Aaronson's results show that a 1 percent increase in the minimum wage leads to a statistically significant increase of approximately 0.07 percent in restaurant prices in both countries. Moreover, he finds that these price adjustments are short-run phenomena concentrated in the quarters before and after the enactment of the minimum wage increase. Card and Krueger (1995, 54) conclude that "prices rose 4% faster as a result of the minimum-wage increase" based on a comparison of price growth in New Jersey and Pennsylvania after a minimum wage increase in New Jersey, although the impacts on prices are imprecisely estimated in their cross-state comparisons. Still, Card and Krueger surmise that two different sources of data (city-specific consumer price indexes and observations on hamburger prices collected by the American Chamber of Commerce Research Association) indicate the same pattern of faster price increases in areas more affected by minimum wage increases. In fact, they find that the relationship between higher wages and these higher prices approximates the labor share of product costs, a result consistent with the theory that the majority of the costs are being passed on in higher prices.

Another set of studies indirectly estimate price impacts of minimum wages using input-output models to trace wage increases on the interindustry flow of goods and services to simulate impacts on employment, output, and prices in the aggregate economy and various market sectors. Assuming a full pass-through effect, no substitution effects, no employment effects, and no spillover effects, Wolf and Nadiri (1981) used an input-output model and data from the Current Population Survey to estimate the price effects attributable to the 1963, 1972, and 1979 minimum wage increases. They estimate that a 10–25 percent minimum wage increase raises prices by 0.3–0.4 percent. Under similar assumptions, Lee and O'Roark (1999) use an input-output model to estimate price effects in the food and food service industries. They calculate that a 50-cent minimum wage increase would raise consumer prices of food and kindred products by approximately 0.3 percent. Moreover, the same increase would raise prices by 0.9 percent in eating and drinking establishments, an industry with a higher concentration of minimum wage workers and a larger share of labor costs. They also consider the potential impacts of wage spillovers that refer to increases in wages that occur for those earning slightly more than the minimum wage. This spillover leads to consumer prices increasing slightly more, but never by more than 1.5 percent in eating and drinking establishments and by 0.4 percent in food and kindred products.

Not all empirical studies find evidence of rising prices in response to a minimum wage increase. Katz and Krueger (1992), Machin, Manning, and Rahman (2003), and Draca, Machin, and Van Reenen (2011) do not obtain statistically significant impacts. But this evidence is not compelling since the predicted impacts of minimum wages on prices are small and price data are highly variable and influenced by many factors.

While the precise magnitude of the responsiveness of prices to minimum wage hikes is not firmly established, the direction of the price response seems clear. Most economists and policy makers accept the view that higher minimum wages translate into higher prices for the goods and services produced either directly or intermediately by low-wage workers affected by these policies. At least some of the burden of the increased wage bills faced by low-wage firms is passed on to the consumer through higher prices.

C. *Reducing Profits*

Since the minimum wage forces employers to pay higher wages, many policy makers and voters presume that minimum wages will be paid out of employer profits. However, a variety of reasons lead one to suspect that profits will not be a significant source for paying the costs of minimum wages. Most economic theory does not suggest that profits are a likely source of covering costs. Rebitzer and Taylor (1995), for example, show in a simple employment matching model with a large number of employers that the introduction of a minimum wage does not reduce profits for employers. Also, Card and Krueger (1995) demonstrate that the introduction of a minimum wage in an efficiency wage model does not reduce profits for employers.

From a less formal perspective, low-wage employers are less likely than other employers to have large profits. The firms that typically employ low-wage workers are in highly competitive industries. Internal Revenue Service data from corporate income tax returns for major industries that employ low-wage workers (e.g., food stores, eating and drinking establishments, retail trade and department stores) show that most of these industries have lower net incomes than the average across all industries. Low-wage workers are also more likely to work for small employers (e.g., see Card and Krueger 1995). Small employers face greater competition in both the labor market and the product market, meaning that they are unable to command monopoly power in the hiring of workers or in the setting of product prices and therefore have lower profits.

Moreover, even among the most profitable firms, capital is likely to bear little, if any, of the costs of a wage increase. This is especially true for large, publicly traded firms. It is a general result in public finance that taxes are borne by those who are least able to adjust. Capital stock markets are extremely efficient, and the supply of capital is very price sensitive, meaning that a small decrease in returns to capital will cause investors to move their money into a firm with better returns. Firms therefore cannot reduce the returns on their stock and still expect investment.

Unfortunately, little empirical research exists on this subject. Card and Krueger (1995) use an event study of stock prices of firms that employ many low-wage workers such as McDonald's and Wal-Mart. However, stock prices follow investors' expectations about future profitability, so the connection between stock prices and the minimum wage is tenuous at best. Card and Krueger find little systematic relationship between excess returns and news about minimum wage changes. Using data from the United Kingdom, Draca et al. (2011) find some evidence suggesting that the minimum wage reduces firm profits in the very short run, but the long-run impacts are left unanswered.

In the case of small business employers, responses in entrepreneurial resources and capital investments to increases in factor prices are likely to occur over longer periods but would nonetheless mostly neutralize impacts on profits. While entrepreneurs may not be able to shift rapidly from an industry because of their specific skills and fixed costs, those on the margin will do so over time. The opportunity cost of small business entrepreneurs is to become highly paid employees. A reduction in their "profits" (i.e., their earnings) will induce the least profitable of them to move to their next-best alternatives through the closure of establishments. Consequently, just like capital, entrepreneurial resources will shift out of those industries with increased factor costs until equalization of returns is reestablished across industries.

Thus, despite the popular belief that firms pay for minimum wage increases through lower profits, there is little empirical evidence to date supporting this hypothesis, and basic economics suggests compelling reasons this would be a minor factor. In fact, the discussion of the GE model later in this paper outlines why economic theory could predict that returns to capital (and, thus, profits) can be expected to rise in response to an increase in the minimum wage when employment losses are assumed not to occur for the labor receiving this wage.

D. Assumptions on Paying for Minimum Wages in Assessing Distributional Consequences

To depict the circumstances deemed most likely to apply by minimum wage advocates, the analysis below assumes that no employment or profit losses occur as a result of minimum wage increases. Although many economists remain convinced that increases in the minimum wage will decrease employment, the recent literature on this subject has convinced most policy makers that such employment effects are very minimal. While many in the public policy community intimate that minimum wage increases are paid out of firm profits, no reliable evidence supports this position and few minimum wage advocates in the United States cite this position.[5] This leaves price adjustments as the source for paying for minimum wage increases. If all the costs of the minimum wage are passed on to consumers in the form of higher prices, then price increases should reflect the wage increase multiplied by labor's share of the total cost. In order to have no job or profit loss, consumers must continue to purchase the same amount of low-wage goods at the higher price. Thus, our simulations make three related assumptions:

- consumers do not reduce consumption as prices rise,
- all increased labor costs are passed on in higher prices, and
- low-wage workers remain employed at the same number of hours after the minimum wage rises.

Taken together, these three assumptions provide a setting for simulating the expected effects of minimum wage increases in a relatively straightforward manner. One need not believe that all these assumptions hold in reality, preferring instead to believe that firms pay for minimum wage hikes through all possible sources. This simulation environment, however, depicts a world with no job loss, which is the notion popularly maintained by proponents of the minimum wage. The simulation findings provide a basis for understanding the effectiveness of the minimum wage in redistributing resources across the household income distribution.

[5] If minimum wages do reduce profits, then their effects on the income distribution may be more progressive than measured in this study, since stockholders tend to be more wealthy Americans. However, how much more progressive is unclear since many Americans, even ones who are not particularly wealthy, own stock through private and public retirement portfolios.

III. Overview of Methodology and Data

Although the above discussion primarily focuses on payment sources for costs, one must also consider the benefit side of the picture to understand the distributional effects of a minimum wage. The two sides of the simulation analysis—benefits and costs—presented below require different data sets. This section presents an overview of these data and the methodology applied to measure the benefits and costs of an increase in the minimum wage.

A. Description of Data

To calculate the benefits of a minimum wage increase, the analysis relies on data from SIPP, a nationally representative survey of households conducted by the US Census Bureau. To depict circumstances relevant to the 1996 increase in the federal minimum wage, the analysis uses data from waves 1–3 of the 1996 SIPP; the dates covered by these survey waves place them before the 1996 change in the minimum wage. The SIPP data provide information on households, families, and individuals over 15 years of age, including monthly data on income and earnings by source, wages, hours worked, demographic characteristics, family structure, and public-assistance program participation. These data permit identification of low-wage workers, their occupations and industries, their family income, and sufficient information to determine income tax burdens under alternative income scenarios using the National Bureau of Economic Research (NBER) income tax simulator (TAXSIM) program. The following analysis uses SIPP to simulate both the before- and after-tax effects of a minimum wage increase on the earnings and incomes of families with various characteristics.

To translate the effects of price increases induced by a minimum wage on families' costs of consumption, the analysis relies on data from the CES matched to the same time period as SIPP. The CES is a nationally representative survey of households conducted by the US Bureau of Labor Statistics that includes information on family expenditures on a comprehensive and detailed array of goods and services. It also incorporates a number of income measures and demographic characteristics. Although the income and demographic measures in the CES are not as detailed as those in SIPP, both data sets identify comparable categories of families characterized by their position in the income distribution, poverty level, welfare status, and family structure.

To trace the higher earnings of workers affected by the minimum wage to the prices of the products produced by these workers, the analysis uses national input-output data constructed by the IMPLAN Group. These IMPLAN input-output tables summarize databases on employment, value added, output, and product demand for 528 industrial sectors in all states and counties in the United States.[6]

B. *Overview of Methodology*

Figure 1 illustrates the steps that make up the methodology implemented below to simulate the distributional consequences of increases in the minimum wage. In the figure, data sets are listed in a bold font, and the arrows indicate inputs into the next step.

Starting with SIPP data in this figure, the first step calculates the effect of the 1996 increase in the minimum wage on the earnings of affected workers and on their family income, assuming no change in hours worked. Section IV.A describes the precise formulation of these calculations. This information is then used for both the benefit and the cost sides of the computations.

On the benefit side, these SIPP calculations measure how much the income of each individual family in the survey changes as a result of the wage increase. The second step computes the distribution of these benefits across families categorized by their income quintiles, poverty levels, extent of dependence on low-wage earnings, welfare recipient status, and demographic characteristics. To translate benefits into after-tax values, the third step applies the NBER TAXSIM calculator to each family's circumstances to determine how much of these additional benefits (i.e., earnings) are reduced through federal, state, and payroll taxes. This produces the final after-tax benefits for each family. The last step on the benefit side generates the distribution of after-tax benefits for the same family categorizations used for the before-tax distributions. Section IV presents these findings.

On the cost side, computations of the minimum wage increase are far more challenging. Inferring the shares of costs borne by the different categories of families requires two sets of calculations: (i) measures of how much prices rise by commodity in response to the minimum wage increase and (ii) the effects of these price increases on the consumption costs by family given its expenditure composition across commodities.

Computing measures of price impacts requires two steps after the first step described above making up the SIPP calculations measuring how much the labor cost of each individual rises as a result of a minimum wage increase. Using information in SIPP on each low-wage worker's industry of employment, the second step computes the amounts that labor costs rise in each industry. In addition to higher wage costs, employers must also pay higher payroll taxes, primarily in the form of employers'

[6] The IMPLAN data come from data collected by the Bureau of Economic Analysis, the Bureau of Labor Statistics, and the Census Bureau, among other sources.

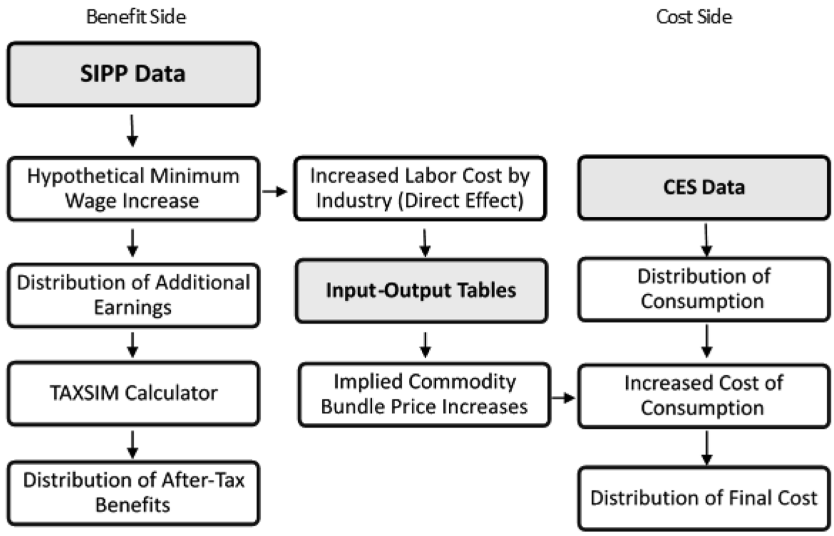

FIG. 1.—Data and methodology overview

contributions to Social Security. Both higher wages and taxes are included in the increased labor costs computed by industry. Then, the third step translates these higher employment costs (i.e., direct costs) into price increases for each final consumer good and service using the IMPLAN input-output tables. This is simply an accounting exercise consistent with the assumption that firms respond to higher labor costs by increasing prices. Sections V.A and V.B present details on the calculation of final price increases.

Computing measures of consumption costs involves two additional steps building on the second and third steps implemented above in calculating price impacts. The fourth step on the cost side uses data from the CES to identify the composition and levels of consumption by different family types for each good and service and translates commodity price increases into consumption cost increases for each family, assuming no change in the family's quantities consumed. The fifth and last step categorizes families in the CES by income quintiles, consumption quintile, poverty status, welfare participation, and other family characteristics and computes the distributions of increased consumption costs across these categories. Section V.C presents these findings.

Finally, to infer the net effects of an increase in minimum wages, Section VI integrates the benefits and cost allocations across and within family types to compute the overall distributions for each category of families. The analysis also calculates the aggregate benefits and cost transfers through a minimum wage increase. Increases in the minimum wage are known to have spillover effects on raising the wages of workers just above the minimum wage, which is ignored in this analysis. While the following calculations do not measure these effects, computations done in supplemental work analogous to those implemented below produce distributional findings fully compatible with those presented in this paper.

IV. Who Benefits from Increases in the Minimum Wage?

This section first shows how to calculate the additional pretax and posttax earnings for each family induced by an increase in the minimum wage and then examines how these additional earnings are distributed across families by a variety of characteristics with emphasis on particular types of families that might be considered the most important targets of minimum wage policy. Finally, the section reviews previous research done on the distribution of benefits.

A. *Calculating Pretax and After-Tax Benefits of Families with Low-Wage Workers*

Family gross earnings and income are raised by the combined increase in earnings of all family members; this change in family earnings is the pretax benefit and is calculated as follows. For each worker in the family identified as earning an hourly wage below the new legally specified minimum wage level in 1996, the analysis assumes that his or her hourly wage rises to the new minimum, that is, from as low as $4.25 (the old minimum) to exactly $5.15 (in 1996 dollars). The computations use the new wage rate and annual number of hours worked to calculate the implied increase in total earnings for each worker during the year assuming that there is no change in hours of work. For workers earning less than the old minimum wage of $4.25, the analysis assumes that they also receive a $0.90 wage increase, which does not bring them up to the full $5.15 per hour. The computations assume no spillover benefits for workers already earning more than the new minimum wage.

For the after-tax benefit, the analysis adjusts the increased income for federal and state income taxes (fully incorporating the net effects of the Earned Income Tax Credit [EITC]) and for payroll taxes using the NBER TAXSIM program. These calculations account for the dependent status of young workers as this plays an important role in determining tax liability.[7] These calculations also assume that all married couples are joint taxpayers. Because of data limitations, all taxpayers are assumed to

[7] In 1996, taxpayers could claim a dependent exemption if they had a dependent under age 18 or had a dependent under age 23 who was a full-time student. The computations

take the standard deduction rather than itemize their deductions, which should have little impact on low-income taxpayers.

B. Distribution of Benefits across Families by Income: Before and After Tax

Using the before- and after-tax benefits calculated for each family in SIPP, one can compute the shares of benefits received by families sorted by a variety of characteristics, including income quintiles, income as a multiple of the poverty level, presence of children, headship and marriage status, wage rate levels, and dependency on public assistance. Table 1 presents the distributions of benefits across different partitions of families.

To highlight the distribution of benefits across family income, panel A of table 1 segments families into five income quintiles and reports the average levels and distribution of benefits (i.e., higher earnings) across these quintiles. For each quintile, column 5 shows the share of families that include one or more minimum wage workers (i.e., those who benefit from the minimum wage increase). The result is perhaps surprising for those unfamiliar with similar findings in the literature. The minimum wage population is almost equally distributed across the income distribution. While 22.3 percent of all families have one or more minimum wage workers, only slightly more (22.6 percent) families in the lowest quintile include low-wage workers and therefore benefit from the minimum wage increase. This is nearly identical to the 22.7 percent of families in the highest income quintile that have a worker who benefits from a minimum wage increase. Thus, approximately one in five families benefit, regardless of their income.

The more relevant question of "Where do the dollars go?" is addressed in columns 2–4 of table 1. If high-income households have low-wage workers who typically work fewer hours than the low-wage workers at the bottom of the distribution (e.g., part-time teenagers as opposed to family breadwinners), then one would expect the additional dollars from the wage increase to flow disproportionately to the poorer families. Column 2 presents the distribution of additional earnings due to the minimum wage increase across the five quintiles. If the benefits were identically distributed across all families, each quintile would receive about 20 percent of the extra earnings and more than its share of the additional earnings if it receives more than 20 percent. This is essentially the story revealed in table 1: benefits are evenly divided across quintiles.

here assume that any child under age 18 who lived at home for some part of the sample period and earned less than $20,000 (in 1996 dollars) was claimed as a dependent by the parent(s). Children under age 23 who reported being enrolled in college were also assumed to be claimed as dependents by the parent(s). The TAXSIM program fully accounts for these factors in its calculations of income taxes and EITC.

The 40 percent of families at the bottom of the income distribution receive only 38.3 percent of the additional earnings from the minimum wage. Conversely, the top 40 percent of families receive 40.3 percent of the extra earnings. The minimum wage increase distributes money to families at all income levels with little preference given to any group.

Since the US tax system is progressive, the distribution of extra earnings changes when calculating the shares of earnings after taxes, as reported in column 3. The poorest families lose less of their extra earnings to taxes: their share drops only 2.2 points from 19.9 percent to 17.7 percent. Those families in the highest income quintile fare worse: their share drops 6 percentage points from 18.6 percent to 12.6 percent. The distributional impact of the tax system is also apparent from comparing the average value of after-tax benefits for families that have a minimum wage worker as reported in column 4 of table 1. Again, low-income families benefit more than high-income families, though not by as much as might have been expected. Through taxation, the government captures about one-quarter of the total benefits from the minimum wage increase.

These calculations ignore the potential loss of cash and in-kind welfare benefits for families under and near the poverty level whose income rises as a result of the minimum wage. The computation of after-tax benefits performed in this analysis includes transfers from the EITC program, but not from such income support programs as Temporary Assistance to Needy Families (TANF), Aid to Families with Dependent Children (AFDC), and food stamps. Accounting for these welfare transfers would strictly worsen the distributional consequences of the minimum wage conveyed by this study.

C. *Benefits to Other Target Families*

While ranking families by income does not take into account family size, poverty levels do. Panel C of table 1 report the shares of minimum wage benefits going to families with income and sizes measured against multiples of the poverty threshold. As shown in the after-tax shares in table 1, 13.4 percent of benefits go to families below the poverty threshold. However, nearly 30 percent of the after-tax benefits go to families with incomes that are more than three times the poverty threshold. Thus, the majority of the additional earnings do not go to poor (or near-poor) families.

Another primary target of the minimum wage consists of families dependent on the earnings from a low-wage worker for a substantial part of total family earnings. Panel D of table 1 lists results for four different specifications of families with children that rely on the earnings of low-wage employees: families for which more than 50 percent of their total earnings comes from employment that pays (i) no more than $5.15 per

TABLE 1
MINIMUM WAGE BENEFITS BY VARIOUS FAMILY TYPES

Family Type	Percent of All Families (1)	Percent of Pretax Benefits (2)	Percent of After-Tax Benefits (3)	Average After-Tax Benefits Families with Minimum Wage Worker ($) (4)	Percent of Families with Minimum Wage Worker (5)
A. Income quintile:					
Lowest income quintile	20.0	19.9	17.7	595	22.6
2nd income quintile	20.0	18.4	13.5	518	19.7
Middle income quintile	20.0	21.4	15.7	525	22.6
4th income quintile	20.0	21.7	15.0	475	24.0
Highest income quintile	20.0	18.6	12.6	421	22.7
B. Taxes:					
Federal income taxes	. . .	. . .	14.6	. . .	. . .
State income taxes	. . .	. . .	3.0	. . .	. . .
Payroll taxes (FICA)	. . .	. . .	7.9	. . .	. . .
C. Poverty level:					
Less than half the poverty threshold	5.3	3.6	3.9	502	22.0
50%–100% of the poverty threshold	8.9	10.7	9.5	603	26.7
1–2 times the poverty threshold	18.4	20.7	17.4	573	25.2
2–3 times the poverty threshold	16.1	17.3	14.0	552	23.9
More than 3 times the poverty threshold	51.2	46.1	29.6	436	20.1

D. Wages: families with children where ≥50% earnings (1996 $) come from:					
Jobs paying at most $5.15/hour	1.7	9.9	8.7	774	99.8
Jobs paying at most $6.00/hour	3.7	14.8	12.5	660	76.7
Jobs paying at most $7.50/hour	7.0	20.1	16.3	588	60.2
Jobs paying at most $10.00/hour	12.5	28.1	22.2	543	49.3
E. Family type:					
Married	48.9	57.3	40.7	488	26.0
Married with children (under 18)	25.6	39.0	28.0	485	34.3
Single	48.8	40.4	31.9	546	18.2
Single with children (under 18)	10.6	14.4	12.3	513	34.2
Families below 2 times poverty with children	11.1	18.5	15.5	563	37.9
Families below poverty with children	5.0	7.4	7.5	599	38.0
Welfare recipient families	18.2	24.8	20.3	549	30.9
Welfare recipients with children	9.5	16.5	13.8	548	40.2
Families with minimum wage worker	22.3	100.0	75.3	511	100.0

NOTE.—This table uses the 1996 SIPP data on all workers aged 15 and over to compute the impact of a $0.90 increase in the 1996 minimum wage, as described in the text. Column 4 reports after-tax benefits in 2010 dollars.

hour, (ii) no more than \$6.00 per hour, (iii) no more than \$7.50 per hour, and (iv) no more than \$10.00 per hour. Not surprisingly, table 1 shows that these target families receive larger after-tax benefits on average and receive a disproportionate share of minimum wage benefits. For example, families in the third category receive 20 percent of all minimum wage benefits, even though they make up only 7 percent of all families. However, even when the low-wage threshold is expanded to include wages as high as \$10.00 per hour, only 22 percent of total after-tax minimum wage benefits go to these target families.

Panel E of table 1 presents projected allocations for married and single families, distinguishing those with children. In general, families with children receive more benefits than those without. Families with children below twice the poverty level receive only 15.5 percent of the total after-tax minimum wage benefits. Table 1 also gives results for families who received welfare at some time during the year. With welfare interpreted as public cash aid and/or food stamps, welfare recipient families with children account for 9.5 percent of families, and they are projected to receive 13.8 percent of the after-tax additional earnings generated by a minimum wage increase.

D. Previous Research on the Distribution of Benefits

This assessment of the distribution of benefits mostly replicates early work by Gramlich (1976), Johnson and Browning (1983), Burkhauser and Finegan (1989), Horrigan and Mincy (1993), and Burkhauser and Sabia (2007). These studies also document that many low-wage workers are members of high-income families. This is especially true for teenagers who are distributed throughout the entire family income distribution and often find employment in minimum wage jobs. This literature consistently shows that while the minimum wage has a small effect on earnings inequality, it has virtually no effect on income inequality.[8] Johnson and Browning (1983) and Horrigan and Mincy (1993) focus on the distribution of minimum wage benefits by family income quintile and show that the additional minimum wage earnings are only mildly redistributive, with somewhat larger benefits going to families in the second to lowest income quintile. Burkhauser and Finegan (1989) and Burkhauser et al. (2000) focus on the distribution of benefits by families' income measured as multiples of the poverty threshold. They find that the distribution of

[8] Several sets of results in table 1 are not elsewhere in the literature: most important, benefits going to families who depend on low-wage employment for more than half of total family earnings and to families who participate in a welfare program. The findings for these groups, however, fit with the well-established conclusion of this literature: the minimum wage represents a very blunt policy instrument for providing benefits to low-income families.

benefits is not significantly different from the population shares. Burkhauser and Finegan (1989), for example, find that only 18 percent of workers who benefit from a minimum wage increase had a family income that was below the poverty threshold. Burkhauser et al. (2000) find that only 13 percent of affected workers were in poverty. Card and Krueger (1995) report similar results, as do Burkhauser and Sabia (2007), who report benefit shares not only on the distribution of minimum wage benefits by family income quintile but also for near-poor families defined by poverty levels.

E. Summary: Distribution of Benefits

Minimum wage policy offers an inefficient mechanism for boosting the incomes of families that policy makers typically think of as the intended beneficiaries of minimum wage increases: poor families, those supported primarily by low-wage work, and those on welfare. About 35 percent of the total increase in after-tax benefits goes to families with income less than two times the poverty threshold, a common definition of the working poor or near-poor; nearly 13 percent goes to families principally supported by low-wage workers defined as earning wages at or below 117 percent (= $6.00/$5.15) of the new 1996 minimum wage; and only about 14 percent goes to families with children on welfare.

Unlike most public income support programs, increased earnings from the minimum wage are taxable. Over 25 percent of the increased earnings are collected back as income and payroll taxes, including the net effect of EITC, which subsidizes low-earning families. Even after taxes, 27.6 percent of increased earnings go to families in the top 40 percent of the income distribution.

V. Who Pays for Increases in the Minimum Wage?

If employment and profits are unaffected, then the cost of the minimum wage increase is covered through higher prices. As prices rise on the goods and services produced by low-wage workers, all consumers of these products are essentially subsidizing the low-wage workers. The following discussion shows that prices rise on a wide variety of goods, imposing across-the-board price increases that hit all consumers.

To assess the distributional impacts of these price increases, Section V.A relies on national input-output tables to calculate how much individual product prices must rise to cover the new labor costs induced by the minimum wage increase, and Section V.B summarizes the findings produced by this analysis. From the employer's perspective, the increase in labor costs will be greater than the increase in earnings since employers will also have to pay higher payroll tax contributions. These price calculations assume a national market with the new prices imposed on all consumers. The analysis then translates these price increases into total consumption cost by family, and Section V.C describes the allocation of these consumption costs across families broken down by their income and demographic characteristics.

A. Attributing Labor Costs to Price Increases

The first step in determining who pays for the minimum wage hike involves calculating the impact of the increased labor costs on the total cost of final goods and services. The following analysis assumes that, if the cost of labor increases in a particular industry, then the price of that industry's output will rise to increase consumer expenditures by the same amount. There are two ways for the total cost of goods to increase after a minimum wage increase. First, there is the direct effect on the cost of labor for industries hiring low-wage workers. Second, there is the indirect effect through intermediate goods. While a portion of an industry's output is consumed by final users (e.g., households and government), the rest of the output is allocated to intermediate use, where the output of the original industry becomes an input for another. Thus, even if an industry employs no minimum wage workers, the prices for that industry's output may rise because the industry uses goods or contracts for services produced with minimum wage labor. This feedback through intermediate uses continues ad infinitum, so the price shock from the wage hike propagates throughout the economy.

The calculations begin by determining the industries that employ low-wage workers. From the SIPP, one can identify all industries that employed workers at wages below the new minimum of $5.15. Considering all low-wage workers in a given industry, one can infer the total increase in industry labor costs resulting from the wage hike, including additional employer contributions for Social Security. Denote these increases by the vector x_0.

The next step is to translate these cost changes into price increases of final goods. The input-output tables provide information to construct the square matrix B, where the (i, j) element of this matrix represents the share of commodity j produced by industry i. In this representation of the economy, the vector $y_0 = B'x_0$ specifies the initial increase in costs to produce each commodity or commodity bundle,[9] where elements of the vector x_0 measure the increases in labor costs for each industry attributable to the minimum wage hike. To account for the phenomenon that many commodities are used as inputs in the production of other com-

[9] Commodity bundles are given broad definitions such as food inside the home, food outside the home, rent or home ownership costs, automobile expenditures, etc.

modities, input-output tables specify a square matrix U, where the (i, j) element of this matrix represents the proportion of commodity i's output used by industry j. Given these specifications, the vector $y_1 = (I + B'U')B'x_0$ constitutes first-round carryover cost incorporating the price increases of intermediate goods. After a sufficiently large number of iterations, the long-run vector of costs arising from the initial increase in labor expenses equals $y_\infty = (I - B'U')^{-1}B'x_0$. To allocate these increased costs into the final uses of production, input-output frameworks provide data to construct diagonal matrices F_k with diagonal elements f_{ki} designating the fraction of commodity i's total production that goes to final use k, where $k = 1, \ldots, 5$ identifies one of the following five categories of final use: households, gross investment, government, inventories, and exports and imports.[10] When results are combined, the amount of increased costs passed on to final-use category k is $F_k y_\infty$. Finally, to close the system, one must allocate final-use costs for gross investment and inventories to consumption goods in order not to lose their higher costs in the computations of price increases. In the case of gross investment, this computational analysis treats investment as a form of intermediate goods and allocates their costs in proportion to each industry's use of capital as reported by the Bureau of Economic Analysis 1992 Capital Flow Table.[11] The analysis treats residential investment as a final consumption good. In the case of inventories, the analysis allocates costs proportionally to the two domestic final users: households and government.

Given these computations, the analysis is now parallel to the starting point on the benefits side. The CES specifies the levels of goods and services consumed by each family. To calculate price effects, one must bundle these products into industries and commodities consistent with the input-output tables. For example, the commodities grocery stores, dairy product stores, retail bakeries, and food stores are mapped into the goods expenditure category "food inside the home." Given these mappings, one can add up the price increases calculated above across bundles to compute the increased expenditures required for a family to maintain its original level of consumption after the price increases implied by the minimum wage increase.

As with the benefit side, analyzing costs at the family level relates expenditure increases to family characteristics. In particular, one can measure the additional consumption costs allocated to families according to their income and consumption quintile, income relative to the poverty

[10] The IMPLAN input-output tables have 10 final-use sectors, which this analysis aggregates into the consumption groups considered in this paper.

[11] The Bureau of Economic Analysis investment data by using industry are available online at http://www.bea.gov/industry/capflow_data.htm. These 1992 data are closest to year 1996, which is analyzed in this study.

level, welfare status, marriage status, classification as female headship, and the presence of children.

B. *Price Increases from Increased Labor Costs*

While the computations below account for all goods and services, one can better understand the cost of the minimum wage on prices by considering the effect on a subset of heavily affected industries. Table 2 lists the 23 industries with the largest number of minimum wage workers. These 23 most heavily affected industries account for 75 percent of all minimum wage jobs. Column 1 presents the percentage of all workers employed in the designated industry benefiting from the 1996 increase of $0.90 in the federal minimum wage. Column 2 gives the per-

TABLE 2
Minimum Wage Jobs and Cost Increase by Industry

Industry	Percent All Minimum Wage Jobs (1)	Percent All Minimum Wage Hours (2)	Percent Direct Costs (3)	Percent Final Costs (4)
Eating and drinking places	20.97	18.45	18.67	19.83
Other retail trade	6.36	5.60	5.02	5.20
Grocery stores	6.31	5.24	4.49	4.58
Elementary and secondary schools	4.07	4.20	5.00	5.50
Household miscellaneous personal services	3.66	3.35	3.98	4.24
Government	2.96	3.42	4.19	1.43
Colleges and universities	2.89	2.29	2.63	2.87
Miscellaneous entertainment and recreation	2.86	2.26	2.15	2.42
Department stores	2.69	2.31	1.78	1.97
Construction	2.52	3.00	2.94	2.63
Hotels and motels	2.22	2.27	2.03	1.01
Wholesale goods	2.02	2.47	2.37	1.44
Child day care services	1.68	1.54	1.52	1.75
Apparel and accessories	1.58	1.95	2.05	2.18
Agricultural production crops	1.55	1.92	2.15	.81
Motor vehicle dealers	1.51	2.03	1.99	2.39
Movies and videos	1.37	1.02	.93	.49
Real estate	1.27	1.67	1.96	4.82
Health services	1.24	1.22	1.14	1.51
Trucking and warehousing	1.23	1.96	2.23	.74
Apparel and accessory stores	1.21	.89	.76	.88
Nursing and personal care facilities	1.18	1.15	.86	1.17
Religious organizations	1.16	1.22	1.45	1.69

Note.—The 1996 SIPP data on all workers aged 15 and over are used in cols. 1 and 2 to determine the industry of workers who benefit from the $0.90 increase in the 1996 minimum wage, as described in the text. The IMPLAN input-output tables are used in combination with the SIPP data in cols. 3 and 4 to calculate the direct and final costs as described in the text.

centage of all hours worked by employees benefiting from the minimum wage increase. Column 3 reports the percentage of total direct labor cost increases by industry, and column 4 lists the percentage of total final costs (which includes the increased cost of intermediate goods).

For a number of consumption goods, the final cost increase is lower (in dollar value, not just percentage) than the direct increase in labor costs. This can occur when the final users of the outputs live outside the United States. In these instances, the United States exports some of the costs of the wage increase. Alternatively, the costs may be redirected to government expenditures (which are not tracked). Final costs can also be larger than direct costs when the industry uses as inputs the output from other industries employing low-wage workers. For example, a large part of the construction industry involves building residential homes, which then become an input to the real estate industry that sells the homes; thus, much of the direct costs to the construction industry show up in the real estate industry's final costs.

Table 3 reports the share of the total national cost increase accounted for by commodities grouped into broad consumption categories in column 1. Prices increased for a very long list of goods purchased by families. As expected, food outside the home accounts for the largest share of additional costs since eating and drinking establishments make up the industry most affected by the increased labor costs.

The magnitude of the final price increase depends on the size of the labor cost increase relative to the industry's overall costs of production. For each good, dividing the additional costs by the total expenditures yields a percentage cost increase. The discussion below refers to these price increases as "implicit incremental tax rates" on household consumption goods. Essentially, these tax rates identify the amount by which consumer prices must increase to cover the total costs added by the minimum wage hike.

Table 3 presents these incremental price increases by broad commodity bundles in column 2. These price increases may at first appear relatively small; one of the largest rates is only 1.85 percent for food outside the home. However, a 0.0185 tax rate increase is large when compared to common state-level sales tax rates. The largest incremental price increases occur for education and social services, moving and storage, miscellaneous personal services such as beauty and barber shops, and food outside the home. It is worth noting that, although these price increases appear small enough to justify the assumption that consumption levels do not change, most families facing these higher prices do not receive additional earnings, so the higher prices will require either a reduction in consumption in nonaffected goods or a reduction in savings.

TABLE 3
Minimum Wage Jobs and Cost Increase by Industry

Commodity Bundle (Industry)	Share of Increased Cost Accounted for by Commodity (%) (1)	Implicit Incremental Tax Rate on Commodity (2)
Food: outside home	21.04	.0185
Education and social services	11.06	.0280
Food: inside home	9.56	.0034
Other: general trade	9.06	.0005
Other: personal consumption	7.80	.0004
Health care and insurance	7.72	.0004
Household: personal services	6.21	.0200
Housing: rent	5.15	.0025
Entertainment and recreation	3.87	.0097
Household: clothing	3.44	.0035
Transportation: car	3.20	.0012
Household: utilities	2.57	.0018
Banking and financial services	2.41	.0029
Household: child care	1.85	.0100
Transportation: auto service	1.51	.0030
Housing: hotels	.95	.0053
Household: furniture	.79	.0027
Household: moving and storage	.65	.0235
Household: laundry and cleanings	.32	.0034
Transportation: air travel	.32	.0016
Household: legal services	.26	.0029
Household: computers and office supplies	.15	.0010
Household: landscape services	.12	.0013
Household: appliance repair	.02	.0012

Note.—The 1996 SIPP data and the IMPLAN input-output tables are used in combination to calculate the final cost by commodity, as described in the text.

The price increases reported in table 3 are well within the range found elsewhere in the literature. As reviewed briefly in Section II, the estimated elasticities for responses in prices to increases in the minimum wage fall between 0.04 and 0.4. The computations in this paper consider a 21.2 percent increase in the minimum wage from \$4.25 to \$5.15. This implies that price increases should be between 0.0085 and 0.085 on average. As shown in column 2 of table 3, the implicit tax rates found in this paper are, on average, in the lower part of this range.

C. *Distribution of Costs across Families*

The costs paid by each family for the 1996 increase in the minimum wage are determined by applying the implicit tax rates in table 3 to the data on individual consumption goods and services reported in the CES for each family. As with the benefit side, one can further aggregate these costs by family characteristics including income quintile, income relative to the poverty level, and family structure.[12] Additionally, one can also aggregate costs for families by consumption quintile.

Table 4 reports the percentage of minimum wage costs borne by those in the specified quintile or family type in column 2 and the average annual cost in column 3. On average, families pay $136 (in 2010 dollars) more per year for their purchases to pay for the 1996 increase in the minimum wage. The amount a particular family pays depends on its level of consumer expenditures, which typically varies by income. These costs range from $74 annually for families in the lowest category to $250 for the richest families. Families in the highest income quintile pay 31.7 percent of the costs of the minimum wage, whereas the poorest 20 percent pay only 9.3 percent of the costs. Families living in poverty pay only 8.3 percent of the costs, compared to the 51 percent of costs paid by families with incomes greater than three times the poverty threshold.

Unsurprisingly, the costs of the minimum wage increase are more correlated with consumption than with income. According to table 4, families in the lowest consumption quintile bear only 5.3 percent of the cost while those in the highest consumption quintile bear 37.6 percent, though, as seen in column 4, the cost is a larger percentage of annual expenditure for families in the lowest consumption quintile compared to those in the highest consumption quintile. This indicates that families with lower levels of consumption disproportionately purchase the goods produced with the larger shares of minimum wage labor.

D. *Summary: Cost Incidence of Minimum Wage Is More Regressive than Sales Tax*

One of the realities of minimum wage policy is that families are unlikely to associate these minor price increases directly with the wage increase. Imagine, however, a value-added or sales tax that had the identical effect. That is, instead of increasing wages, the government could impose a value-added tax on specific products and distribute the proceeds from the tax to supplement the earnings of low-wage workers. Of course, no such tax is being considered, but it is useful to consider the price effects in this context.

Given this "value-added tax" interpretation of the price increases, the implicit tax rates reported in table 3 needed to pay for the 1996 hike in the minimum wage for the most affected commodity groups fall in the range 0.04–2.8 percent. The consequences of these differential tax rates across commodities on the total cost of a family's consumption depend

[12] No doubt the broad industry categories applied in this analysis may mask some of the regressivity in calculated price increases. Poor people shop at Wal-Mart and eat at McDonald's, while the rich are more likely to eat and shop in places where few or no workers earn the minimum wage.

TABLE 4
Minimum Wage Costs Paid by Various Family Types

Consumer Group	Percent All Families (1)	Percent Minimum Wage Costs (2)	Average Annual Cost per Family ($) (3)	Cost as Percentage of Annual Family Expenditure (4)
A. Income quintile:				
Lowest income quintile	20.0	9.3	74	.59
2nd income quintile	20.0	10.9	86	.50
Middle income quintile	20.0	14.4	114	.51
4th income quintile	20.0	19.5	154	.54
Highest income quintile	20.0	31.7	250	.58
B. Consumption quintile:				
Lowest consumption quintile	20.0	5.3	42	.63
Mid-low consumption quintile	20.0	9.0	71	.56
Middle consumption quintile	20.0	13.3	105	.56
Mid-high consumption quintile	20.0	20.6	163	.57
Highest consumption quintile	20.0	37.6	297	.52
C. Consumption sectors:				
All families (domestic)	100.0	85.9	136	.54
Federal, state, and local government	. . .	7.6	. . .	. . .
Foreign consumers	. . .	6.7	. . .	. . .
D. Poverty level:				
Less than half the poverty threshold	6.3	3.4	85	.63
50%–100% of the poverty threshold	9.9	4.9	78	.54
1–2 times the poverty threshold	23.3	12.9	88	.51
2–3 times the poverty threshold	18.6	13.7	116	.51
More than 3 times the poverty threshold	41.9	51.0	193	.56
E. Family type:				
Married	52.3	55.7	169	.54
Married with children under 18	24.2	27.4	180	.54
Single	47.7	30.0	100	.56
Single with children under 18	8.5	5.9	111	.53
All families with children under 18	32.6	33.3	162	.54
Families below 2 times poverty with children	12.9	8.2	101	.49
Families below poverty with children	5.3	2.8	84	.47
Welfare recipient families	9.8	4.4	71	.46
Welfare recipients with children	4.6	2.1	74	.46

Note.—This table relies on the Consumer Expenditure Survey to calculate family consumption of goods for which there was a minimum wage–induced price increase. Differences between this table and table 1 with respect to the characterization of families are due to differences between the CES and SIPP data. Column 3 reports average annual cost in 2010 dollars.

on the degree to which the family purchases the commodities apportioned the higher rates. Column 4 of table 4 shows the combined impact of these implicit tax rates given the consumption patterns of families grouped by various family characteristics. One sees from these results that the poorest families typically pay the higher aggregated rates. Rates decrease monotonically from 0.63 percent for families in the lowest consumption quintile to 0.52 percent in the highest. Rates are larger for the lowest income quintile than for the highest and even larger than for the middle quintiles. The same pattern holds for families with income measured compared to the poverty level. Welfare recipients are the only lower-income group who incur lower implicit tax rates on consumption than the average incurred for all families.

State sales taxes often specifically exclude goods that are considered necessities, such as health care, housing, and food purchases. The aim of excluding these goods is to lessen the regressivity of the sale tax since low-income families purchase a disproportionately larger share of these goods in their overall spending. Interpreted as a sales tax, the minimum wage price increases do exactly the opposite. Prices tend to go up most on those goods that make up a larger fraction of consumption for the poor. So, although the rich pay more in terms of dollars, a "minimum wage tax" is more regressive than a typical sales tax.

VI. Net Effects of Minimum Wage Increases

The policy question posed in the introduction rests on the effectiveness of the minimum wage in targeting resources to poor families, where effective targeting means that benefits accrue disproportionately to low-income families and the costs fall disproportionately on high-income families. The previous two sections separately examined the benefits and the costs of the minimum wage for different categories of families, assuming that all costs are passed through as higher prices. Section VI.A now brings these two sides together to explore the net effects across different groups of families to assess how well a minimum wage increase targets resources to the poor. Section VI.B summarizes the aggregate costs and benefits for US workers, consumers, and taxpayers.

A. Net Distributional Effects by Family Characteristics

According to results from the previous sections, families paid $136 annually, on average, in higher consumption costs to fund the 1996 increase of $0.90 in the federal minimum wage and families received $114, on average, annually in benefits through higher earnings. The cost is larger than the benefit, on average, primarily because of taxation; the cost to employers including payroll taxation exceeds the after-tax benefit to consumers.

Although the data from SIPP and CES are not fully compatible, integrating information in tables 1 and 4 by matching the quintile estimates for benefits and costs provides evidence of the net distributional effects of the minimum wage increase. Two kinds of families make up each income group: those with low-wage workers and those without.

These two kinds of families provide the basis for understanding the effect of a minimum wage law on the income distribution since not all families benefit but all families pay higher prices. The average annual cost listed in table 4 is the costs that all families pay as a result of the rise in prices. The benefits listed in table 1 go only to families with a minimum wage worker.

Table 5 integrates the findings of tables 1 and 4 to depict the circumstances of families within each income quintile and of the population at large. Column 3 reports the net benefits to families with a minimum wage worker, and column 4 presents the net benefits to families without a minimum wage worker. Because families without a minimum wage worker receive no benefits, column 4 comes directly from the average annual cost given in column 3 of table 4. The final column of table 5 reports the net benefit for all families in the income quintile (a weighted average of cols. 3 and 4, where cols. 1 and 2 are the weights).

Table 5 reveals a large amount of income redistribution between families within the bottom income quintile.[13] While the 22.6 percent of families in the bottom income quintile with a minimum wage worker gain $521 on average, the 77.5 percent of families without a minimum wage worker lose $74 on average. Thus, the minimum wage increase is equivalent to taking $74 from 3.4 poor families, for a total of $252, and then giving this amount plus an additional $269 from nonpoor families to one poor family with a minimum wage worker. Nearly half the total income redistribution to families with minimum wage workers in the lowest income quintile comes from other poor families. Looking at column 5, it is clear that there is redistribution from wealthy families to poorer families, though there are large differences between families with and without a minimum wage worker within each income quintile.[14]

As one moves up the income distribution, the costs begin to outweigh the benefits, so that the average family in the highest income quintile pays $154 more in costs than it receives in benefits. However, high-income families with a minimum wage worker still averaged more in additional earnings than they paid in higher prices. Averaging across all

[13] The benefits and costs calculated throughout this analysis represent only a snapshot of families in a year and fail to recognize that the presence of minimum wage workers in and the income quintiles of families invariably shifts over time, potentially by large amounts. Thus, when viewed in a life cycle context, a far greater portion of families will benefit by having a member who is a minimum wage worker than is portrayed in table 5. At the same time, the share of benefits going to these families over a longer horizon will be smaller than depicted in the table. Similar circumstances could, of course, arise in consumption patterns. An interesting research task would be to follow households over longer periods, but this would require data beyond those used in this study.

[14] No standard errors associated with either estimation error or data quality appear in table 5 or in any other table. The computational approach implemented in this study corresponds to familiar calibration methods applied throughout economics, and the measured impacts presented here should be interpreted accordingly.

TABLE 5
NET EFFECTS OF THE MINIMUM WAGE INCREASE BY INCOME QUINTILE

	SHARE OF FAMILIES		AVERAGE NET BENEFIT/COST FOR FAMILIES ($)		
INCOME QUINTILE	With a Minimum Wage Worker (1)	Without a Minimum Wage Worker (2)	With a Minimum Wage Worker (3)	Without a Minimum Wage Worker (4)	All Families (5)
Lowest income quintile	22.4	77.5	521	−74	60
2nd income quintile	19.9	80.1	427	−86	16
Middle income quintile	22.5	77.5	412	−114	5
4th income quintile	24.1	75.9	318	−154	−40
Highest income quintile	22.5	77.5	172	−250	−154
All families	22.3	77.7	370	−136	−23

NOTE.—This table relies on SIPP and CES together with the IMPLAN input-output data to perform the calculations. Columns 1 and 2 come directly from table 1. Columns 3–5 depend on both SIPP and CES data, but the income quintiles come from the CES data. All dollar values are inflation adjusted to 2010 dollars using the Consumer Price Index for All Urban Consumers.

families yields a negative net effect since 25.5 percent of benefits go to taxes.

B. *Aggregate Costs and Benefits*

In considering the benefits and costs, the previous discussion primarily concentrates on the individual effects for different types of families. However, it is helpful to know the total magnitude and distribution of the minimum wage increase among workers, taxpayers, and consumers. Nationwide, the above analysis predicts that the 1996 wage law resulted in higher annual expenditures of $15 billion in 2010 dollars. The cost of this minimum wage increase is nearly half the amount spent in 1996 by the federal government on the EITC program, on the AFDC/TANF program, or on the food stamp program.

Panel A of table 6 summarizes the allocation of these total benefits across different economic groups. From the national minimum wage increase, low-wage workers receive $14 billion annually in higher gross earnings but only $10 billion in higher after-tax income. The remainder goes to income and payroll taxes.

Panel B of table 6 presents the cost side of the ledger, with costs split among taxpayers and consumers, both inside and outside the United States (because of exports). US consumers pay nearly $13 billion an-

TABLE 6
Allocation of Projected Aggregate Benefits and Costs (2010 $ Millions)

	Allocation	Amount ($)
	A. Aggregate Benefits	
All low-wage workers and taxpayers	Total increase in earnings and tax payments	15,079
Minimum wage workers	Increase in employees' after-tax earnings	10,548
	Increase in employees' gross earnings	14,007
Taxpayers	Total payroll and income tax gains from increased low-wage earnings	4,531
	B. Aggregate Costs	
All consumers and taxpayers	Total increase in expenditures on goods and services produced by low-wage labor	15,079
US consumers	Increase in spending on consumer goods	12,920
Consumers outside United States	Increase in spending on consumer goods	1,016
US taxpayers	Increase in federal, state, and local government expenditures	1,143

Note.—The table uses the SIPP and the CES together with the IMPLAN input-output data to perform the calculations. All dollar values are inflation adjusted to 2010 dollars using the Consumer Price Index for All Urban Consumers.

nually through higher prices, and consumers outside the United States and US taxpayers roughly equally split covering the $15 billion cost of the minimum wage increase. On net, the aggregate cost for domestic consumers exceeds the increase in after-tax earnings by more than $2 billion. This net loss shows up in table 5 as the negative per-family net benefit listed in the last row and column.

VII. Projecting Impacts of Economic Factors on Distributional Effects

The measurement approach implemented above constitutes a simple accounting structure that ignores the potential counterbalancing impacts of economic forces, which raises concerns about the validity of the estimates since such behavioral factors will surely activate to prevent violation of budget constraints. Economic models in the empirical minimum wage literature do not offer an adequate framework for assessing how such behavioral elements might change the above distributional findings because these models focus on labor markets alone in partial equilibrium settings.[15] To create a flexible framework for evaluating the possible impacts of behavioral factors, the Appendix formulates a gen-

[15] For a review of economic models in the minimum wage literature, see Brown et al. (1982).

eral equilibrium model that incorporates the essential economic elements needed to understand the limitations of the empirical findings in this study.

GE models incorporating minimum wages can be found in the existing literature, but their features make them unsuitable for this analysis. A series of studies in the international trade literature, spawned by Johnson (1969) and Brecher (1974, 1980), construct GE models adapting the familiar Edgeworth-Bowley and Heckscher-Ohlin frameworks to investigate the impacts of minimum wages. A critical drawback of these frameworks relates to their dependence on fixed endowments of labor and capital inputs, implying the absence of any input supply responses. Moreover, these models mostly consider only a single type of labor and household,[16] and their key results primarily rest on assumptions about international trade.

The GE model developed in the Appendix consists of a two-commodity economy with three factors of production: low-wage labor, high-wage labor, and capital. The key feature is that only one of the commodities is produced by low-wage labor. A "low-wage" commodity is produced by all three factors of production, and a second "high-wage" good is produced without any low-wage labor. Three types of households make up the economy: low-wage households, high-wage households, and nonworking households. High-wage households own capital, but the key results do not critically rely on this assumption. To complete compatibility with the empirical framework used above, the model also includes both foreign and government sectors, with both sectors consuming both commodities along with all types of households. Taxes on labor income fund government. Finally, a fixed-coefficient production function makes up the production technology, which is consistent with the input-output analysis utilized above.

The following discussion considers three formulations of this GE model to interpret and qualify the empirical findings presented above. The first specification fully justifies the calculations performed in the above accounting exercise, making them entirely consistent with a particular variant of a market economy. The second specification allows for flexible elasticities in the supplies of factor inputs in response to the cost increases resulting from a rise in the minimum wage. The third formulation briefly explores how relaxing the key behavioral assumptions needed to produce no employment effects for minimum wage workers could influence estimates of distributional impacts.

[16] As an exception, Flug and Galor (1986) introduce skilled and unskilled labor without capital. This study still maintains the assumption of fixed labor supplies in the short run, and it focuses on analyzing the long-run influence of a minimum wage on encouraging skill acquisition through human capital accumulation.

A. *Economic Specification Supporting Simple Accounting Calculations*

To impose the popular belief of no employment effects induced by increases in the minimum wage, the first formulation of the GE model in the Appendix assumes that all consumer groups (i.e., low-wage households, high-wage households, nonworking households, foreign households, and government) have perfectly inelastic demands for the good produced by low-wage labor. This specification further imposes the commonly held belief that high-wage workers are unresponsive to changes in their after-tax wages.

This GE specification directly predicts the distributional numbers presented above. In response to an increase in the minimum wage (i.e., the wage of low-wage workers), low-wage households increase their consumption of high-wage goods to the same extent that other consumer groups jointly reduce theirs. The degree of increase in consumption by low-wage households depends on the magnitude of their hours worked compared to the amounts they consume of low-wage goods, with increases being larger the lower the share of low-wage goods consumed by minimum wage households.

Tax revenues do indeed rise in this specification paid entirely by minimum wage workers through their higher earnings. Because of the perfect inelasticities assumed in the model, all households without low-wage workers decrease their consumption of high-wage goods to cover the higher taxes and after-tax earnings of low-wage workers. The input-output framework applied above allocates government resources to the direct purchase of goods (e.g., supplies and services used by government) according to historical purchase patterns and does not explicitly recognize government income transfers. One can, however, conceptually entertain having the government instead transfer added resources to various consumer groups and have them undertake the consumption.[17] Assuming that policy makers have the sole goal of undoing the adverse distributional effects of a minimum wage increase, an interesting question becomes whether the government has sufficient incremental resources and inclination to compensate the lowest-income groups for their losses.

To explore the viability of such income transfer policy options, table 6 predicts that the government receives \$4.5 billion in additional tax revenues and must spend \$1.1 billion in higher costs on low-wage goods to maintain its original demands. This leaves \$3.4 billion to be spent on high-wage goods or to be transferred to households. Consider having the government transfer these net resources to those households without minimum wage workers that reduced their consumption in response

[17] To be fully consistent with computations performed in the previous analysis, consumer groups would need to undertake purchases in the same composition as assumed for government in the IMPLAN input-output model.

to higher prices, with the lowest-income households receiving priority in the transfers. To assess how far the government could conceptually make up the consumption losses of the lowest income groups, one can calculate the net aggregate losses of each income quintile using the results in table 5 and the numbers of households in each category. Converting the averages and shares reported in this table to group totals,[18] households without a minimum wage worker in the lowest income quintile suffered an aggregate net cost of $1.1 billion due to the price increases induced by raising the minimum wage, the second-lowest quintile without a minimum wage worker incurred $1.3 billion in aggregate losses, and the middle quintile suffered $1.7 billion in aggregate losses. Thus, through transfers, the government could conceptually cover the losses of the lowest-income households without minimum wage workers up to about the median income.

The idea of using the extra tax revenues implied by this specialized specification of the GE model as a governmental transfer to mitigate the adverse distributional consequences of a minimum wage increase has not been considered elsewhere in the literature to my knowledge; nor has it ever been a part of minimum wage legislation. Operationalizing such a policy dictates that government would need to allocate a significant share of the incremental tax resources to transfers to the poorest families without minimum wage workers; moreover, this allocation would need to be cash transfers appropriate for compensating the relevant disadvantaged families, such as Social Security for the elderly, unemployment insurance and welfare for the nonworking poor, and income support (e.g., food stamps and EITC) for the working poor. The determination of these transfers would be exceedingly complex, and government has not shown itself to be especially capable of earmarking sources of tax revenues to spending priorities even when they are simple and directly mandated by law (such as Social Security taxes for only pensions and gas taxes for only highways).

B. *Incorporating Supply Responses in Factor Inputs*

The Appendix next considers what happens in the GE model when the elasticities of the supply of labor and capital are made flexible, allowing for complete responses to changes in economic circumstances. The model still assumes perfectly inelastic demands for the good produced by low-wage labor for all consumer groups. This GE formulation implies that high-wage workers increase their labor supply in response to the price increases resulting from a rise in the minimum wage. They do so to mit-

[18] The total number of households represented by the 1996 data used in the above empirical analysis is 95.5 million, with about 19.1 million making up each quintile.

igate fully reducing consumption of the high-wage good to pay for the increase in prices of the minimum wage good. Consumption of the high-wage good decreases for high-wage households, but less than otherwise would be the case if their labor supply were completely unresponsive to changes in after-tax wage rates. Consequently, the amount of tax revenue obtained by the government will rise further, leaving more room for the government to potentially compensate low-income households for some of their losses assuming this were deemed the priority of the transfer of extra revenues.

Contrary to a popular notion that costs for increasing the minimum wage come out of profits, the GE model indicates that profits will rise in response to the increase if the model incorporates a positive sloping supply function for capital inputs. In particular, the GE model shows that the returns to capital must rise to provide for the expansion in production of high-wage goods induced by the increase in the labor supply of high-wage households. This increased capital cost leads to higher prices of all goods, including those produced by minimum wage workers. This lowers the amounts of the high-wage goods that can be consumed by all households—recall that consumption of the low-wage good is constant—which worsens the welfare of consumers. A household's net position will depend on its extent of capital ownership and its composition of consumption of the capital-intensive goods. Presuming that low-income households are likely to be minor owners of capital, they will be made worse off with a flexible capital supply and more government transfers would be required to compensate them for a minimum wage increase.

C. GE Specifications Implying Employment Effects for Unskilled Workers

Relaxing the perfectly inelastic restriction on the demand for goods produced by minimum wage labor can be expected to induce a decline in the quantities of these goods in response to an increase in the minimum wage, though the GE model formally implies ambiguous effects. The GE model predicts that the consumption of low-wage goods declines for all consumer groups without minimum wage workers;[19] and for low-wage households, consumption can conceptually go either way depending on the relative elasticities of their preferences for hours of work versus the good produced by these hours and their shares in the consumption and production of this good. The overall outcome in the GE model depends on the sizes of these net effects and the share of low-wage households in the economy. Unless low-wage households entirely make up for the declines in the demands by other groups, which is unlikely since only about

[19] This ignores possible increases from households owning large amounts of capital, which could experience increases if the price of capital rises sufficiently in response to a heightened minimum wage.

one in four households have a minimum wage worker, the consumption of low-wage goods will decline overall according to the GE model. Correspondingly, this decline in demand translates into a loss of employment for minimum wage workers since the fixed-coefficient production technology dictates a proportional decrease in the hours worked by low-wage households.

While such employment losses reduce the total benefits received by low-wage households attributable to a minimum wage increase, the distributional impacts depend on how employment reductions occur across these households. In particular, if job losses principally take place among minimum wage workers from high-income families (e.g., teenagers, secondary workers), then the employment effects would enhance rather than diminish the transfer of income from the rich to the poor. Somewhat paradoxically, then, such employment losses would improve the antipoverty properties of minimum wage policy.

Alternatively, employment losses could function against low-income families and worsen the redistribution effects even more than portrayed above. Within the low-wage group, higher-skilled workers are more likely to remain employed (or to be drawn into the labor force) while lower-skilled workers would have a lower probability of employment. The issue becomes whether higher-skilled workers reside in low- or high-income families. If teenagers, students, and supplementary workers from the higher-income families are the higher skilled,[20] then employment losses go disproportionately against low-income families and further would hinder the redistribution effectiveness of the minimum wage depicted above.

Another source of employment losses for minimum wage workers would arise in the GE model if the fixed-coefficient production technology were abandoned and factor inputs could be substituted for one another at some flexible rate. Even with perfectly inelastic demands for goods produced by low-wage labor, a rise in the minimum wage would induce substitution of other factors of production for low-wage labor, resulting in reductions in employment. Similarly to the discussion above, the distribution implications of such employment effects would depend on who becomes unemployed among minimum wage families.

It is well beyond the scope of this study to attempt to weigh the different impacts described above in the GE settings allowing for employment effects to revise the measures of distributional impacts of the minimum wage. One would need to specify the elasticities of consumer demands for all goods by all groups (including foreign), their labor sup-

[20] This feature arises, e.g., in the search model developed by Lang and Kahn (1998). In testing this model, they find evidence that minimum wage laws shift employment away from adults in favor of teenagers and students. Adult breadwinners from lower-income families may be the least skilled.

ply elasticities, capital supply elasticities, allocations of income/resources across types of households, production technologies and intensities of labor and capital in the production of different goods, and even government behavior. The literature does not provide estimates for many of these quantities in a context that would make them compatible with one another to produce a coherent set of predictions.[21]

VIII. Summary of Findings

Advocates of higher minimum wages often cite helping poor families as the primary motive for raising its value. They argue that families primarily supported by low-wage earnings will receive a substantial portion of the benefits and, moreover, that increasing minimum wages imposes very little public or social cost. Supporters contend that employment impacts experienced by low-wage workers are small, if any at all, and the pass-through of labor costs to prices induces negligible changes.

Using data from SIPP and CES for the year 1996, the exercise described in this paper simulates the distributional impacts of the rise in the federal minimum wage from $4.25 to $5.15 implemented in 1996–97; in 2010 dollars, this increase corresponds to a change from $5.91 to $7.00. Following the assumptions maintained by advocates, the simulation presumes (i) that low-wage workers earned this higher wage with no change in their employment or any reduction in other forms of compensation, (ii) that these higher labor costs were fully passed on to consumers through higher prices, and (iii) that consumers simply paid the extra amount for the goods produced by low-wage labor with no change in their quantities purchased. The cost of this increase is about $15 billion, which was nearly half the amount spent by the federal government on such antipoverty programs as the federal EITC, AFDC/TANF, or food stamp program. The analysis assesses the extent to which various categories of families benefit from higher earnings and the amounts that these groups pay more as consumers through higher prices. Combining these two sides yields a picture of who gains and who pays for minimum wage increases, including the net effects for families.

On the benefit distribution side, as other research has shown, the picture portrayed by this analysis sharply contradicts the view held by proponents of the minimum wage. Low-wage families are typically not low-income families. The increased earnings received by the poorest fam-

[21] The challenges would be even more formidable if one were to attempt to estimate directly who actually benefited from and who actually paid for the 1996 increase in the federal minimum wage in a GE setting. Not only would the data requirements be formidable, one would need compatible estimates for all consumer groups linked to the types of employers that they work for. Moreover, complications would arise in recognizing that neither labor nor goods can be segregated simply into the low-wage and high-wage categories exploited in the GE framework developed in the Appendix.

ilies are only marginally higher than those of the wealthiest. One in four families in the top fifth of the income distribution has a low-wage worker, which is the same share as in the bottom fifth. Virtually as much money goes to the highest-income families as to the lowest. While advocates compare the wage levels to the poverty threshold for a family to make the case for raising the minimum wage, less than $1 in $5 of the additional earnings goes to families with children that rely on low-wage earnings as their primary source of income. Moreover, as a pretax increase, 22 percent of the incremental earnings are taxed away as Social Security contributions and state and federal income taxes. The message of these findings is clear: raising wages wastefully targets the poor contrary to conventional wisdom.

Turning to who pays the costs of an increase in the federal minimum wage through higher prices, the analysis reveals that the richest fifth of families do pay a much larger share (three times more) than those in the poorest fifth. This outcome reflects the fact that the wealthier families simply consume much more. However, when viewed as a percentage of expenditures, the picture looks far less appealing. Expressed as a percentage of families' total nondurable consumption, the extra costs from higher prices are slightly above 0.5 percent for families at large. The picture worsens further when one considers costs as a percentage of the types of consumption normally included in the calculation of state sales taxes, which excludes a number of necessities such as food and health care. More important, the minimum wage costs as a share of "taxable" annual expenditures monotonically fall with families' income. In other words, the costs imposed by the minimum wage are paid in a way that is more regressive than a sales tax.

On net, the minimum wage does redistribute income slightly in favor of lower-income families, with higher-income families paying more in increased prices than they benefit from the rise in their earnings. However, adverse impacts occur within income groups. Whereas fewer than one in four low-income families benefit from a minimum wage increase of the sort adopted in 1996, all low-income families pay for this increase through higher prices, rendering three in four low-income families as net losers. Meanwhile, many higher-income families are net winners.

Political support for the minimum wage largely depends on the apparent clarity of who benefits and the inability to trace who pays for the wage increase, irrespective of whether costs are paid through higher prices, lower profits, or cutbacks in jobs or employee benefits. As shown in this study, the benefits created by the minimum wage go to families essentially evenly distributed across the income distribution; and, when minimum wage increases are paid through higher prices, the induced rise in consumption costs mimics the imposition of a value-added or sales tax with a higher tax rate enacted on the goods and services purchased disproportionately by low-income families. Effectively, then, a minimum wage increase emulates imposition of a "national consumption tax" that is more regressive than a typical state sales tax, with its proceeds allocated to families unrelated to their income. Far more poor families suffer reductions in resources than those who gain, and as many rich families gain as poor families. These income transfer properties of the minimum wage reveal it to be an ineffectual antipoverty policy.

Appendix

General Equilibrium Model Incorporating Minimum Wages

This appendix formulates a general equilibrium (GE) model that motivates the calculations presented in this study and that allows for assessing the impacts of relaxing the stringent economic behavioral assumptions need to fully justify these calculations. The following model includes two goods produced by three factor inputs: low-wage labor, high-wage labor, and capital. Five groups consume these goods: low-wage households, high-wage households, nonworking households, a foreign sector, and a government sector. A key feature of this model is that only one of the goods uses low-wage labor as an input and production has a fixed-coefficient technology, which enables development of a specification that implies no employment effects in response to changes in the minimum wage.

Section A describes the production technology of the GE model, and Section B characterizes the demand structure of its economy. Section C presents the implications of raising the minimum wage assuming perfectly inelastic demands for the low-wage good; this specification implies no employment effects on minimum wage workers. Section D presents details of a specification of the GE model that is consistent with the computations performed in this study. Finally, Section E briefly explores how alternative behavioral elements in the GE framework are likely to affect impacts of a minimum wage on equilibrium values of goods and inputs and on distributional consequences.

A. Production Technology and Costs

This GE model consists of a two-sector economy: a "low-wage" and a "high-wage" good. The low-wage good (x) is produced by all three factors of production: low-wage labor (ℓ), high-wage labor (h), and capital (k). The high-wage good (y) is produced with high-wage labor (h) and capital (k) but without any low-wage labor (ℓ). Consistent with the input-output framework used in the paper's empirical calculations, the following fixed-coefficient production functions make up the production technology:

$$x = \min(\alpha_\ell \ell,\ \alpha_h h_x,\ \alpha_k k_x) \quad \text{and} \quad y = \min(\beta_h h_y,\ \beta_k k_y). \tag{A1}$$

The production function coefficients α_ℓ, α_h, α_k, β_h, and β_k determine the intensities of labor and capital inputs. The quantities h_x and h_y and k_x and k_y measure the amounts of high-wage labor and capital serving as inputs in the production of the goods x and y; no subscript appears for the low-wage labor input ℓ since this factor is used only in the production of good x.

A fixed-coefficient production technology is well known to imply the following relationships linking factor inputs and outputs:

$$\begin{aligned} x &= \alpha_\ell \ell = \alpha_h h_x = \alpha_k k_x, \\ \ell &= \frac{x}{\alpha_\ell},\ h_x = \frac{x}{\alpha_h},\ k_x = \frac{x}{\alpha_k}, \end{aligned} \tag{A2}$$

and

$$\begin{aligned} y &= \beta_h h_y = \beta_k k_y, \\ h_y &= \frac{y}{\beta_h},\ k_y = \frac{y}{\beta_k}. \end{aligned} \tag{A3}$$

Defining $k = k_x + k_y$ and $h = h_x + h_y$, the above relationships imply

$$k = k_x + k_y = \frac{\alpha_h}{\alpha_k} h_x + \frac{\beta_h}{\beta_k} h_y = \frac{\beta_h}{\beta_k} h + \left(\frac{\alpha_h}{\alpha_k} - \frac{\beta_h}{\beta_k} \right) h_x, \tag{A4}$$

which is exploited below in the derivation of comparative-static results.

The corresponding cost and price structure implied by this production technology takes the form

$$\begin{aligned} C_x &= \omega\ell + h_x + rk_x = \left(\frac{\omega}{\alpha_\ell} + \frac{1}{\alpha_h} + \frac{r}{\alpha_k} \right) x = P_x x, \\ C_y &= h_y + rk_y = \left(\frac{1}{\beta_h} + \frac{r}{\beta_k} \right) y = P_y y, \end{aligned} \tag{A5}$$

where ω denotes the wage of ℓ (relative to the wage of high-skilled, high-wage labor), r designates the input price of capital (relative to the wage of high-skilled labor), P_x equals the price of good x, and P_y equals the price of good y.

B. Household Sectors and Consumer Groups: Demands for Goods and Labor Supply

Three types of households make up the economy: "high-wage" households, "low-wage" households, and "nonworking" households. In addition, product demands are determined by a government and foreign sector.

1. High-Wage Households

High-wage households select their consumer demands for goods y_h and x_h and their labor supply h by solving the following utility optimization problem:

$$\max U_h(y_h,\ h,\ x_h) \quad \text{subject to } h - \tau_h + (rk - q) = P_x x_h + P_y y_h; \tag{A6}$$

the quantity τ_h in the budget constraint represents the income tax levied on hours of work h; $\tau_h = \tau_h(h)$ is a monotonically increasing convex function of h. This GE formulation presumes that only high-wage households own capital, which accounts for the term $rk - q$ in their budget constraint. The quantity rk measures the income received by these households, and q constitutes the cost of supplying capital; $q = q(k)$ is a monotonically increasing convex function of k. One can think of the function q as incorporating payments of taxes on capital income, but this generalization is ignored in the current construction of the GE model to simplify the exposition.

To characterize preferences for high-wage households, designate their marginal rates of substitution (MRS) between the high-wage good and hours of work and between the low-wage good and hours of work as

$$\begin{aligned} M_h(y_h, h, x_h) = M_h &= -\frac{\partial U_h}{\partial y_h} \Big/ \frac{\partial U_h}{\partial h} > 0, \\ S_h &= -\frac{\partial U_h}{\partial x_h} \Big/ \frac{\partial U_h}{\partial h} > 0. \end{aligned} \tag{A7}$$

Quasi concavity of preferences in consumption y_h and in leisure (i.e., $-h$) implies

$$\frac{\partial M_h}{\partial y_h} < 0 \quad \text{and} \quad \frac{\partial M_h}{\partial h} < 0. \tag{A8}$$

Analogous preference assumptions would imply the same inequality properties for S_h.

Equilibrium values of goods x_h and y_h and labor supply h must satisfy the first-order conditions

$$\begin{aligned} M_h(y_h, h, x_h) &= M_h\left(\frac{1}{P_y}[h - \tau_h + (rk - q) - P_x x_h], h, x_h\right) \\ &= \frac{P_y}{1 - \tau'_h}, \\ S_h &= \frac{P_x}{1 - \tau'_h}, \end{aligned} \tag{A9}$$

where $\tau'_h > 0$ denotes the marginal tax rate on hours of work h. Equilibrium values of capital inputs k satisfy

$$r = q' \equiv \frac{\partial q}{\partial k} > 0 \quad \text{and} \quad q'' = \frac{\partial^2 q}{\partial k^2} > 0, \tag{A10}$$

where the inequalities follow from the properties of the function q.

2. Low-Wage Households

Low-wage households select their consumer demands for goods y_ℓ and x_ℓ and their labor supply ℓ by solving the following utility optimization problem:

$$\max U_\ell(y_\ell, \ell, x_\ell) \quad \text{subject to } \omega\ell - \tau_\ell = P_x x_\ell + P_y y_\ell; \tag{A11}$$

the quantity τ_ℓ in the budget constraint represents the income tax levied on hours of work ℓ; $\tau_\ell = \tau_\ell(\omega\ell)$ is a monotonically increasing function of earning $\omega\ell$.

One can define expressions for the MRS relationships M_ℓ and S_ℓ analogous to (A7) with properties (A8).

Equilibrium values of goods x_ℓ and y_ℓ and labor supply ℓ must satisfy conditions

$$M_\ell\left(\frac{1}{P_y}(\omega\ell - \tau_\ell - P_x x_\ell),\ \ell,\ x_\ell\right) = \frac{P_y}{(1-\tau'_\ell)\omega},$$
$$S_\ell = \frac{P_x}{(1-\tau'_\ell)\omega}, \tag{A12}$$

where $\tau'_\ell > 0$ denotes the marginal tax rate on hours of work ℓ.

3. Nonworking Households

Nonworking households select their consumer demands for goods x_n and y_n by solving the following utility optimization problem:

$$\max U_n(y_n,\ x_n) \quad \text{subject to } \tau_n = P_x x_n + P_y y_n; \tag{A13}$$

τ_n represents transfers from the government. One can also readily introduce capital returns as another source of income for these households without any substantive change in the key results below, but again this is not done to simplify the exposition.

One can define expressions for nonworking households' MRS function R_n between goods y and x with properties analogous to (A7).

Equilibrium values of goods y_n and x_n must satisfy conditions

$$R_n = R_n\left(\frac{1}{P_y}(\tau_n - P_x x_n),\ x_n\right) = \frac{P_y}{P_x}. \tag{A14}$$

4. Government and Foreign Sectors

The model includes both foreign and government sectors, with taxes on labor income funding government. Goods demand for government must satisfy

$$\tau_\ell + \tau_h = \tau_n + P_x x_g + P_y y_g. \tag{A15}$$

A similar representation can be introduced for the foreign sector.

C. GE Specification with Perfectly Inelastic Demands for the Minimum Wage Good

The initial formulation of the GE model considered here assumes perfectly inelastic demands for good x for all categories of consumers, which implies in equilibrium that all of the following quantities are fixed: x_h, x_ℓ, x_n, x_g, x, ℓ, h_x, and k_x. Under this assumption, the discussion below describes the impacts of raising the minimum wage on the behavior of the five consumer groups.

1. Impacts of Minimum Wage Increase on High-Wage Households

A standard comparative-statics analysis provides the information necessary for evaluating the effects of raising ω on the values of high-wage households' demand for y_h and their supply of h and k. As the first step, total differentiation of the right-hand-side MRS equilibrium condition in (A9) with respect to ω with x_h held fixed yields

$$\frac{\partial M_h}{\partial y_h}\frac{dy_h}{d\omega} + \frac{\partial M_h}{\partial h}\frac{dh}{d\omega} = \tau_h''\frac{P_y}{(1-\tau_h')^2}\frac{dh}{d\omega} + \frac{1}{1-\tau_h'}\frac{q''}{\beta_k}\frac{dk}{d\omega}. \tag{A16}$$

As the second step, total differentiation of the budget constraint (A6) with respect to ω with x_h held fixed yields[22]

$$(1-\tau_h')\frac{dh}{d\omega} + q''k\frac{dk}{d\omega} = \frac{1}{\alpha_\ell}x_h + \frac{q''}{\alpha_k}x_h\frac{dk}{d\omega} + P_y\frac{dy_h}{d\omega} + \frac{q''}{\beta_k}y_h\frac{dk}{d\omega}. \tag{A17}$$

Total differentiation of (A4) holding x (and, therefore, h_x) constant yields $dk/d\omega = (\beta_h/\beta_k)(dh/d\omega)$, which substituted into (A17) produces

$$\frac{dy_h}{d\omega} = -\frac{1}{P_y}\frac{x_h}{\alpha_\ell} + \frac{1}{P_y}\left[(1-\tau_h') + q''\frac{\beta_h}{\beta_k}\left(k - \frac{x_h}{\alpha_k} - \frac{y_h}{\beta_k}\right)\right]\frac{dh}{d\omega}. \tag{A18}$$

The quantity $k - y_h/\beta_k - x_h/\alpha_k > 0$ since all capital is not fully exhausted by the consumption of high-wage households, and the entire quantity multiplying $dh/d\omega$ is therefore positive.

As the third and final step, substitution of relationship (A18) into (A16) yields

$$\begin{aligned}&\left\{\frac{\partial M_h}{\partial y_h}\cdot\frac{1}{P_y}\left[(1-\tau_h') + q''\frac{\beta_h}{\beta_k}\left(k - \frac{y_h}{\beta_k} - \frac{x_h}{\alpha_k}\right)\right]\right.\\ &\left.+\frac{\partial M_h}{\partial h} - \frac{\tau_h'' P_y}{(1-\tau_h')^2} - \frac{q''\beta_h}{(1-\tau_h')\beta_k^2}\right\}\frac{dh}{d\omega} = \frac{\partial M_h}{\partial y_h}\cdot\frac{x_h}{P_y\alpha_\ell}.\end{aligned} \tag{A19}$$

Since the expression in the right-hand brace of relationship (A19) multiplying $dh/d\omega$ is negative and the right-hand side of this relationship is also negative, this relationship implies

$$\frac{dh}{d\omega} \geq 0 \quad \text{and} \quad \frac{dk}{d\omega} \geq 0, \tag{A20}$$

where the second inequality follows from differentiation of (A4) and using the first inequality. Consequently, with this specification of the GE model, a rise in the minimum wage leads to an increase in the hours worked by high-wage households.

2. Impacts of a Minimum Wage Increase on Low-Wage Households

A similar comparative-statics exercise provides the information needed to assess the impacts of raising ω on the values of low-wage households' demand for y_ℓ. (Recall that their labor supply ℓ remains constant.) This demand response is determined by total differentiation of their budget constraint ($P_y y_\ell = \omega\ell - \tau_\ell - P_x x_\ell$) with x_ℓ and ℓ held fixed, which yields

[22] This result uses $dP_x/d\omega = 1/\alpha_\ell + q''/\alpha_k \cdot dk/d\omega$ and $dP_y/d\omega = q''/\beta_k \cdot dk/d\omega$, which follows $P_x = \omega/\alpha_\ell + 1/\alpha_h + r/\alpha_k$ and $P_y = 1/\beta_h + r/\beta_k$ from (A5) and (A10).

$$\frac{dy_\ell}{d\omega} = \frac{1}{P_y}\left[(1-\tau'_\ell)\ell - \frac{x_\ell}{\alpha_\ell}\right] - \frac{q''}{P_y}\frac{\beta_h}{\beta_k}\left(\frac{x_\ell}{\alpha_k} + \frac{y_\ell}{\beta_k}\right)\frac{dh}{d\omega}. \tag{A21}$$

(This derivation relies on the differentiation relationships exploited in obtaining [A19].)

3. Impacts of a Minimum Wage Increase on Nonworking Households and Other Sectors

The implied effect of the consumption of nonworking households is essentially a special case of the high-wage household without a labor supply response option and no capital ownership. Adapting (A18) without an own labor supply response creates the following relationship showing the effect of raising the minimum wage for nonworking households on their demand for the low-wage good:

$$\frac{dy_n}{d\omega} = \frac{1}{P_y}\left(\frac{d\tau_n}{d\omega} - \frac{x_n}{\alpha_\ell}\right) - \frac{q''}{P_y}\frac{\beta_h}{\beta_k}\left(\frac{x_n}{\alpha_k} + \frac{y_n}{\beta_k}\right)\frac{dh}{d\omega}. \tag{A22}$$

A similar expression can be derived for the government and foreign sectors, but to do so provides no insights beyond what already appears above.

D. GE Specification Consistent with Empirical Calculations in the Study

In addition to having no employment effects occur for low-wage workers in response to changes in the minimum wage as accomplished above by assuming perfectly inelastic demands for good x, the calculations performed in this study also maintain the behavioral assumption that the labor supply of high-wage workers is also perfectly inelastic. This no–employment impact characterization of the economy mimics the critical notions advocated by many supporters of minimum wage policies.

For high-wage households, if one introduces the commonly held belief that the labor supply of the high-wage households is entirely unresponsive to their wages, then (A18) reduces to

$$\frac{dy_h}{d\omega} = -\frac{1}{P_y}\frac{x_h}{\alpha_\ell} < 0. \tag{A23}$$

Comparison with (A18) reveals that the decline in the demand for high-wage goods by high-wage households is mitigated when these households have elastic labor supplies and respond positively to compensate for the loss of resources arising from higher prices for the low-wage good induced by increasing the minimum wage.

For low-wage households, (A21) simplifies to

$$\frac{dy_\ell}{d\omega} = \frac{1}{P_y}\left(\ell - \frac{x_\ell}{\alpha_\ell} - \tau'_\ell\ell\right). \tag{A24}$$

The quantity $\ell - x_\ell/\alpha_\ell > 0$ since all of the low-wage good is not fully consumed by low-wage households. Consequently, the consumption of the high-wage good by low-wage households increases unless the progressivity of taxation overcomes this effect.

Finally, for nonworking households, (A22) reduces to

$$\frac{dy_n}{d\omega} = \frac{1}{P_y}\left(\frac{d\tau_n}{d\omega} - \frac{x_n}{\alpha_\ell}\right). \tag{A25}$$

Accordingly, consumption of the high-wage good by these households will decline because the loss of resources attributable to higher prices for the low-wage good induced a higher minimum wage, unless sufficient governmental transfers make up for the difference. Note that all of these transfers come from minimum wage households through their higher taxation on earning.

Relationships (A18), (A19), (A21), and (A22) determine the effects of increasing the minimum wage in a GE framework with the consumer demands for the low-wage good assumed to be perfectly inelastic. With the labor supply response of high-wage workers also deemed to be perfectly inelastic, these relationships become (A23), (A24), and (A25). When combined with the analogous relationships for the government and foreign sections, this specification of a GE model is consistent with the accounting computations presented in this study.

E. *Evaluating Minimum Wage Impacts under More Flexible Behavioral Assumptions*

The above relationships provide insights into how business owners share in the costs of increasing the minimum wage in this GE setting. If the supply of capital inputs is perfectly elastic—which could arise when international markets set rates of return and the foreign sector supplies incremental capital at a constant rate—then $q'' = 0$. In this case, all of the simplifications for the demands of low-wage and nonworking households in Section D apply without assuming that high-wage households have unresponsive labor supply. The income earned by capital is unaffected by the minimum wage.

Alternatively, if one relaxes this elasticity assumption and allows the supply of capital to involve increasing costs (as captured by $q = q(k)$), then raising the minimum wage will increase the returns to capital (and profits). When high-wage households have responsive labor supply, a rise in the minimum wage induces an increase in the hours worked by these households (see [A20]), and capital inputs must rise to accommodate increased production of the high-wage good. Relationship (A19) shows that $dh/d\omega$ (and $dk/d\omega$) declines as the marginal costs of capital ($q'' > 0$) increase. The impact on the demand for y_h is formally ambiguous according to relationship (A18) because of the contribution of capital returns to the income of high-wage households. However, this is not the case for the demands y_ℓ and y_n, which unambiguously decline according to relationships (A21) and (A22) as the marginal costs of capital q'' increases.

Loss of employment will occur for low-wage labor when the production technology allows for flexible factor substitution among inputs, and this will be true even with perfectly inelastic demands for goods produced by low-wage workers. Without the fixed-coefficient production technology, a rise in the minimum wage would induce substitution of other factors of production against low-wage labor in the GE specification presented above.

Relaxing the perfect inelasticity of the demands for low-wage goods invokes operation of the MRS relationships S_h, S_ℓ, and R_n characterized by relationships (A7) along with equilibrium conditions (A9) for all consumer groups. Conventional demand income and substitution effects apply. High-wage and nonworking households will substitute against the low-wage good in response to its higher price, contributing to a decline in its aggregate demand. This effect also operates for low-wage workers, but the increase in their wages more than compensates for the rise in higher prices given the production technologies maintained in this GE framework. The impact on their labor supply depends on the familiar forces determining whether workers exhibit backward-bending labor supply. Given these counterbalancing forces, the overall impact in this GE setting will depend on the size of these net effects and the share of low-wage households in the economy. Because the fixed-coefficient production technology requires the hours of work of low-wage workers and the goods produced by this labor to remain in fixed proportions, an overall decline in the demand for low-wage goods would necessarily translate into a loss of employment for minimum wage workers.

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PEOPLE VERSUS MACHINES:
THE IMPACT OF MINIMUM WAGES ON AUTOMATABLE JOBS

Grace Lordan
David Neumark

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People Versus Machines: The Impact of Minimum Wages on Automatable Jobs
Grace Lordan and David Neumark
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ABSTRACT

We study the effect of minimum wage increases on employment in automatable jobs – jobs in which employers may find it easier to substitute machines for people – focusing on low-skilled workers for whom such substitution may be spurred by minimum wage increases. Based on CPS data from 1980-2015, we find that increasing the minimum wage decreases significantly the share of automatable employment held by low-skilled workers, and increases the likelihood that low-skilled workers in automatable jobs become nonemployed or employed in worse jobs. The average effects mask significant heterogeneity by industry and demographic group, including substantive adverse effects for older, low-skilled workers in manufacturing. We also find some evidence that the same changes improve job opportunities for higher-skilled workers. The findings imply that groups often ignored in the minimum wage literature are in fact quite vulnerable to employment changes and job loss because of automation following a minimum wage increase.

Grace Lordan
Department of Social Policy
London School of Economics
Houghton Street
London WC2A 2AE
United Kingdom
G.Lordan@lse.ac.uk

David Neumark
Department of Economics
University of California, Irvine
3151 Social Science Plaza
Irvine, CA 92697
and NBER
dneumark@uci.edu

Introduction

For decades, economists have studied the effects of the minimum wage on employees in the United States. These studies have largely focused on the employment effects for low-skilled workers – with the principal focus on teenagers. Overall, there is some controversy regarding whether disemployment effects exist, with some studies finding no effects,[1] although with more – and more diverse kinds of studies – finding evidence of disemployment effects.[2]

In this study, we explore the extent to which minimum wages induce substitution away from workers whose jobs are more easily automated. For instance, employers may substitute away from labor with technological innovations – such as supermarkets substituting self-service checkout for cashiers, and assembly lines in manufacturing plants substituting robotic arms for workers. At the same time, firms may hire other workers who perform new tasks that are complementary with the new technology. For example, a firm using more robots may hire individuals to service, troubleshoot, and maintain these new machines. It seems reasonable to expect that the workers more likely to be replaced following minimum wage increases are those who are low skilled, earning wages affected by increases in the minimum wage, while workers who "tend" the machines are higher skilled. This suggests that there is a potential for labor reallocation away from jobs that are automatable following increases in the minimum wage, that low-skilled workers in automatable jobs are particularly vulnerable to minimum wage increases, and that the net disemployment effects

[1] See, for example, Card and Kruger (1994); Card and Kruger (2000); Dube, Lester, and Reich (2010); Allegretto, Dube, and Reich (2011); and Addison, Blackburn, and Cotti (2012).
[2] See for example Neumark and Wascher (1996); Neumark (2001); Singell and Terborg (2007); Neumark and Wascher (2007); Thompson (2009); Sabia, Burkhauser, and Hansen (2012); Neumark, Salas, and Wascher (2014a, 2014b); Clemens and Wither (2016); Meer and West (2015); and Powell (2016). Neumark (2017) reviews the very recent literature, classifying the kinds of studies that find disemployment effects and the kinds that do not.

may be smaller than the gross effects that workers in automatable tasks experience.[3,4]

We choose to focus on automation as it has been one of the dominant forces that has threatened low-skilled jobs in the United States in recent decades (Autor and Dorn, 2013; Autor, Dorn, and Hanson, 2015), presumably because of both technological advances and reductions in the cost of technology that can substitute for low-skilled labor. As emphasized by Autor and Dorn (2013) and Goos, Manning, and Salomons (2014), the hollowing out of mid-skill occupations has been a significant channel through which automation has affected the occupation distribution over time. However, the advancement of technology in industry has also touched the occupations in which low-skilled individuals work. This is illustrated in Figure 1, which shows a clear downward trend in the degree to which job tasks of low-skilled individuals are automatable, from 1980-2015.[5] There is also evidence that this was spurred by computerization. As shown by Autor and Dorn (2013), computerization in industry has accelerated over the last four decades, and this technology diffused faster into areas that have higher shares of automatable employment. Such evidence suggests, as we would expect, that firms choose to substitute technology for workers as it becomes cheaper for them to do so.

The core idea or hypothesis underlying our analysis is that minimum wage increases have the potential to spur the automation of low-skilled jobs, via substituting technology for low-skilled workers. These minimum wages increases raise the price of low-skilled labor, increasing the cost savings from this substitution. The main aim of our paper is to explore this

[3] Of course, employers can respond to an increase in the minimum wage in a number of ways besides culling jobs. Other channels of adjustment that have been explored in the minimum wage literature include changes in hours – where the empirical evidence is mixed (see Neumark and Washer, 2008, p. 78), job amenities (see Simon and Kaestner, 2004), prices (see Aaronson, 2001; Aaronson, French, and MacDonald, 2008; Lemos, 2008; and MaCurdy, 2015), and compression of wage differentials (see DiNardo, Fortin, and Lemieux, 1996; and Autor, Manning, and Smith, 2016).

[4] In a recent paper, Basker et al. (2017) explore a different kind of substitution of technology for labor (at least, the firm's labor) that can occur in response to a higher minimum wage – namely, substitution of a customer's labor for a worker's labor (in, e.g., a self-service gas station, or using a bank ATM). They suggest that this kind substitution may occur when low-skilled labor becomes expensive and technology enables labor replacement in tasks that are not easy to automate.

[5] Figure 1 is based on a measure of "routine task intensity" (*RTI*) discussed below (see equation (1)).

hypothesis, and in so doing to provide a richer understanding of how minimum wage policies have been shaping the type of employment held in the United States, within industries, and for particular demographic and skill groups.

Specifically, we first assess whether the share of employment that is automatable declines in response to minimum wage increases. We focus on jobs that tend to be held by low-skilled workers, given that these are the jobs for which labor costs increase the most in relative terms following a minimum wage increase, which can prompt firms to substitute from people (low-skilled ones, in particular) towards machines. We complement our analyses of how the share of employment in automatable jobs responds to minimum wage increases with analyses of employment impacts for individual workers, estimating whether the probability that a low-skilled individual working in an automatable loses their job is larger following a minimum wage increase. We also explore other impacts on low-skilled workers, as well as whether job opportunities improve for higher-skilled workers in the industries where a high share of low-skilled employed was in automatable jobs.

Our analysis is related to concurrent research by Aaronson and Phelan (forthcoming), who, for the period 1999-2009, analyze the susceptibility of low-wage employment to technological substitution in the short run. Specifically, they focus on regressions that model the probability of being employed within the next two years against measures of the task content in an individual's current job. They find that minimum wage increases lead to job losses for cognitively-routine jobs, but not manually-routine or non-routine jobs. Their study provides some evidence that firms may automate routine jobs in response to a minimum wage increase, reducing employment opportunities for workers in routine jobs.

Our study contributes beyond this analysis in a number of ways. First, while Aaronson and Phelan (2017) are concerned with an average individual's job loss, we focus on quantifying how shares in the employment of automatable tasks change following a minimum wage change, to provide more evidence on how the task composition of the workforce is affected. Second, we expect that automation is a viable and likely substitute for certain types of low-skilled jobs, and therefore also certain types of low-skilled labor, implying that average effects may mask significant heterogeneity. We therefore attempt to provide a fuller picture of labor market adjustments across industries and a variety of demographic groups, which can uncover these important differential responses. As discussed below, this if of particular interest with respect to the broader minimum wage literature.

Third, for those who lose their jobs to automation following a minimum wage increase, we expect that the risk of not being able to find a similar job is greater for some groups as compared to others, and that an inability to do so has longer-term adverse consequences for earnings (and re-employment). Hence, we also analyze the effects of minimum wage increases on whether particular types of low-skilled individuals working in automatable jobs are more or less likely to stay in the same "job" (narrow occupation and broad industry) following a minimum wage increase. Finally, we extend the analysis to cover more outcomes for low-skilled workers, and to assess effects on higher-skilled workers.

Together, our analyses provide the first evidence on how the shares of automatable jobs change following a minimum wage increase, and on the effects of minimum wages on groups that are very often ignored in the minimum wage literature, such as effects on older less-skilled workers who are in jobs where it is easier to replace people with machines.

Our work is timely given that many U.S. states have continued to regularly raise their minimum wages, and a large number of additional states have newly implemented minimum wage laws (all higher than the federal minimum wage), with a number of states now indexing their minimum wages. As of January 7, 2017, 30 states (including the District of Columbia) had a minimum wage higher than the federal minimum wage of $7.25, ranging as high as $11 in Washington State, and $11.50 in the District of Columbia.[6] Moreover, many U.S. cities

[6] See https://www.dol.gov/whd/minwage/america.htm (viewed February 1, 2017).

have implemented minimum wages, with the minimum wage in Seattle (and nearby Sea-Tac) already reaching $15. Policy debate regarding these increases frequently references the literature on disemployment effects discussed above (a literature from which advocates on either side can pick evidence to support their view). But this literature largely focuses on teenagers, for whom employment effects are either irrelevant, or at best very tangentially related, to the more important policy question of whether higher minimum wages help low-income families. If employment changes in response to higher minimum wages mask larger gross effects for subgroups of low-skilled workers in automatable tasks – and in particular subgroups ignored in the existing minimum wage literature – then the reliance of policymakers on evidence for teenagers may be ignoring potentially adverse effects for older workers more likely to be major contributors to their families' incomes.

Our empirical analysis draws on CPS data from 1980-2015. We distinguish between occupations that are intensive in automatable tasks by drawing on definitions provided in Autor and Dorn (2013) and Autor et al. (2015). We calculate for each industry within each state-year cell an automatable employment share.[7] The core of our analysis links these measures to changes in the relevant minimum wage.

Overall, we find that increasing the minimum wage decreases significantly the share of automatable employment held by low-skilled workers. Our estimates suggest that the elasticity of this share with respect to the minimum wage is −0.10. However, these average effects mask significant heterogeneity by industry and by demographic group. In particular, there are large effects on the shares of automatable employment in manufacturing, where we estimate an elasticity of −0.18). Within manufacturing, the share of older workers in automatable employment declines most sharply, and the share of workers in automatable employment also declines sharply for women and blacks.

[7] We actually distinguish between urban and non-urban areas within each state.

Our analysis at the individual level draws many similar conclusions. We find that a significant number of individuals who were previously in automatable employment are nonemployed in the period following a minimum wage increase. These effects are relatively larger for individuals employed in manufacturing, and are larger for the oldest and youngest workers, for females and for blacks. Overall, this analysis points to important heterogeneity in the employment effects of minimum wages – including some potentially positive effects for higher-skilled workers in jobs where the minimum wage spurs substitution away from low-skilled workers in automatable jobs. Moreover, our evidence highlights potentially adverse consequences of higher minimum wages for groups of workers that have not typically been considered in the extensive research literature on the employment effects of minimum wages. Thus, a main message from our work is that groups often ignored in the minimum wage literature are in fact quite vulnerable to employment changes and job loss because of automation following a minimum wage increase.

Analysis of Shares of Employment in Automatable Jobs

Methods

Most of our analysis focuses on low-skilled individuals, who we define as having a high school diploma equivalent or less. We use data from Autor and Dorn (2013) and Autor et al. (2015) to measure routine task intensity (*RTI*) in jobs held by low-skilled workers. These authors use *RTI* as a proxy for determining the degree to which the tasks within an occupation are automatable. In particular, routine task intensity in each three-digit occupation is defined as follows:

$$RTI_k = \ln(T_k{}^R) - \ln(T_k{}^M) - \ln(T_k{}^A) \qquad (1)$$

where $T_k{}^R$, $T_k{}^M$, and $T_k{}^A$ are the levels of routine, manual, and abstract task inputs for occupation k.[8] Routine tasks involve a repeated sequence of actions, are easily codifiable, and

[8] These levels are defined using variables from versions of the *Dictionary of Occupation Titles,* where incumbents are asked to grade the level of their occupation with respect to particular attributes.

are therefore substitutable with technology. In contrast, manual tasks require actions that are not generally predictable in sequence, so substitution with technology is limited.

To provide some examples, blue-collar jobs that are highly routine include machinists and typesetters. Jobs with low routine task intensity include bus driving and service station occupations. Blue-collar jobs that are classified as high on manual task intensity include taxi drivers, operating agents of construction equipment, and drivers of heavy vehicles, while meat cutters and upholsterers are low on this domain. Abstract tasks require high-level thinking that is more complementary with technology (Autor, 2013). Examples of low-skilled jobs that are high on abstract task intensity include supervisors of motor vehicle transportation, railroad conductors, and production foremen. Jobs that are low on abstract task intensity are garbage collectors, parking lot attendants, and packers. Thus, equation (1) is increasing in the absolute and relative quantity of tasks that are automatable within occupation *k*.

We further calculate for each industry *i*, within each area *a* (defined as states divided into urban and nonurban areas), in year *t*, a routine employment share, as follows:

$$RSH_{iat} = (\sum_{k=1}^{K}(L_{iat}) \cdot 1[RTI_k > RTI^{P66}])(\sum_{k=1}^{K}(L_{iat}))^{-1} \ . \qquad (2)$$

In equation (2), L_{iat} is equal to total employment in industry *i* in area *a* at time *t*. 1[.] is an indicator function equal to one if an occupation is in the top third of the employment-weighted distribution of *RTI* across occupations (RTI^{P66} denotes the 66th percentile), using only low-skilled workers. The numerator is then the share of automatable low-skill employment in a particular industry, area, and year, and the denominator is total low-skilled employment in that industry, area, and year.

Our analysis initially focuses on the following specification:

$$RSH_{iat} = b_1 Log(MW_{st}) + A_a \gamma + T_t \lambda + \varepsilon_{iat} \quad , \qquad (3)$$

where MW_{st} denotes the minimum wage in state *s* at time *t*. We use the log of minimum wages following the literature on minimum wages in the last decade or more. Equation (3) also includes area (A_a) and year (T_t), fixed effects. Area is defined as state-specific dummy variables interacted with whether the individual lives in an urban area or not. Negative and significant estimates of b_1 would imply that the share of employment that is automatable declines in response to minimum wage increases.[9]

We next turn to disaggregating these effects across industries and demographic groups, to see whether there are sectors or groups particularly vulnerable to automation in response to minimum wage increases. In other work, differential patterns of task reallocation have been documented across demographic groups. For example, less-educated, male, and young workers have been the most susceptible to reductions in employment that is intensive in routine tasks (Autor and Dorn, 2013; Autor and Dorn, 2009). We therefore focus on differences in effects by age and sex, and we also examine differences by race.[10] Specifically, for race we look at whites and blacks (we do not look at other categories given small cell sizes), and for age we look at those aged 40 and over, those aged 25 or younger, and the intermediate group aged 26-39.

To unpack the impact of minimum wage increases by age, sex, and race, we use measures of task intensity for each subgroup (indexed by c), as follows:

$$RSH_{ciat} = (\sum_{k=1}^{K}(L_{ciat}) \cdot 1[RTI_k > RTI^{P66}])(\sum_{k=1}^{K}(L_{ciat}))^{-1} \quad .^{11} \tag{4}$$

In this case the numerator is the share of automatable employment held by a subgroup

[9] We also augmented equation (3) adding up to three lags of the minimum wage variable. The inclusion of lags allows for a period of adjustment to reorganize the factors of production away from labor and towards capital investments in technology (and perhaps other complementary labor). In all models, the lags were not significant, suggesting that investment in technology is relatively fast. As we discuss later, however, the minimum wage is defined based on the average minimum wage in the current and past 11 months, itself averaged over the year, so that the absence of lagged effects still allows effects that can arise over nearly two years.

[10] The minimum wage literature also has many examples of papers that consider variation in employment effects across subgroups – for example, gender (Dube, Lester, and Reich, 2016), age (Giuliano, 2012), and ethnicity (Allegretto, Dube, and Reich, 2011).

[11] RTI_k and RTI^{P66} are computed, as before, for all low-skilled workers.

in a specific industry, area, and year, and the denominator is total employment of a subgroup by industry, area, and year. We estimate equation (3) for the separate subgroups, indexed by *c*, using *RSH* as defined in equation (4).

There are two main sources of tasks that are routine intensive. The first are tasks found in blue-collar manufacturing occupations that are also capital intensive. For example, automobiles are most often produced using conveyor belts. Workers perform tasks within this assembly line, which are routine and substitutable with robotic arms. The second is codifiable administrative-support tasks that are typical to the inputs required in the financial services industries, among others (Autor and Dorn, 2013; Autor et al., 2015). The variation across industries in the proportion of individuals that are working in automatable employment, among low-skilled workers, is reported in column (1) of Table 1. Finance, retail, manufacturing, and public administration ("P. Adm.") have particularly high shares of low-skilled workers doing automatable tasks.

We expect the minimum wage to change the share of employment in automatable tasks in differing degrees for particular industries. The impact directly relates to how dominant an automatable task type is among low-skilled in the industry in question, and the ease and cost of automating tasks. To uncover whether there are differential effects by industry we estimate equation (3) separately by one-digit industry, in the aggregate (using *RSH* as defined in equation (2)), and by demographic group (using *RSH* as defined in equation (4)).

Data

Our main data source for the analysis of employment shares is pooled monthly CPS samples from 1980-2015. These data are matched to monthly state-level data on the minimum wage.[12] We allow for a period of adjustment by defining the minimum wage as the

[12] These minimum wage data are available at https://www.socsci.uci.edu/~dneumark/datasets.html.

average over the current month plus the last 11 months. In addition, we do not include agriculture and mining in our subgroup analysis by industry, as we cannot meaningfully or reliably calculate RSH_{iat} in many states or areas with a low representation of these industries. We then create our share of employment variable on a yearly basis.[13]

We rely on crosswalks provided by Autor and Dorn (2013) and Dorn (2009) to convert occupation codes in the CPS to a consistent coding system across years.[14] *RTI*, described in equation (1), is provided by Autor and Dorn (2013) and is matched to the CPS data using this coding system. As noted earlier, we use data on low-skilled individuals with a high school diploma equivalent or less.

Individual-Level Analysis

Methods

Even if the share of automatable jobs declines for low-skilled workers (per the prior analysis), employment opportunities need not decline if these workers are reallocated to non-automatable jobs. We therefore also estimate regressions using individual-level data on low-skilled individuals to explore whether job prospects worsen for those low-skilled workers who were in routine jobs when the minimum wage increases. Specifically, we estimate the model:

$$Emp_{jiai+1}=b_1 \cdot RSH_{jiat} \cdot Log(MW_{at})+ b_2 RSH_{jiat} +T_t \cdot A_s \lambda + \varepsilon_{jiat} \quad , \qquad (5)$$

where *Emp* is the probability that the j^{th} person is employed in industry *i*, area *a*, at time *t*+1. It is assigned zero if a person was nonemployed in *t+1*. The sample consists of those employed in period *t*, and either employed or nonemployed (i.e., unemployed or not in the labor force) in period *t+1*.

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[13] This choice is made for statistical reasons given that cell sizes are too small for accurate calculation of RSH_{iat} on a monthly basis, especially for some industries and demographic groups. This level of analysis is also more intuitive given that automation requires some period of adjustment.

[14] Specifically, we follow Lordan and Pischke (2016) and match the currently relevant Census occupation code system (1980, 1990, 2000 or 2010) to the relevant Autor and Dorn crosswalk. This gives us a consistent coding system that can be matched directly to our measure of automatable tasks.

Equation (5) relates this job loss to workers having held a routine job in period *t*, and faced a minimum wage increase, with the coefficient b_1 on the interaction $RSH_{jiat} \cdot Log(MW_{at})$ capturing whether a person in automatable work is more vulnerable to job loss following a minimum wage increase, compared to those not in automatable work. Note that the minimum wage and the routine share (*RSH*) are measured in period *t*, and the employment transition is measured from period *t* to period *t+1*.[15] All control variables are also measured at time *t*. We can only look at those initially employed because we need to classify jobs by *RTI*; hence, we capture only flows out of employment.[16]

Equation (5) also includes a full set of area-by-year interactions (where area is defined by state and urban or nonurban areas within states), to allow flexibly for differential yearly shocks to states and subareas of states.[17] Given the inclusion of the area-by-year interactions, the main effect Log(MW_{st}) drops out of the equation, and identification of the coefficient on the interaction comes from variation in the availability of automatable jobs within areas across time.[18]

All other definitions are consistent with equations (1) through (4). If individuals working in automatable jobs at the time of a minimum wage increase are more likely to lose

[15] One might want to measure *RSH* prior to when the minimum wage is measured, to avoid contemporaneous changes associated with the minimum wage. But we do not have longer lagged information on employment with which to lag the measurement of *RSH.*
[16] We cannot investigate models with lags or additional leads as we do not know where the individual was working beyond two periods.
[17] We cannot allow this much flexibility in the share analysis because this is the level at which the minimum wage variation arises in that analysis. In contrast, here we can because we are interested in the effect of the interaction between *RSH* and the minimum wage.
[18] We cannot meaningfully document the overall effect of minimum wages on wages of those in automatable work, since this would restrict us only to those who are employed in both periods, and because the main effect of the minimum wage is subsumed in the fixed effects. Moreover, we do not necessarily expect a larger wage effect for those in automatable work; the substitution response may simply be larger. We did verify that in models for wages, the estimate of b_1 is negative and significant. Assuming (as in past work) that minimum wages on average raise wages of low-skill workers, this suggests that the pay increase induced by a higher minimum wage for those in automatable work is not as high as for those in non-automatable work, which fits the story that automation reduces demand for those in automatable tasks and may increase demand for workers with different (and likely higher) skills.

jobs by the next period, compared to individuals affected by the same minimum wage increase but who are in jobs that are not automatable, we would expect the coefficient on b_1 to be negative. As in the share analysis, we explore heterogeneity in b_1 by estimating equation (5) separately by industry and by demographic subgroup.

We complement these regressions with analyses that consider a dependent variable that equals one if an individual had the same narrow occupation code (3-digit) and broad industry code (1-digit) in the interview year, and zero otherwise (including both the nonemployed and "job" switchers). In these analyses, $b_1 < 0$ would reflect transitions to other employment or to nonemployment – with the former presumably reflecting, to some extent, movements of out of employment in automatable tasks following a minimum wage increase.

Data

We estimate equation (5) using data from the Annual Social and Economic Supplement (ASEC) of the CPS. We focus only on individuals with a high school diploma equivalent or less, as in our shares analysis. The ASEC files are useful for our purposes because they include information on the occupation and industry of the job held by respondents in the previous year, which is period *t* in the analysis described above. Thus, *RSH* is based on this occupation. Columns (2) and (3) of Table 1 report the average probabilities that employed, low-skilled workers in automatable jobs remain employed, or in the same "jobs" (for those who remain in the labor force).

Identification

A potential issue in estimating the effects of minimum wages is whether minimum wage variation is correlated with shocks to low-skill labor markets – possibly due to endogenous policy – in which case we may not identify causal effects of minimum wages. This issue has arisen prominently in recent exchanges on the employment effects of minimum wages; see, most recently, Allegretto, Dube, and Reich (2017) and Neumark and

Wascher (2017).[19] However, we are estimating effects on a subgroup of low-skilled workers, and it seems less plausible that policy is chosen endogenously with respect to outcomes for one subgroup of low-skilled workers. Moreover, our individual-level analysis is even more insulated from this identification issue, because we control in an unrestricted fashion for yearly shocks to states, and their urban and nonurban areas separately. This approach of isolating the effects of minimum wages controlling for state or substate shocks has been advocated by Allegretto et al. (2011) and Dube et al. (2010). While this approach may raise other concerns (see Neumark et al., 2014a), it does have the virtue of potentially controlling for shocks to low-skilled labor markets that are correlated with minimum wage changes.

Finally, evidence of leading minimum wage effects could provide evidence that minimum wage changes respond to expected future changes, in which case our evidence may not be causal. We can assess this evidence for our share analysis, which is based on a panel on observations on areas and industries over time. We estimated versions of equation (3) allowing up to three annual leading terms; these were never statistically significant, and were centered around zero.

Results

Effects on Employment Shares

The results from our share of employment analyses (equation (3)) are reported in Table 2. In the aggregate across all industries, as shown in column (1), we find that minimum wage increases cause a statistically significant reallocation of labour away from automatable tasks. We find that a 10 percent increase in the minimum wage leads to a 0.31 percentage point decrease in the share of automatable jobs done by low-skilled workers, implying an

[19] Recent work by Clemens and Wither (2015) and Baskaya and Rubinstein (2012) indicates that, if anything, the employment effects are more negative when accounting for correlated shocks, suggesting that policy variation is correlated with positive shocks.

elasticity of −0.10.[20]

When we look separately by industry, the estimated effects in construction, wholesale, retail, finance, and public administration are small, centered around zero, and not statistically significant. In contrast, the effects are larger for manufacturing, transport, and services, and significant at the 5- or 10-percent level for manufacturing and transport. For example, the estimates imply that the elasticity of the share of automatable jobs among low-skilled workers in manufacturing with respect to the minimum wage is −0.18.

Table 3 presents our analysis of the effects of the minimum wage on the share of employment in automatable jobs, broken down by demographic group and (in columns (2)-(9) by industry. The estimates point to significant heterogeneity in these effects beyond the differences by industry documented in Table 2. For example, a higher minimum wage significantly reduces the shares of low-skilled workers in automatable jobs for all three age groups (only at the 10-percent level in two cases), but the magnitudes are larger for the youngest and oldest workers. Looking by both age and industry, for older workers (≥ 40 years old) the negative effect mainly arises in manufacturing, retail, and public administration, while for younger workers (≤ 25 years old) the effects are large in many sectors, but the estimate is close to zero in manufacturing, and statistically significant only in services. For the middle age group (26-39) there is sizable estimated decline in manufacturing, but it is well under one-half the effect for older workers. Thus, older workers appear more vulnerable to substitution away from automatable jobs in manufacturing when the minimum wage increases. Moreover, the general adverse effect of the minimum wage for older jobs in automatable jobs is interesting in light of the typical focus of the minimum wage literature – and the evidence of disemployment effects – for very young workers.

[20] We do not include industry fixed effects in the pooled estimates, so that we can detect changes in the share of employment in automatable jobs arising from industry reallocation. However, the estimates including industry fixed effects were very similar.

On average, females are affected more adversely than males: in the aggregate estimates in column (1), the negative estimate is negative and significant only for females, and is ten times larger, indicating that, for females, 10 percent increase in the minimum wage reduces the share of automatable jobs (among the low-skilled) by 0.78 percentage point (an elasticity of −1.53). Across industries, these negative effects for females are concentrated in manufacturing, services, and public administration. For males, none of the industry-specific estimates are statistically significant, but the estimated effects are negative and sizable for manufacturing and retail.

Table 3 also points to more adverse effects on the share in automatable employment for blacks than for whites, with the effect more than double for blacks.[21] However, the effects are heterogeneous across industries. There are sizable adverse estimated effects for whites in manufacturing, transport, services, and public administration, although only the transport estimate is statistically significant. For blacks, there are much larger, and statistically significant, decreases in automatable shares in manufacturing and transport.[22]

Effects on Remaining Employed[23]

The evidence discussed thus far indicates that higher minimum wages lead to substitution away from labor doing routine tasks, among low-skilled workers. However, the

[21] The implied elasticities −0.22 and −0.10 respectively.

[22] We have run the state-level results in Table 2 and 3 with state-specific linear trends. The point estimates are generally consistent with what is reported in Tables 2 and 3 (results available upon request), although the increases in standard errors tend to make the estimated effect insignificant (although not always). In our view, the value of this kind of specification check is sometimes overstated. For example, over long sample periods, the linear restriction is typically unjustified, and linear trends imposed over long periods can lead to nonsensical results (like outcomes that must be positive becoming negative). Moreover, we can largely end up substantially reducing the identifying information. Finally, note that in the individual-level analysis we are able to add state-by-urban-by-year fixed effects, which completely subsume any area-specific trends (which are just restricted versions of arbitrary state-by-year fixed effects). This is an important advantage of the individual-level analysis.

[23] As in the employment shares analysis, we focus here on a dummy indicating whether or not a person is in automatable employment. Appendix B reports similar analyses to those in this subsection, but using a continuous measure of *RTI*. The overall conclusions are generally qualitatively similar and in some cases stronger.

decline in the share of employment in automatable tasks may be accompanied by reallocation of these low-skilled workers to less routine, less automatable tasks. Still, it seems unlikely that job prospects would not have worsened for low-skilled workers in the aggregate, assuming that to some extent jobs with less routine, less automatable tasks are higher skilled.

To study whether a higher minimum wage increases transitions to nonemployment among low-skilled workers who were in jobs with routine tasks, Table 4 reports estimates of equation (5), which models the effects of minimum wage increases on the probability a particular individual who holds an automatable job is still employed in the next period.

Overall, we find evidence indicating that the negative effects on employment shares in automatable jobs reported in Tables 2 and 3 are associated with job loss and transitions to nonemployment among low-skilled workers who were initially doing automatable jobs. Looking across industries in the pooled estimates in column (1), we find evidence (significant at the 10-percent level) of a decline in the probability of remaining employed – and hence an increase in the probability of becoming nonemployed. The −0.001 estimate translates into a small elasticity of the probability of a transition to nonemployment with respect to the minimum wage, −0.013.[24] Examining the results by industry, there is some correspondence between the results in Table 4 and Table 2. For example, the decline in the probability of remaining employed is large in manufacturing, and is sizable (and significant at the 10-percent level) for services. Of course, we do not necessarily expect a tight correspondence between the two types of results across industries, as the possibilities for reallocation low-skilled workers from automatable jobs may vary by industry. There appears to be a tighter correspondence between the results by demographic group, with the evidence in Table 4 pointing stronger effects on job loss for younger workers and black workers.

[24] In computing these elasticities for the estimates of equation (5), note that we use the baseline proportion who become nonemployed (or, in Table 5, change jobs); these are one minus the types of mean probabilities shown in columns (2) and (3) of Table 1.

In Table 4, the estimates in columns (2)-(9) for the second row and below report results disaggregating by both industry and demographic group. One interesting results is that, in manufacturing, there are adverse employment effects for both the oldest and youngest groups of workers in automatable jobs, with implied declines in the probability of employment, from a 10 percent minimum wage increase, of 0.25 and 0.22 percentage point, respectively. The implied elasticities of the probability of becoming nonemployed are −0.28 for older workers in manufacturing, and −0.17 for younger workers in manufacturing. Again, this evidence points to subsets of workers who are not typically considered in the minimum wage literature, yet who are vulnerable to job loss from higher minimum wages. Note, also, that within manufacturing, the adverse effect on employment arises for women, but not for men, and there is statistically significant evidence of job loss for whites, but not blacks (although the point estimate is larger for blacks). On the other hand, looking by industry, the estimates point to larger job loss effects for blacks in transport, wholesale, retail, finance, and services (although the estimates for the latter two industries fall well short of statistical significance).

Effects on Job Switching

Table 5 reports estimates of the same specification, but redefining the dependent variable to equal to one if an individual stayed in the same "job" in the subsequent period, and zero otherwise. A person is defined as being in the same job in *t+1* if they have the same 3-digit occupation code and 1-digit industry code. As in Table 4, the sample is restricted to those employed in period *t*; in addition, those employed must have valid occupation codes. Thus, the estimated effect of the minimum wage-routine interaction captures the change in job opportunities in the worker's initial occupation and broad industry, with a "decline" captured in either non-employment *or* a change of jobs.

Overall, there are many additional larger, significant, and negative effects reported in Table 5, suggesting that higher minimum wages lead to a good deal of job switching among low-skilled workers in automatable jobs, in addition to transitions to nonemployment; this job switching is presumably another cost of higher minimum wages for these workers. In addition, the evidence of such effects within industries suggests there is substantial re-allocation of labor within industries because of the minimum wage increase.

Turning to some specific magnitudes, the overall pooled estimate of −0.0213 implies an elasticity, with respect to the minimum wage, of the probability of changing or losing one's job of −0.15. Across industries, the effect is negative and significant in manufacturing, transport, wholesale, finance, services, and public administration. The estimate is positive only in retail. By demographic group, the adverse effects are, as in Table 5, larger for the youngest and oldest workers. Interestingly, once we include job switching as well as transitions to nonemployment, as we do in Table 5, the evidence of adverse effects for white workers becomes more pronounced, and arises in every industry but retail. In contrast, when we looked only at transitions to nonemployment, in Table 4, the evidence of adverse effects for whites was much weaker. This, again, suggests that negative effects of minimum wages for low-skill workers in automatable jobs arise for groups that have not been the focus of traditional work on the employment effects of minimum wages.

Transitions to Low-Wage Industries

A natural follow-on question is whether individuals who are in automatable employment who switch jobs because of minimum wage increases are more likely to end up in specific industries. Autor and Dorn (2013) argue that workers displaced from automatable jobs tend to move to the retail and services sectors. To explore the evidence in the context of minimum wage effects, we can re-estimate equation (5). We restrict the sample to those employed in period t, as before, but also to those employed only in industries aside from retail or services. We then define the dependent variable to equal one if a person moves to retail or services industry in $t+1$, and zero if they remain employed in an industry outside these two sectors; in the top panel, those nonemployed in period $t+1$ are also coded as zero.

Thus, $b_1 > 0$ in equation (5) (the coefficient on the interaction $RSH_{jiat} \cdot Log(MW_{at}))$ implies that a higher minimum wage pushes low-skilled workers who were in automatable jobs into the retail or services sectors. The results reported in Table 6 indicate that this is the case for both retail and services – whether considered separately or together.

Hours Effects

Our analysis so far has focused on employment. However, there is also a potential for hours in automatable work to decrease following a minimum wage increase. We consider hours explicitly by re-estimating equation (3) using as the dependent variable the share of hours worked among low-skilled workers in automatable employment. We also re-estimate a version of equation (5), for the difference between an individual's usual hours worked in year $t+1$ and year t. In this case, we focus only on those who are employed in the two periods, with positive hours worked, to focus on the intensive margin response.

The results of this analysis to some extent parallel the employment share results in Table 3 and the employment transition results in Table 4. The pooled estimates in the top panel of Table 7 imply that a minimum wage increase of \$1 causes a 0.15 percentage point decrease in the share of hours in automatable jobs done by low-skilled workers overall (an elasticity of −0.05), although this estimate is not statistically significant. However, as for employment, there is a much larger negative effect in manufacturing. We also find larger hours share reductions for women and for blacks, paralleling the findings in Table 3, and large hours share reductions for older workers.

The individual-level analysis is reported in the lower Panel of Table 7. The data for both periods are recalled in the same interview period. The samples are smaller than in table 4 because it only includes individuals who kept their jobs between the two periods. There is also loss due to non-response on the "hours worked last year" question. The estimates suggest significant decreases in hours worked for those initially in automatable jobs following a minimum wage increase. Based on the pooled estimate, a 10 percent increase in the minimum wage generates a 0.16 decrease in hours worked for low-skilled individuals who held an automatable job in the previous period – a small but statistically significant effect. The estimated decline is negative, typically larger, and statistically significant in construction, manufacturing, transport, wholesale, finance, services, and public administration (in the last case only at the 10-percent level). Overall, the results indicate that those in automatable low-skilled work are vulnerable to hours reductions following a minimum wage increase. Across demographic groups, the estimated coefficients are mostly significant and negative. The estimated hours reductions are larger for older workers and the middle age group, for males versus females, and for whites versus blacks.

Are the Effects Stronger in More Recent Data?

It is interesting to re-estimate these models using a shorter, more recent time period, at the risk of losing observations, given that the move towards automation has likely accelerated over time, as technology has been getting cheaper, and labor more expensive. To this end, in Table 8 we report estimates covering 1995-2016, rather than going back to 1980. (We do not report estimates by industry crossed with demographic subgroups.) Comparisons with Tables 2-5 reveal that the overall estimates are generally stronger in the more recent subperiod. This suggests that the substitution response to minimum wages was higher in more recent years, likely because of increased ease of automation (and perhaps minimum wages reaching higher levels).

Moreover, the qualitative pattern across industries and demographic groups often remains similar, although not always. For example, we still find large negative estimates for manufacturing and transport, although the manufacturing estimate is attenuated slightly relative to Table 2. One difference is that in Table 8, there is a considerably larger negative estimated effect for public administration (marginally significant), which could be related to more recent diffusion of personal computers into this industry.

Looking at demographic subgroups, one striking difference is the sharper adverse effect of minimum wages on remaining employed (or employed in the same job) for older workers. This estimated negative effect is largest for older workers in Table 8 (in both the middle and lower panels), but not in Tables 4 or 5. The implication is that, in more recent years, the adverse effect of minimum wages on employment for those in automatable jobs has become relatively worse for older workers, which could reflect a combination of a lower likelihood of retaining a job in the automatable subset of jobs, or a lower ability or willingness to make a transition to a non-automatable job.

One potential concern with comparing results across sample periods is that who gets only a high school diploma or less is changing over time, with people achieving higher levels of education in more recent years. Therefore, there is a risk that negative selection into our definition of the low-skilled also partially explains the strengthening of the results in the most recent time period. However, the most important concern would be if this selection is associated with changes in the minimum wage; based on other research, we regard this as unlikely.[25]

Probing the Effects in Manufacturing

Returning to Tables 3-5, many of our results by industry point to declines in the share of automatable jobs, and increased job loss, in manufacturing. These types of findings are unusual in the minimum wage literature, which usually focuses on very low-skilled workers (hence the emphasis on teenagers, for example, and retail or restaurant workers). Then again, our analysis does not focus on manufacturing in the aggregate, but on low-skilled workers in automatable jobs. Nonetheless, if the effects we estimate in manufacturing are in fact driven

[25] Some past research suggests that minimum wages may lower schooling, possibly by drawing some workers out of school and into full-time work, displacing from the job market high school dropouts who are already working (Neumark and Nizalova, 2007; Neumark and Wascher, 2003). Newer work, however, finds little evidence of such an effect (Neumark and Shupe, in progress). Note also that many of our interesting and in some ways novel results refer to workers who – unlike much past minimum wage research – are not teenagers or young adults, for whom any such schooling response is likely to be largely non-existent.

by minimum wage increases, they should be generated from low-wage rather than high-wage workers.

To that end, we estimate our key results for higher-wage and lower-wage workers in the manufacturing industry, based on wages in occupations within manufacturing. For each low-skill occupation within manufacturing,[26] we compute average wages from the 1980-2016 Merged Outgoing Rotation Groups of the CPS. The low-wage subsample is then defined as the bottom tertile of occupations in this distribution, and the high-wage subsample as the top tertile. These definitions are then matched to the data used for the analyses in Tables 3-5, and we estimate equations (3) and (5) separately for the two sub-samples. Examples of occupations that fall into the high-wage and low-wage categories under this definition are given in Table 9. Those occupations classified as low wage are typically machine operators of some description; in contrast, high wage earners more commonly maintain and install machinery. Notably, those in these low-wage occupations in the bottom tertile regularly earn wages at or near the minimum wage.

The estimates in Table 10 are strongly consistent with the adverse effects of minimum wages on the share of employment in automatable jobs in manufacturing arising from low-wage jobs. Specifically, the coefficient estimates for the high-wage regressions are small, almost never statistically significant, and centered around zero. In contrast, the coefficients in the models for low-wage jobs are uniformly negative, and often sizable and statistically significant. For example, the pooled estimates for low-wage occupations are negative and statistically significant in all three panels, as are the estimates for older workers for the share of employment and the probability of remaining employed (the middle panel). The only case where the evidence of adverse effects for low-wage workers in manufacturing is statistically

[26] We calculate the proportion of low-skilled workers in each occupation. Those with shares greater than 0.5 are defined as being low-skilled occupations.

weak is in the lower panel, for the probability that workers who are in automatable employment hold the same job in the next period; the estimates are always negative, but only the pooled estimate is statistically significant.[27]

Effects on Higher-skilled Workers

We might expect that as the minimum wage reduces jobs for low-skilled workers in automatable jobs, it could also increase jobs for higher-skilled workers who “tend” the machines. For instance, going back to our manufacturing analysis, operators can be replaced with robotic arms, but the robotic arms need maintenance and troubleshooting.

We explore this in Table 11. We estimate the same specification as in equation (5), with the only difference being that we define the dependent variable (and hence the sample) for higher-skilled workers). We continue to define routine work for low-skilled workers, so that we obtain a parallel analysis to the earlier analysis in Tables 4 and 5, but now asking whether the interaction of the minimum wage with a higher share of routine work for low-skilled workers – which reduces job opportunities for them – at the same time increases job opportunities for higher-skilled workers. The estimates in the top panel of Table 11 are for the probability of remaining employed (as in Table 4), and the estimates in the bottom panel are for the probability of remaining employed in the same job (as in Table 5).

The evidence indicates that job opportunities are improved for higher-skilled workers. Nearly every estimated coefficient in Table 11 is positive, and the estimates are often sizable and in some cases statistically significant. For example, in the top panel, we find significant

[27] We consider an alternative definition based on industry, in which for each low-skill sub-industry (at the two-digit level) within manufacturing, we compute average wages from the 1980-2016 Merged Outgoing Rotation Groups of the CPS. The low-wage sub-sample is the bottom tertile of industries in this distribution, and the high-wage subsample is the top tertile. These definitions are again matched to the data used for the previous analyses. The results are shown in Appendix A. Compared to Table 10, the results are quite similar. One difference is that, in this case, the is stronger statistical evidence of adverse effects on the probability of remaining in the same job, by demographic subgroup (e.g., for the oldest and youngest workers, and for women). estimates are often slightly attenuated, although the overall conclusions are the same.

positive effects for the youngest workers and those aged 26-39, and in the bottom panel we find significant (or marginally significant) positive effects for women, and in transport, services, and public administration. Notably, we do not find evidence of a positive effect for older higher-skill workers in either panel, perhaps because the kinds of job opportunities opened up by automation require skills that these older workers are less likely to have or obtain.

Conclusions

This study empirically assesses whether there is labor reallocation away from automatable employment following increases in the minimum wage, and how this reallocation affects the type of employment held in the United States, overall, within industries, and for particular demographic groups. We focus specifically on jobs that tend to be held by low-skilled workers, for which labor costs increase the most in response to minimum wage increases. We estimate the impact of minimum wage increases on the share of low-skilled employment in automatable jobs, and on the probability that a low-skilled individual working in an automatable job stays employed (or stays employed in the same job). We explore and document considerable heterogeneity in these effects across demographic groups, and across industries. The analysis goes beyond the types of workers usually considered in the conventional, long-standing research on the employment effects of minimum wages, such as teenagers – studying, for example, the effects of minimum wages on older less-skilled workers who are in jobs where it is easier to replace people with machines, and on manufacturing workers in such jobs.

Based on CPS data from 1980-2015, we find that increasing the minimum wage decreases significantly the share of automatable employment held by low-skilled workers. The average effects mask significant heterogeneity by industry and demographic group. For example, one striking result is that the share in automatable employment declines rather sharply for older workers – and within manufacturing, most sharply for this age group. An analysis of individual transitions from employment to nonemployment (or to employment in a different job) leads to similar overall conclusions. The heterogeneous adverse effects we document indicate that some groups typically ignored in the minimum wage literature are in fact quite vulnerable to job loss because of automation following a minimum wage increase. At the same time, we find that some of the adverse employment effects among low-skilled workers in automatable jobs are offset by increased employment opportunities for higher-skilled workers, likely because automation of low-skilled work creates other kinds of jobs.

Our work suggests that sharp minimum wage increases in the United States in coming years will shape the types of jobs held by low-skilled workers, and create employment challenges for some of them. Given data limitations, we cannot address the permanence of the effects. However, the decision to use labor-saving technology seems likely to be relatively permanent, especially if – as is becoming increasingly common – minimum wages are indexed so that a minimum wage increase results in permanently higher relative costs of low-skilled labor (Sorkin, 2015).

We have followed the definitions of automatable work as provided by Autor and Dorn (2013). These are very useful definitions for a retrospective analysis, given that the occupations identified as automatable are highly credible. However, in the future many more occupations that employ low-skill workers are on track to be automated, even if they are not currently labelled as 'automatable.' These include, for example, taxi drivers,[28] cashiers,[29] and bricklayers.[30] Therefore, it is important to acknowledge that increases in minimum wage will give incentives for firm to adopt new technologies that replace workers earlier. While these

[28] For example, Uber is currently troubleshooting their driverless car.
[29] There is increasing use of innovations in app technology that allow customers to help themselves to the products they need, pay online and never see a cashier or checkout. This technology has already been adopted for low-value purchases in Apple Stores and in Amazon GO (Amazon's new grocery store).
[30] For example, Fastbrick Robotics has now developed Hadrian X – a robot that lays 1,000 standard bricks in 60 minutes.

adoptions undoubtedly lead to increased job opportunities for some workers – for which we find some evidence – it is likely that there are workers who will be displaced that do not have the skills to do the new tasks. We have identified workers whose vulnerability to being replaced by machines has been amplified by minimum wage increases. Such effects may spread to more workers in the future.

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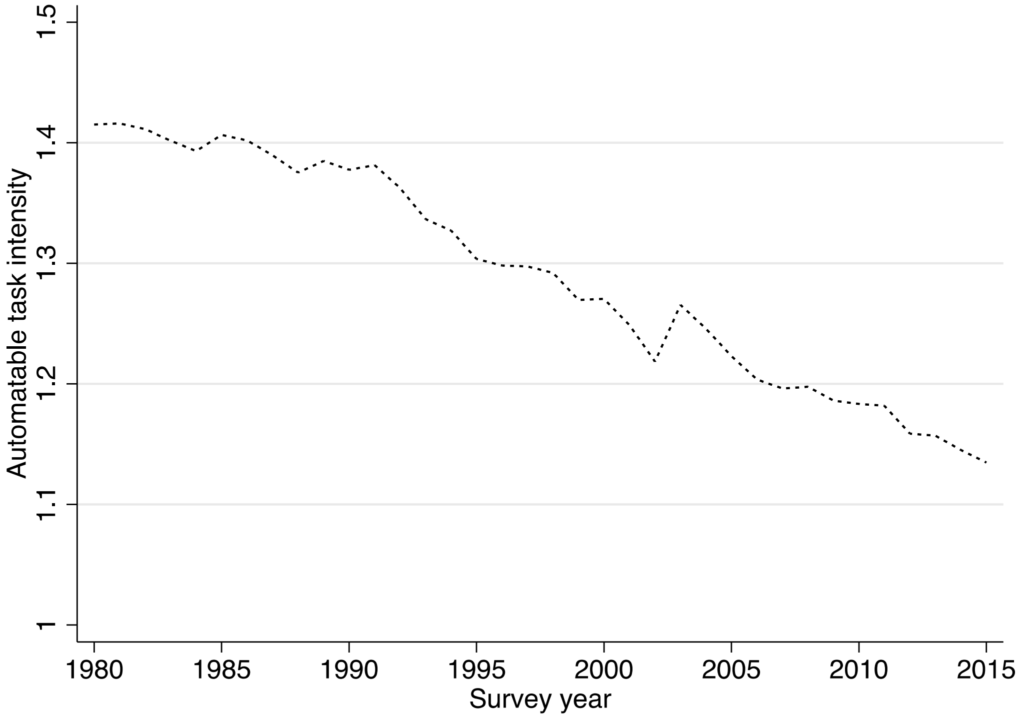
Figure 1: Low-skilled jobs and the level of automation over time



Notes: We plot the average routine task intensity for each year, as given by equation (1). In this figure, the routine task intensity variable is standardized to have a mean of zero and a standard deviation of one.

Table 1: Descriptive Statistics for the Dependent Variables for Each Analysis					
	(1)	(2)	(3)	(4)	(5)
	Shares of automatable employment	P(employed in next period | initially in automatable job)	P(employed in next period in same occupation | initially in automatable job)	Shares of automatable hours	Difference in hours worked from t to t+1
Total routine	30%	0.92	0.86	29%	0.56
Construction	5%	0.92	0.88	4%	0.39
Manufacturing	41%	0.88	0.88	40%	0.50
Transport	22%	0.95	0.92	19%	0.67
Wholesale	26%	0.92	0.88	25%	0.49
Retail	40%	0.91	0.83	41%	0.47
Finance	39%	0.95	0.89	36%	0.43
Services	32%	0.92	0.88	29%	0.62
P. Adm.	37%	0.96	0.90	35%	0.71
Male	19%	0.91	0.87	19%	0.57
Female	51%	0.92	0.85	48%	0.54
≥ 40 years old	29%	0.89	0.86	29%	0.53
26-39 years old	28%	0.95	0.89	28%	0.61
≤ 25 years old	31%	0.88	0.79	32%	0.58
White	29%	0.92	0.87	28%	0.56
Black	31%	0.87	0.86	31%	0.59

Table 2: Full Sample Estimates, Shares of Employment in Automatable Jobs									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pooled	Construction	Manufacturing	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Dependent Variable = Share of Automatable Employment									
Log Min Wage	-0.031	0.003	-0.073	-0.052	0.025	-0.021	-0.002	-0.049	-0.013
	(0.014)	(0.018)	(0.040)	(0.025)	(0.043)	(0.023)	(0.059)	(0.035)	(0.095)
N	30963	3157	3157	3152	3147	3157	3138	3156	3060

Notes: OLS coefficient estimates of equation (3) are reported, with standard errors in parentheses. Standard errors are clustered by state. Low-skilled workers are defined as those who have a high school diploma equivalent or less. The share of automatable employment is based on equation (2), with data derived from Autor and Dorn (2013) and Autor et al. (2015). A job is classified as automatable at the three-digit occupation code level. The share of automatable employment is calculated by industry, state, and year. All regressions include area (state x urban) and year fixed effects. The minimum wage is measured in 2015 dollars (for which the average minimum wage is $6.77).

Table 3: Disaggregated Estimates, Shares of Employment in Automatable Jobs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pooled	Construction	Manufacturing	Transport	Wholesale	Retail	Finance	Services	P. Adm.
				≥ 40 Years Old					
Log Min Wage	-0.051	0.010	-0.132	-0.027	0.012	-0.073	0.049	0.011	-0.239
	(0.027)	(0.020)	(0.071)	(0.059)	(0.103)	(0.048)	(0.124)	(0.055)	(0.098)
N	30963	3157	3157	3152	3147	3157	3138	3156	3060
				26-39 Years Old					
Log Min Wage	-0.036	0.001	-0.051	-0.076	-0.006	-0.014	-0.015	-0.064	-0.097
	(0.019)	(0.025)	(0.033)	(0.043)	(0.066)	(0.044)	(0.070)	(0.047)	(0.096)
N	30963	3157	3157	3152	3147	3157	3138	3156	3060
				≤ 25 Years Old					
Log Min Wage	-0.074	0.018	-0.009	-0.098	-0.125	-0.014	-0.134	-0.088	-0.113
	(0.029)	(0.024)	(0.074)	(0.079)	(0.110)	(0.031)	(0.102)	(0.034)	(0.143)
N	30963	3157	3157	3152	3147	3157	3138	3156	3060
				Males					
Log Min Wage	0.007	-0.007	-0.046	0.006	0.042	-0.047	0.035	-0.018	0.090
	(0.016)	(0.006)	(0.034)	(0.022)	(0.045)	(0.038)	(0.091)	(0.028)	(0.072)
N	30963	3157	3157	3152	3147	3157	3138	3156	3060
				Females					
Log Min Wage	-0.078	0.067	-0.177	-0.090	0.011	-0.005	0.077	-0.080	-0.257
	(0.026)	(0.083)	(0.078)	(0.074)	(0.102)	(0.030)	(0.049)	(0.046)	(0.100)
N	30963	3157	3157	3152	3147	3157	3138	3156	3060
				White					
Log Min Wage	-0.028	-0.010	-0.065	-0.071	0.030	-0.007	0.005	-0.052	-0.110
	(0.016)	(0.020)	(0.041)	(0.033)	(0.057)	(0.033)	(0.077)	(0.036)	(0.106)
N	30963	3157	3157	3152	3141	3157	3138	3156	3150
				Black					
Log Min Wage	-0.067	0.026	-0.322	-0.316	0.080	0.139	-0.105	0.035	0.078
	(0.036)	(0.044)	(0.129)	(0.112)	(0.165)	(0.117)	(0.180)	(0.104)	(0.136)
N	22800	2273	2538	2274	1891	2730	1782	2787	2105

Notes: See notes to Table 2.

Table 4: Probability of Being Employed in the Next Period, for those Initially in Automatable Job

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pooled	Construction	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Full Sample									
Log Min Wage	-0.0010	-0.0244	-0.0048	0.0063	-0.0009	-0.0053	-0.0055	-0.0038	0.0023
x Routine	(0.0006)	(0.0101)	(0.0021)	(0.0039)	(0.0075)	(0.0042)	(0.0055)	(0.0021)	(0.0054)
N	1070647	92826	255203	71470	38970	177495	50855	258671	45706
≥ 40 Years Old									
Log Min Wage	-0.0062	0.0154	-0.0251	0.0039	-0.0104	0.0002	-0.0141	-0.0014	0.0031
x Routine	(0.0017)	(0.0141)	(0.0045)	(0.0073)	(0.0093)	(0.0043)	(0.0073)	(0.0034)	(0.0042)
N	442627	37310	113679	34030	16449	56512	22175	113640	24171
26-39 Years Old									
Log Min Wage	-0.0004	-0.0254	-0.0007	0.0174	-0.0037	-0.0016	-0.0164	0.0010	0.0043
x Routine	(0.0018)	(0.0162)	(0.0034)	(0.0093)	(0.0451)	(0.0073)	(0.0086)	(0.0053)	(0.0055)
N	372237	37251	95876	27700	14805	51022	17918	86850	15753
≤ 25 Years Old									
Log Min Wage	-0.0154	-0.0459	-0.0224	0.0061	0.0132	-0.0143	0.0082	-0.0127	-0.0031
x Routine	(0.0029)	(0.0269)	(0.0092)	(0.0214)	(0.0243)	(0.0082)	(0.0201)	(0.0087)	(0.0363)
N	255783	18265	45648	9740	7716	69961	10762	58181	5782
Males									
Log Min Wage	-0.0039	-0.0574	-0.0033	0.0127	-0.0145	0.0041	-0.0040	-0.0124	-0.0013
x Routine	(0.0021)	(0.0152)	(0.0034)	(0.0088)	(0.0111)	(0.0081)	(0.0102)	(0.0059)	(0.0072)
N	585546	86709	164507	54742	27107	81671	14970	87839	25612
Females									
Log Min Wage	-0.0028	0.0143	-0.0198	0.0072	-0.0055	-0.0141	-0.0200	-0.0025	-0.0134
x Routine	(0.0020)	(0.0262)	(0.0056)	(0.0119)	(0.0124)	(0.0059)	(0.010)	(0.0035)	(0.0114)
N	485101	6117	90696	16728	11863	95824	35885	170832	20094
White									
Log Min Wage	-0.0016	-0.0184	-0.0045	0.0132	0.0017	-0.0010	-0.0003	-0.0013	0.0024
x Routine	(0.0012)	(0.0108)	(0.0023)	(0.0105)	(0.0067)	(0.0047)	(0.0057)	(0.0032)	(0.0052)
N	919099	84306	223215	62070	35172	156556	45125	209997	36738
Black									
Log Min Wage	-0.0038	-0.0445	-0.0074	-0.0324	-0.0767	-0.0263	-0.0328	-0.0077	0.0012
x Routine	(0.0051)	(0.0693)	(0.0081)	(0.0201)	(0.0424)	(0.0202)	(0.0363)	(0.0054)	(0.0163)
N	120221	6460	25866	7870	2870	14621	4497	40118	7263

Notes: See notes to Table 2. OLS coefficient estimates of equation (3) are reported, with standard errors in parentheses. Standard errors are clustered by state. Dependent variable is equal to 1 if a person is employed in *t+1*, 0 if they nonemployed. Sample is those employed in period *t*. All regressions include state x urban x year fixed effects, and an urban dummy variable.

Table 5: Probability of Being Employed, in the Same Job, in the Next Period, for those Initially in Automatable Job									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pooled	Construction	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Full Sample									
Log Min Wage	-0.0213	-0.0197	-0.0168	-0.0323	-0.0282	0.0514	-0.0432	-0.0407	-0.0348
x Routine	(0.0015)	(0.0157)	(0.0051)	(0.0092)	(0.0129)	(0.0054)	(0.0078)	(0.0046)	(0.0071)
N	1070647	92826	255203	71470	38970	177495	50855	258671	45706
≥ 40 Years Old									
Log Min Wage	-0.0265	0.0204	-0.0194	-0.0179	0.0017	0.0284	-0.0319	-0.0301	-0.0196
x Routine	(0.0022)	(0.0223)	(0.0055)	(0.0113)	(0.0197)	(0.0087)	(0.0121)	(0.0058)	(0.0124)
N	442627	37310	113679	34030	16449	56512	22175	113640	24171
26-39 Years Old									
Log Min Wage	-0.0039	-0.0253	-0.0091	0.0174	0.016	-0.0025	-0.0165	0.0013	0.0093
x Routine	(0.0027)	(0.0163)	(0.0037)	(0.0098)	(0.0154	(0.0088)	(0.0091)	(0.0054)	(0.0082)
N	372237	37251	95876	27700	14805	51022	17918	86850	15753
≤ 25 Years Old									
Log Min Wage	-0.0468	-0.1000	-0.0019	-0.1088	-0.1352	0.0695	-0.0512	-0.0503	-0.0737
x Routine	(0.0039)	(0.0474)	(0.0121)	(0.0372)	(0.0450)	(0.0095)	(0.0458)	(0.0098)	(0.0375)
N	255783	18265	45648	9740	7716	69961	10762	58181	5782
Males									
Log Min Wage	-0.0172	-0.0126	-0.0110	-0.0174	0.0068	0.0291	-0.0950	-0.0593	-0.0573
x Routine	(0.0023)	(0.0234)	(0.0040)	(0.0159)	(0.0247)	(0.0086)	(0.0136)	(0.0111)	(0.0127)
N	585546	86709	164507	54742	27107	81671	14970	87839	25612
Females									
Log Min Wage	-0.0079	-0.1672	0.0069	-0.0767	-0.1012	0.0709	-0.0943	-0.0257	-0.1096
x Routine	(0.0022)	(0.0326)	(0.0103)	(0.0181)	(0.0416)	(0.0089)	(0.0193)	(0.0047)	(0.0127)
N	485101	6117	90696	16728	11863	95824	35885	170832	20094
White									
Log Min Wage	-0.0152	-0.0191	-0.0101	-0.0308	-0.0276	0.0559	-0.0779	-0.0456	-0.0229
x Routine	(0.0017)	(0.0160)	(0.0033)	(0.0105)	(0.0096)	(0.0063)	(0.0084)	(0.0060)	(0.0079)
N	919099	84306	223215	62070	35172	156556	45125	209997	36738
Black									
Log Min Wage	-0.0142	0.0995	0.0274	-0.0319	0.0021	-0.0093	0.0225	-0.0198	-0.0865
x Routine	(0.0050)	(0.0853)	(0.0192)	(0.0228)	(0.0544)	(0.0224)	(0.0304)	(0.0110)	(0.0335)
N	120221	6460	25866	7870	2870	14621	4497	40118	7263

Notes: See notes to Tables 2 and 4. . Dependent variable is equal to 1 if a person is employed in the same 3-digit occupation and 1-digit industry in *t*+1, and 0 if they are nonemployed or not in the same “job.”

Table 6: Probability of Being Employed in a Specific Industry in *t+1* if Employed in an Automatable Job in Period *t*

	(1)	(2)	(3)
	Retail	Services	Retail or Services
Dependent Variable = Employed in Retail/Services in *t+1*			
Include nonemployed in *t+1*			
Log Min Wage	0.0190	0.0101	0.0106
	(0.0009)	(0.0012)	(0.0010)
N	893152	811976	634481
Exclude nonemployed in *t+1*			
Log Min Wage	0.0147	0.0135	0.0129
	(0.0008)	(0.0012)	(0.0013)
N	818733	797465	545551

Notes: See notes to Table 4. Sample is the subsample of Table 4 that is employed in period *t*, but not in retail or services (or both, depending on the column). In bottom panel, those nonemployed in *t+1* are excluded. Dependent variable is equal to 1 if a person moves to the indicated industry in *t*+1, and 0 if they are continued to work in a different industry (or, in top panel, are nonemployed). For example, in the bottom panel of column (1), the sample is those employed, but not in retail, in period *t*; the dependent variable is equal to 1 if the person is employed in retail in *t+1*, and zero otherwise.

Table 7: Hours Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable = Share of Hours in Automatable Jobs								
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Log Min Wage	-0.015	-0.077	-0.006	-0.014	-0.023	-0.094	-0.013	-0.074
	(0.017)	(0.035)	(0.021)	(0.039)	(0.0016)	(0.0028)	(0.019)	(0.035)
N	30963	30963	30963	30963	30963	30963	30963	22800
	Construct	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Log Min Wage	-0.010	-0.084	-0.052	0.077	0.003	0.060	-0.018	-0.125
	(0.012)	(0.041)	(0.040)	(0.060)	(0.027)	(0.072)	(0.024)	(0.068)
N	3017	3017	3011	3000	3017	2990	3016	3006
Dependent Variable = Hours Difference from Period 1 to Period 2								
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Log Min Wage x Routine	-1.646	-2.508	-3.607	0.555	-2.669	-0.975	-2.562	-0.896
	(0.175)	(0.272)	(0.447)	(0.561)	(0.380)	(0.266)	(0.293)	(0.603)
N	696432	330014	225466	140952	384574	311858	568524	82581
	Construct	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Log Min Wage x Routine	-10.356	-3.035	-5.790	-3.096	0.022	-2.748	-1.401	-1.942
	(1.674)	(1.516)	(1.338)	(1.478)	(0.567)	(0.934)	(0.460)	(1.101)
N	77628	122638	46009	23443	138791	29655	208287	39762

Notes: See notes to Table 2. In the top panel, the share of automatable hours worked is calculated in the same manner as the share of automatable employment in Table 2. In the bottom panel, the sample only includes individuals who remained employed between the two periods, so the sample sizes are lower than for the employment regressions.

Table 8: Contemporary Analysis, 1995-2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable = Share of Employment in Automatable Jobs								
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Log Min Wage	-0.038	-0.069	-0.025	-0.050	-0.021	-0.058	-0.029	-0.030
	(0.022)	(0.034)	(0.027)	(0.037)	(0.020)	(0.034)	(0.022)	(0.059)
N	19154	11886	11860	11510	12020	11553	12025	8264
	Construct	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Log Min Wage	0.001	-0.066	-0.079	0.093	-0.024	-0.021	-0.036	-0.147
	(0.017)	(0.062)	(0.048)	(0.057)	(0.030)	(0.068)	(0.031)	(0.090)
N	1964	1964	1959	1954	1964	1945	1963	1957
Dependent Variable = Probability of Being Employed in the Current Period								
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Log Min Wage x Routine	-0.020	-0.037	-0.027	-0.008	-0.017	-0.025	-0.028	0.027
	(0.009)	(0.015)	(0.011)	(0.030)	(0.012)	(0.0013)	(0.0009)	(0.040)
N	642054	215655	299300	127095	352971	289083	537369	71820
	Construct	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Log Min Wage x Routine	-0.091	-0.067	0.027	-0.067	0.047	-0.037	-0.002	-0.012
	(0.069)	(0.029)	(0.057)	(0.048)	(0.028)	(0.032)	(0.012)	(0.037)
N	69579	114738	40614	23340	110355	32364	175239	23043
Dependent Variable = Probability of Having the Same Job in the Current Period								
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Log Min Wage x Routine	-0.042	-0.059	-0.034	-0.058	-0.018	-0.044	-0.044	-0.020
	(0.011)	(0.020)	(0.013)	(0.039)	(0.013)	(0.016)	(0.012)	(0.037)
N	642054	215655	299300	127095	352971	289083	537369	71820
	Construct	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Log Min Wage x Routine	-0.128	-0.050	0.005	0.023	0.053	-0.008	-0.176	-0.056
	(0.122)	(0.028)	(0.053)	(0.077)	(0.036)	(0.043)	(0.038)	(0.047)
N	69579	114738	40614	23340	110355	32364	175239	23043

Notes: See notes to Table 2 and 4.

Table 9: Examples of Top and Bottom Tertile Wage Occupations in Manufacturing

	Top Tertile	Bottom Tertile
1	Repairers of data processing equipment	Sawing machine operators
2	Water and sewage treatment plant operators	Assemblers of electrical equipment
3	Millwrights	Food roasting and baking machine operators
4	Supervisors of mechanics and repairers	Cooks
5	Elevator installers and repairers	Packers
6	Repairers of electrical equipment	Parking lot attendants
7	Plant and system operators, stationary engineers	Metal platers
8	Railroad conductors and yardmasters	Textile sewing machine operators
9	Electricians	Clothing pressing machine operators
10	Tool and die-makers and die-setters	Molders and casting machine operators

Table 10: Manufacturing Low-Wage versus High-Wage Occupations								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Dependent Variable = Share of Employment in Automatable Jobs								
Low-Wage								
Log Min Wage	-0.161	-0.189	-0.117	-0.131	-0.123	-0.156	-0.182	-0.443
	(0.058)	(0.087)	(0.077)	(0.146)	(0.054)	(0.093)	(0.055)	(0.145)
N	3157	3157	3157	3157	3157	3157	3157	2273
High-Wage								
Log Min Wage	-0.035	-0.080	0.015	-0.086	-0.004	-0.065	0.027	0.168
	(0.056)	(0.079)	(0.072)	(0.075)	(0.061)	(0.084)	(0.065)	(0.160)
N	3157	3157	3157	3157	3157	3157	3157	2273
Dependent Variable = Probability of Being Employed in the Current Period								
Low-Wage								
Log Min Wage	-0.014	-0.043	-0.0002	-0.035	-0.016	-0.015	-0.018	-0.009
x Routine	(0.003)	(0.005)	(0.006)	(0.010)	(0.005)	(0.006)	(0.003)	(0.009)
N	137719	47797	75558	27759	68542	69177	116763	16930
High-Wage								
Log Min Wage	0.003	-0.008	0.002	-0.024	0.007	0.014	0.004	0.010
x Routine	(0.012)	(0.024)	(0.025)	(0.075)	(0.012)	(0.021)	(0.011)	(0.041)
N	24243	12974	9624	1645	19617	4626	23140	767
Dependent Variable = Probability of Being Employed in the Same Job in the Current Period								
Low-Wage								
Log Min Wage	-0.025	-0.017	-0.028	-0.015	-0.018	-0.024	-0.013	-0.240
x Routine	(0.012)	(0.016)	(0.042)	(0.079)	(0.021)	(0.022)	(0.011)	(0.454)
N	137714	75554	47795	27759	68537	69177	116758	16930
High-Wage								
Log Min Wage	0.005	-0.001	0.005	-0.012	0.002	0.015	0.003	0.036
x Routine	(0.004)	(0.006)	(0.009)	(0.009)	(0.005)	(0.005)	(0.004)	(0.015)
N	24230	14611	7975	1644	19606	4624	23129	766

Notes: See notes to Tables 2, 3, and 4.

Table 11: Higher-Skill Workers Related to the Interaction Between Minimum Wage and the Share of Low-Skill Routine Work

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable = Probability of Being Employed in the Current Period								
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Min Wage	0.0562	0.0539	0.0980	0.1992	0.0496	0.0648	0.0351	0.0133
x Share of Low-Skill Routine Work	(0.0474)	(0.0551)	(0.0443)	(0.0958)	(0.0420)	(0.0558)	(0.0390)	(0.0934)
N	1178234	602114	576120	152538	600762	576120	981685	196549
	Construct	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Min Wage	0.8058	0.0141	0.0923	0.0351	-0.0968	-0.0365	0.0782	-0.0293
x Share of Low-Skill Routine Work	(0.6797)	(0.0946)	(0.1559)	(0.1039)	(0.1032)	(0.0420)	(0.0641)	(0.0362)
N	50495	135336	58552	37394	134000	95834	533856	77500
Dependent Variable = Probability of Having the Same Job in the Current Period								
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Min Wage	0.0151	-0.00083	0.0241	0.0225	0.0243	0.0603	0.0130	0.0293
x Share of Low-Skill Routine Work	(0.0173)	(0.0115)	(0.0248)	(0.0369)	(0.0213)	(0.0170)	(0.0178)	(0.0398)
N	1178234	602114	576120	152538	600762	576120	981685	196549
	Construct	Manu.	Transport	Wholesale	Retail	Finance	Services	P. Adm.
Min Wage	0.3163	0.0196	0.3296	-0.0048	0.0147	-0.0193	0.1308	0.1338
x Share of Low-Skill Routine Work	(0.5400)	(0.1187)	(0.1829)	(0.1417)	(0.2332)	(0.0415)	(0.0845)	(0.0462)
N	50495	135336	58552	37394	134000	95834	533856	77500

Notes: The Share of Low-Skill Routine Work is defined as the share in the individual's area, year, and industry. This share is calculated following equation (5) and matched into the dataset used for the analysis in Table 4 based on industry, area, and year. In this case the data retains higher-skill individuals only in the sample. Higher-skilled individuals are those with more than a high school degree. See also notes to Table 2.

Appendix A: Manufacturing Low-Wage Industries versus High-Wage Industries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pooled	≥ 40 Years Old	26-39 Years Old	≤ 25 Years Old	Male	Female	White	Black
Dependent Variable = Share of Employment in Automatable Jobs								
Low-Wage								
Min Wage	-0.109	-0.147	-0.091	-0.068	-0.094	-0.149	-0.128	-0.213
	(0.051)	(0.077)	(0.054)	(0.010)	(0.054)	(0.073)	(0.055)	(0.133)
N	3157	3157	3157	3157	3157	3157	3157	2273
High-Wage								
Min Wage	0.012	0.009	0.055	0.157	-0.006	-0.061	0.005	-0.101
	(0.042)	(0.068)	(0.062)	(0.084)	(0.050)	(0.064)	(0.053)	(0.124)
N	3157	3157	3157	3157	3157	3157	3157	2273
Dependent Variable = Probability of Being Employed in the Current Period								
Low-Wage								
Log Min Wage	-0.010	-0.029	-0.006	-0.035	-0.004	-0.021	-0.008	-0.010
x Routine	(0.004)	(0.006)	(0.007)	(0.012)	(0.006)	(0.008)	(0.004)	(0.012)
N	90175	48311	31037	17272	48065	42110	77096	10258
High-Wage								
Log Min Wage	0.005	-0.005	0.000	-0.011	0.005	-0.025	0.007	0.014
x Routine	(0.004)	(0.009)	(0.010)	(0.017)	(0.005)	(0.011)	(0.004)	(0.021)
N	66188	32402	23434	8968	50941	15247	57967	7216
Dependent Variable = Probability of Being Employed in the Same Job Current Period								
Low-Wage								
Log Min Wage	-0.019	-0.018	-0.013	-0.043	-0.0011	-0.025	-0.011	-0.033
x Routine	(0.005)	(0.008)	(0.012)	(0.015)	(0.007)	(0.014)	(0.005)	(0.021)
N	90167	48308	31035	17272	48058	42109	77088	10258
High-Wage								
Log Min Wage	0.002	0.003	0.002	0.002	-0.003	0.009	0.003	0.006
x Routine	(0.001)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.001)	(0.006)
N	66179	32401	23433	8968	50925	115244	57961	7214

Notes: See notes to Tables 2, 3, and 4. For each low-skill sub-industry (at the two-digit level) within manufacturing, we compute average wages from the 1980-2016 Merged Outgoing Rotation Groups of the CPS. The low-wage sub-sample is the bottom tertile of industries in this distribution, and the high-wage subsample is the top tertile.

Minimum Wage and Real Wage Inequality: Evidence from Pass-Through to Retail Prices

Justin H. Leung*

University of Chicago

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Abstract

This paper jointly considers the impact of the minimum wage on both labor and product markets using detailed store-level scanner data. I provide empirical evidence that a 10% increase in the minimum wage raises grocery store prices by 0.6%-0.8%, and suggest that the minimum wage not only raises labor costs but also affects product demand, especially in poorer regions. This points to novel channels of heterogeneity in pass-through that have distributional consequences, with key implications for real wage inequality, residential segregation, and future minimum wage increases. I also find that price rigidity within retail chains ameliorates these effects, reducing the pass-through elasticity for retail prices by about 60%.

Keywords: Minimum wage, inequality, pass-through, scanner price indices

JEL Classification Numbers: E24, E31, J31, J38

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1 Introduction

Minimum wage laws are one of the most frequently used policies to combat poverty around the world. They are a nearly universal policy instrument and applied in around 90% of all countries (ILO, 2006). However, the efficacy of minimum wage laws as an anti-poverty tool has been debated for many decades, beginning with Stigler (1946). While the minimum wage raises wages for low wage workers, less clear are who exactly pays for these increases and how much. There are three main ways through which these higher labor costs are transmitted throughout the economy. First, firms can reduce employment or adjust non-monetary returns to workers (e.g. less fringe benefits such as fewer paid lunch hours and holidays). In this case, low wage workers pay. Second, firms may reduce profits, which means that owners pay. Third, firms may raise prices, hence consumers also pay. The first mechanism has received much of the attention in the empirical literature, but the magnitude of the disemployment effect is still hotly debated.[1] Significantly less studied are the latter two mechanisms of prices and in particular, profits.[2]

This paper studies the impact of the minimum wage on retail prices for a wide range of products. To estimate the minimum wage pass-through elasticity, store and product group specific price indices are constructed using retail scanner data. I apply a standard difference-in-differences approach to exploit a large number of federal, state, and city minimum wage law changes in the US from 2006-2015 as sources of variation, covering both changes during the Great Recession and the subsequent recovery. I show that this standard identification strategy generates estimates that can be interpreted as plausibly causal because stores in different states exhibit no differential pre-trends. In the labor market, I find that a 10% minimum wage hike raises earnings of grocery store workers up to 1.5%. In the product market, I find that the impact of the minimum wage on retail prices is economically large and statistically significant in grocery stores. A 10% hike in the minimum wage raises grocery store prices by around 0.58%. These estimates are economically significant since both the national CPI and grocery store inflation rates are around 2% annually over the sample period.

Pass-through estimates for other store types such as drug and merchandise stores are statistically insignificant. I provide empirical evidence that most grocery chains either adopt regional pricing or operate only in a few states, while most drug and merchandise chains adopt rigid pricing within retail chains across the nation. Among these store types, grocery stores

[1]One side has shown no significant disemployment effect (e.g. Card and Krueger 1995; Allegretto et al. 2013) while the other side has shown significantly negative employment effects (e.g. Neumark and Wascher 2006).

[2]Draca et al. (2011) find that the minimum wage reduces firm profitability in the UK.

account for about 60% of consumer expenditure while drug and merchandise stores account for 40%.[3] I estimate that within-chain price rigidity attenuates the impact of an increase in *local* minimum wage on retail prices by 58%. These findings suggest that extrapolations from my data about the impact of a rise in the *federal* minimum wage on retail prices can only be made using grocery stores, but we cannot rule out the possibility that drug and merchandise stores would also change prices nationally across the entire retail chain in response to *federal* minimum wage hikes.

Furthermore, there is substantial heterogeneity in pass-through elasticities between stores in rich and poor counties. When focusing only on grocery stores in poor counties where the minimum wage is more binding, the estimated pass-through elasticity is larger than predicted by theory if minimum wage hikes are purely labor cost shocks. Interacting the pass-through elasticity with measures of how binding the minimum wage is within counties gives strongly significant coefficients. I propose that demand-induced feedback is one mechanism that can explain the large magnitude in pass-through elasticity as well as the dispersion between rich and poor counties. If the minimum wage generates spillover effects, a large number of workers would experience an increase in income and possibly household credit, lowering demand elasticities and raising prices as stores increase markups. This effect would be particularly strong in regions where the minimum wage is more binding. I derive pass-through formulas to predict the magnitude of the pass-through elasticity if the minimum wage only increased demand and show that theoretical calibrations are consistent with the reduced-form estimate. I also provide suggestive evidence that poorer households reduce their shopping intensities when the minimum wage rises.

The estimated pass-through elasticity passes a series of robustness checks and exhibits significant heterogeneity in ways consistent with theory. For example, the pass-through elasticities are higher for product groups with lower demand elasticities, consistent with changes in optimal markups. In addition, minimum wage pass-through elasticities vary geographically. For example, they are larger in counties with a higher proportion of low wage workers and stores with lower revenue. These results point to novel channels of spatial heterogeneity in pass-through elasticity that have distributional consequences.

My paper contributes to several strands of literature in labor economics on the minimum wage. First, the literature on the price effects of the minimum wage is surveyed in Lemos (2008), who concludes that most studies have found that a 10% US minimum wage increase raises food prices by no more than 4% and overall prices by no more than 0.4%. The literature has focused mostly on prices of food away from home since minimum wage workers

[3] These shares are calculated from the Nielsen Consumer Panel using expenditures on store types available in the Nielsen Retail Scanner.

are predominantly hired by restaurants, accounting for close to a third of all minimum wage workers.[4] Table 1 shows the share of minimum wage workers in the industries that hire them. I study specifically retail stores for two reasons. First, scanner data for goods sold in retail stores have unparalleled richness, providing information on quantities and prices on a weekly basis for over two million goods sold in over 35,000 stores across the entire US, covering a 10-year panel from 2006-2015 with over 220 minimum wage changes. This overcomes challenges in the previous literature, where prices of goods are sampled from stores and often subject to sampling error. Second, retail stores hire many minimum wage workers, accounting for over 7% of all minimum wage workers, and are a crucial part of the consumption basket, covering around 18% of all expenditure in the CPI as shown in Figure 1.[5] Poorer consumer units spend a larger share of their income on retail goods, which further magnifies the distributional effect of increasing retail prices. To my knowledge, there are two contemporaneous papers that also estimate the minimum wage pass-through elasticity using retail scanner data. Renkin et al. (2017) find smaller results with an elasticity of around 0.02 with different data, while I find that a 10% minimum wage hike raises grocery store prices by about 0.58%. Ganapati and Weaver (2017) uses the same data but also different methodology and obtain different results. I describe both of their approaches and reconcile the differences in Appendix A.

Second, there is a scant but growing literature demonstrating that the minimum wage generates spending responses of considerable magnitude. Kennan (1995) was among the first to hypothesize the existence of demand-induced feedbacks, proposing that the "Hungry Teenager Theory" could explain why estimated disemployment effects are often small and statistically insignificant. Aaronson et al. (2012) use the CEX to show that minimum wage hikes lead to increased household income and debt, consequently increasing spending on durables such as vehicles, although they are unable to find similar evidence for non-durables. Dettling and Hsu (2017) also find that minimum wage hikes increase credit supply to low-income adults, and the reduction in borrowing costs can increase disposable income by 20-110% more than the direct effect on earnings alone. Alonso (2016) argues that measurement error might explain the null results for non-durables, and shows a positively significant spending response for non-durables with the same scanner data used in this paper, although he does not focus on the impact on prices. Using a longer sample period that

[4] Aaronson (2001) uses ACCRA price indices and publicly available BLS data while Aaronson et al. (2008) use restricted CPI data on food away from home but only for a short panel from 1995-1997 and 7,500 food items across 1,000 establishments.

[5] According to the Consumption Expenditure Survey (CEX) conducted by the BLS (BLS 2015), expenditures on food at home exceeds those on food away from home, taking up around 60% of food expenditures and 10% of the overall consumer basket.

contains rich minimum wage variation and slightly different measures of real spending, I find that the results suggesting a spending response are not very robust, and argue that both labor cost shocks and higher markups could lower real spending. In addition, whether the effect of increased income among minimum wage workers who remained employed may be offset by income losses of workers who become unemployed is an empirical question. To my knowledge, this is the first paper to demonstrate that minimum wage hikes increase prices through *both* supply and demand.

Third, several recent studies have tried to jointly consider all these mechanisms to understand who pays for the minimum wage. Aaronson and French (2007) simulate a model that uses price responses in the restaurant industry to infer about the labor market structure and its implications for employment effects. My paper also investigates jointly both labor markets and product markets. These two markets do not act in isolation, and their interaction provides useful information about the impact of minimum wages. Specifically, I focus on the impact of the minimum wage on labor costs and the resulting cost pass-through in product markets. Building on cost pass-through formulas derived by Weyl and Fabinger (2013), I am able to pin down the pass-through elasticity under different assumptions about the amount of minimum wage spillovers.[6] I then use the reduced form estimates of pass-through elasticities to recover what the range of spillovers would be to justify these estimates, and find that even the largest spillover estimates in the literature cannot explain the magnitude of the estimated pass-through elasticity. This relates to a vast literature on the drivers of wage inequality in the past four decades, which has often found that declining real minimum wages had major impacts on rising wage inequality. Autor et al. (2016) (hereafter AMS) is the latest contribution to that debate, and they find substantial spillover effects and suggest that this might result from measurement error.[7] Dube (2017) also finds that minimum wages increase family incomes consistent with spillover effects. To my knowledge, my paper is the first to link the two separate strands of minimum wage literature on prices and wage inequality. I find that the reduction in real wage inequality caused by a minimum wage hike is smaller than the reduction in nominal wage inequality.

Fourth, there are a few comprehensive studies such as Harasztosi and Lindner (2015), who focus mostly on the employment and profit effects of the minimum wage in Hungary and MaCurdy (2015), who focuses on the price effects. In particular, MaCurdy (2015) uses an

[6] Minimum wage spillovers are defined as the effect of the minimum wage on the wages of workers earning above the percentiles at which the minimum wage binds.

[7] One side of the debate has attributed rising inequality to skill-biased technical change (e.g. Juhn et al. 1993, Autor et al. 2008, and Autor and Dorn 2013) while the other side has emphasized institutional factors such as the declining real minimum wage (e.g. DiNardo et al. 1996 and Lee 1999). Dustmann et al. (2009) find evidence for both, with technological change responsible for upper-tail inequality and episodic events such as supply shocks and changes in labor market institutions responsible for lower-tail inequality.

input-output model to simulate the distributional impacts of the rise in the federal minimum wage on prices under many strong assumptions, concluding that the minimum wage is more regressive than a typical sales tax. The mechanism he highlights is that low income workers tend to consume a higher share of goods produced by minimum wage labor. In this paper, I directly estimate the distributional effect of the minimum wage on prices empirically by using regional heterogeneity in wages and earnings across counties, which allows me to relax the assumptions in MaCurdy (2015). This highlights an additional mechanism that enhances the regressive nature of the minimum wage tax: Regions with more low wage workers experience larger changes in product demand since the minimum wage is more binding and affects a larger share of consumers, leading to a higher minimum wage pass-through elasticity in those places.[8]

Fifth, this paper adds to a growing macroeconomic literature on how large demand shocks can affect retail prices procyclically. For instance, Beraja et al. (2015) show that regions with larger employment declines experience lower growth in price levels. Stroebel and Vavra (2015) (hereafter SV) study how changes in housing prices affect consumer wealth and consequently retail prices by raising markups.[9] I argue that the minimum wage increases retail prices by the same mechanism, providing further evidence on how increased income among consumers may lead to lower household shopping intensities and demand elasticities, generating a procyclical natural markup. I also derive pass-through formulas to shed light on the factors that determine the size of the price response.

Sixth, this paper adds to literature that studies price rigidity within retail chains. DellaVigna and Gentzkow (2017) (hereafter DVG) find that most retail chains in the US implement uniform pricing across stores in the same chain, and show that this dampens the overall response of prices to local economic shocks using a calibration. I provide direct empirical evidence to support this claim and find that this has major policy implications, since the local price response to local minimum wage shocks is completely attenuated for stores in rigid chains.

Seventh, this paper also links to a vast literature on estimating pass-through in international economics, industrial organization, and marketing. Most of the literature has focused on the pass-through of *cost* shocks and consistently shown that cost pass-through is

[8] One limitation is that as mentioned above, scanner data do not cover the entire consumption basket, so any distributional effect found is driven by heterogeneity across income groups in consumption only for goods sold in covered retail stores. Nevertheless, the data cover a broad range of product groups across several store types. I describe this in further detail below.

[9] These papers exploit large, persistent, and unanticipated demand shocks that could potentially shift demand and lower demand elasticities, in contrast to other existing studies that find countercyclical pricing or small price responses such as Chevalier et al. (2003), Gagnon and Lopez-Salido (2014), and Cavallo et al. (2014), which exploit predictable seasonal holidays and episodic weather events.

incomplete in most markets due to markup adjustment in imperfectly competitive markets among other factors. I provide evidence that the minimum wage pass-through to retail prices consists not only of a labor *cost* shock but also a *demand* response, which raises prices further in imperfectly competitive markets. By calibrating the size of the pass-through elasticity when demand increases as a result of a minimum wage hike, I show that the demand response can generate a sizable price effect. I also show suggestive empirical evidence that multi-product retailers price strategically across product groups by raising markups in the most demand inelastic product groups.

This paper is organized as follows. I first describe the data and how I construct the price indices in Section 2. Next, I discuss my empirical strategy in Section 3. Main results are then presented in Section 4. Pass-through formulas are derived in Section 5 to shed light on the determinants of the minimum wage pass-through elasticity. Section 6 corroborates the theory by providing evidence for consumer response in shopping behavior to minimum wage hikes. Section 7 presents results on product and regional heterogeneity of the minimum wage pass-through elasticity, along with a discussion of the key policy implications of the results. Concluding remarks are offered in Section 8.

2 Data and Construction of Price Indices

In this section, I give an overview of the data used for analysis and outline the construction of the price indices.

2.1 Price Indices

2.1.1 Nielsen Retail Scanner

I use the Nielsen Retail Scanner Dataset available through a partnership between the Nielsen Company and the James M. Kilts Center for Marketing at the University of Chicago Booth School of Business.[10] The data consist of weekly pricing, volume, and store merchandising conditions generated by participating retail store point-of-sale systems across the US from 2006-2015. Data are included from approximately 35,000 participating stores and include store types such as drug, grocery, and mass merchandise stores, covering

[10]Information on access to the retail scanner data as well as the consumer panel data described below is available at http://research.chicagobooth.edu/nielsen/. Although alternate price indices released by government agencies do exist, they have limitations that render them less suitable for my analysis, especially due to sampling error. These limitations are outlined in Beraja et al. (2015). ACCRA price indices used in previous work (Aaronson 2001) that covers a wider range of goods are also problematic as illustrated in Handbury and Weinstein (2015). Therefore, this paper uses price indices constructed from micro data.

around 53-55% of national sales in food and drug stores and 32% of national sales in mass merchandise stores. The finest location of each store is given at the county level. I only use stores that appear throughout the entire sample period such that store entry and exit do not affect results. Among the stores in the sample in 2006, 84% remain throughout the entire sample period. A huge number of products from all Nielsen-tracked categories are included in the data, with 2.6 million universal product codes (UPCs) in total aggregated into around 1,100 product modules, which are further aggregated up to 125 product groups.

The advantage of using the retail scanner data as opposed to the Nielsen Consumer Panel is that a wider range of goods is observed at higher frequencies and quantities.[11] Scanner price indices are constructed as in Beraja et al. (2015). I briefly describe the approach they adopt in Appendix B and refer interested readers to their paper for details. I also construct a range of different price indices using alternative methods, which give nearly identical results.

To investigate the behavior of the constructed indices, the scanner price index is compared to the publicly available CPI series. Since the BLS only publishes local price indices for around 20 sample areas in the US, I match the available CPI price indices at the city level with the store price indices constructed by taking a sales-weighted average across stores in each available city. This leaves 16 cities that can be compared to the scanner price indices, and this is done for food, food at home, and food away from home. The indices exhibit a high correlation of around 0.75-0.8. Figure 2 shows the different indices for New York City. Plots for other cities are shown in Appendix Figure H1. Overall, the Nielsen grocery store price indices track the ones produced by the CPI food indices closely. The average annual inflation rates are around 2%, similar to the national CPI inflation over the sample period.

2.2 Nielsen Consumer Panel

The Nielsen Consumer Panel Dataset represents a longitudinal panel of approximately 40,000 to 60,000 US households from 2004 to 2015 who continually provide information to Nielsen about their households and what products they buy, as well as when and where they make purchases. Panelists use in-home scanners to record all their purchases, from any outlet, intended for personal, in-home use. Products include all Nielsen-tracked categories of food and non-food items, across all retail outlets in the US Nielsen samples all states and major markets. Panelists are geographically dispersed and demographically balanced. Each panelist is assigned a projection factor, which enables purchases to be projectable to the

[11] I also attempted to use the consumer panel, but since only a small number of goods are regularly purchased by each household, it is difficult to construct a representative regional price index of high frequency by income group. Furthermore, the consumer panel is subject to non-response bias since consumers may not always scan their purchases.

entire US.

For each period, I calculate the total expenditures of each household. The advantage of using this measure as opposed to the nominal sales for each store is that demographic information for the household can be observed instead of county-level demographics for the store. Several measures of shopping intensities among households can be constructed following SV. For each good purchased, the household records whether the good is purchased with coupons and if it is on sale. The barcode of the good is also scanned so the brand of the good can be observed. Therefore, I use three measures of shopping intensity: (1) the share of expenditures using coupons (coupon share), (2) the share of expenditures on goods that are on sale (deal share), and (3) the share of expenditures on generic store brands (store brand share).[12]

2.3 Minimum Wage Series in the US

I use the state-by-month or state-by-quarter minimum wage data in the US from 2006-2015. These data are made available by Vaghul and Zipperer (2016) and compiled from a wide variety of primary sources.[13] Results are nearly identical when accounting for local minimum wage ordinances at the city or county level. The minimum wage used is the maximum of the federal and state minimum wage, which is commonly known as the state effective minimum wage. I plot the minimum wage over time for all states from 2006-2015 in Figure 3. Note that there is quite a lot of within-state variation over the period of interest that is staggered across time for different states, providing useful variation for identification. The lower envelope is the federal minimum wage over time, since some states are consistently bound by federal changes. This implies that there is substantial variation beyond the federal changes due to state minimum wage laws. Furthermore, federal changes also provide identifying variation since some states are forced to comply with federal legislation while states with higher minimum wages are unaffected.

To get a better understanding of the frequency of minimum wage changes in the sample period, Figure 4 plots the number of minimum wage changes by year and type, pooling together all states, from 2006-2015. The figure indicates that the sample period contains quite a lot of minimum wage variation that comes in two waves across different phases of the business cycle, with mostly federal changes in the first wave in 2007-2009 and only state

[12]Results are robust using alternative measures that control for changes in the shopping bundle across product groups, e.g. by demeaning each household-product group-period observation by the mean within each product group-period observation across households, then aggregating over product groups to the household-period level with household-product group-period consumption weights.

[13]Latest version available at https://github.com/equitablegrowth/VZ_historicalminwage/releases.

changes in the second wave in 2014-2015.[14] There are 4 types of minimum wage changes: federal legislation, state legislation, state ballot (where voters decide whether the minimum wage should be increased), and subsequent changes due to indexation. This implies that the minimum wage is often raised automatically since it is linked to price indices, raising potential concerns about reverse causality. However, almost all states (with the exception of Colorado) use the national CPI for indexation, which implies that period fixed effects are a sufficient control. Results are robust to dropping these indexing states. I also report additional characteristics of the minimum wage changes in Appendix Tables G1 and G2. Minimum wages are often implemented after the announcement of the legislation with some time lag. The average implementation lag is around 4.13 quarters in the first wave and 1.80 quarters in the second wave, which is important for understanding the dynamics of price changes in response to minimum wage hikes.

2.4 Labor Market Data

The Quarterly Workforce Indicators (QWI) dataset provides labor market statistics by county, detailed industry, worker demographics, employer age and size. The QWI is the public use aggregation of the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata, which is collected via a unique federal-state data sharing collaboration and covers 95% of US private sector jobs.[15] It was first used in the minimum wage literature by Dube et al. (2013), which I follow closely to provide empirical evidence on labor market impacts of the minimum wage on retail stores. I use five dependent variables for my analysis. The first two are (1) Earnings: Average monthly earnings of employees who worked on the first day of the reference quarter and (2) Employment: The number of jobs on the last day of the reference quarter. These two variables are consistent with the more commonly used Quarterly Census of Employment and Wages (QCEW) data, which I also use for county-level characteristics such as the number of establishments.[16] The next three variables are novel worker flow variables, which include (3) Hires: The number of workers who started a new job in the reference quarter, (4) Separations: The number of workers whose job with a given employer ended in the reference quarter, and (5) Turnover: The number of hires and separations as a share of total employment, which is defined as hires

[14] Federal minimum wage changes are defined as those which were binding on states. For example, a state which had a state minimum wage lower than the new federal minimum wage would contribute to a federal minimum wage change for that state, while states with a state minimum wage already above the new federal minimum wage would not because that federal minimum wage change was not binding for those states. State minimum wage changes are defined as changes not directly caused by a binding rise in the federal minimum wage.

[15] Information on access to the data is available at http://lehd.ces.census.gov/data/.

[16] Information on access to the data is available at http://www.bls.gov/cew/data.htm.

plus separations divided by two times employment.

I also use the ACS and CPS Merged Outgoing Rotation Groups (MORG) data due to the availability of information on hourly wages, which is unfortunately not present in the QWI. Since they are frequently used in the literature, I do not describe the data here.[17]

3 Empirical Strategy

To estimate the impact of the minimum wage on prices, I apply typical panel fixed effects approaches as opposed to a pure event study methodology or synthetic control due to numerous overlapping minimum wage events in my sample. This also allows me to take advantage of variation in magnitudes of minimum wage hikes across events. While both price indices at the state and store level can be constructed, I report store-level regressions because there is more information available on store type and geographic location. In addition, I report results using quarterly price indices because labor market variables are at the quarterly level, and most pass-through literature also uses quarterly variables. In my preferred specification, the log of the scanner price index P_{it} for store i in state s and time period t is regressed on log minimum wage MW_{st} for the store-year-quarter panel with store and period fixed effects to control for unobserved store characteristics and common time trends that affect prices, as shown in equation 1:

$$\ln P_{it} = \alpha + \beta \ln MW_{st} + X'_{ct}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{1}$$

Since the level of the price index is not interpretable, only relative changes are relevant. The log-log specification gives the interpretation of β as the elasticity of prices with respect to the minimum wage, which is known as the minimum wage pass-through elasticity. The typical approach in the literature is to interpret the percentage change in price as a result of a 10% increase in the minimum wage, which is equal to 10β. This is useful since the average percentage change in minimum wage for my panel is around 8%. I also include control variables X_{ct} matched to the county c the store is located in, such as log housing price, log county unemployment rate, log county average wages and log county population.[18] These

[17] Information on the ACS is available at https://usa.ipums.org/usa/, while the CPS MORG data is available at http://www.nber.org/data/morg.html.

[18] Data on housing prices are obtained from the Federal Housing Finance Agency, which produces housing price indices at both the 3-digit zip code level as well as at the state level from 2006-2015. County housing price indices from 2006-2014 are also obtained from CoreLogic through the Fama-Miller Center at the University of Chicago Booth School of Business. Results are presented using 3-digit zip code level housing prices since they are available for the entire sample period, while results are very similar using county level housing prices whenever available. Labor force variables are obtained from the BLS and the Census.

variables have been shown to have impacts on regional prices in SV, Beraja et al. (2015), and Handbury and Weinstein (2015). Results are robust to the exclusion of control variables. Standard errors are clustered by state to allow for autocorrelation in unobservables within states since the identifying variation is at the state level, following Bertrand et al. (2004).

I compare the contemporaneous effect of the minimum wage estimated with equation 1 with the cumulative effect by using a distributed lag model as shown in equation 2:

$$\ln P_{it} = \alpha + \sum_{j=-k}^{k} \beta_j \ln MW_{s,t-j} + X'_{ct}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{2}$$

The cumulative effect is obtained by adding together all the coefficients. While the standard cumulative effect includes only the sum of the contemporaneous effect and all the lag coefficients, the lead coefficients are added as well because minimum wage changes are often announced ahead of time and there could be anticipatory changes in prices attributable to the minimum wage change. I also explored the alternative of matching the announcement date as opposed to the implementation date to the minimum wage change. More importantly, these leads provide a very useful falsification test that is common in the literature, since the minimum wage is not expected to have effects on variables of interest many quarters before implementation.

In addition, I also implement a triple differences approach by interacting the log minimum wage with several determinants of the pass-through elasticity, denoted as B_{it}, as shown below in equation 3:

$$\ln P_{it} = \alpha + \beta_1 \ln MW_{st} + \beta_2 \ln B_{it} + \beta_3 \ln MW_{st} \times \ln B_{it} + X'_{ct}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{3}$$

For example, B_{it} can be measures of how binding the minimum wage is in each county, such that county-level variation in bindingness can be used in addition to state-level variation in the minimum wage. State-period fixed effects α_{st} can also be used in this case to control for heterogeneous trends at the state level, in which case β_1 will not be identified.

To address the potential issue of heterogeneous trends, I also apply the local controls approach in Dube et al. (2010) to analyze labor markets.[19] The local controls approach is illustrated in equation 4:

[19]One problem with the local controls approach to study product markets is that if consumers travel across state borders to contiguous county stores due to the price rises from a minimum wage increase in their state, using contiguous county stores would attenuate the estimated pass-through elasticity due to price rises in those counties as well from substitution.

$$\ln P_{it} = \alpha + \beta \ln MW_{st} + X'_{ct}\gamma + \alpha_i + \alpha_{pt} + \varepsilon_{it} \tag{4}$$

α_{pt} are county-pair-period fixed effects, which means that the pass-through elasticity is identified only by the variation in minimum wage between contiguous county pairs.

4 Main Results

In this section, I present the main empirical evidence on how the minimum wage impacted retail stores, first through the labor markets, which has implications for the product markets. I give further interpretation of the results and the interaction between these two markets using theory derived in Section 5.

4.1 Labor Markets

As discussed in Section 1, there has been a back and forth debate regarding the choice of control groups used to identify the employment effect of the minimum wage. I first apply standard fixed effects as in equation 1 to estimate the minimum wage effects on earnings and employment. Results are presented for four types of retail stores that are present in scanner data that can be matched to 4-digit NAICS industries: health and personal care stores (drug stores), grocery stores, department stores, and discount stores (merchandise stores). Restaurants are also included for comparison. Table 2 reports estimates of contemporaneous minimum wage effects from 10 separate regressions, with 5 industries and 2 outcomes. To match with the product market evidence using scanner data, the sample period is chosen to be 2006-2015. A 10% minimum wage hike raises earnings of grocery store workers by around 1.5%, while effects are also present for restaurants as in previous literature and also for merchandise stores. To test for parallel trends, I also added six quarters of leads and four quarter of lags to estimate the cumulative effect, and a subset of those results is shown in Figure 5, which indicate that results are not driven by differential pre-trends. Estimates for employment are statistically insignificant.

In addition, to investigate whether the earnings effect is stronger in counties where the minimum wage is more binding, I estimate the effect separately for two samples. I use the Kaitz index, defined as the ratio of the minimum wage to the average wage, in the pre-period, i.e. first quarter of 2006, and define a county as “rich” or “poor” if it has a Kaitz index below or above median respectively. Results shown in Appendix Table G4 imply that the earnings effect is indeeds stronger in poor counties.[20] I also follow Dube et al.

[20] Using triple differences instead implies nearly identical point estimates.

(2013) and use a contiguous county sample to estimate the impact of the minimum wage on earnings, employment, hires, separations, and turnover in Appendix Table G3. In addition to replicating their results on restaurants, I also find similar results for grocery stores, although the contiguous county-pair period fixed effects attenuates the earnings effect for both grocery and merchandise stores. Overall, these results provide suggestive evidence that the minimum wage increases labor costs in grocery and merchandise stores, providing a range of estimates for the minimum wage impact on labor costs. This effect is also stronger in regions where the minimum wage is more binding. I interpret the implications of these results for product markets in Section 5.

4.2 Product Markets

I first estimate the contemporaneous effect of the minimum wage on prices using the standard fixed effects approach and store price indices as shown in equation 1. Table 3 presents the estimated pass-through elasticities for all store types with and without control variables. The point estimates are statistically significant for grocery stores with a pass-through elasticity of 0.058, while the coefficients on the control variables have signs mostly consistent with previous literature. The estimates are not statistically significant for other store types. I explain why the pass-through elasticity is large and significant in grocery stores but not in other store types in Section 4.2.2.

I test for differential pre-trends by estimating a distributed lag model for grocery stores as shown in equation 2. I choose an event window of 6 quarters before and 4 quarters after a minimum wage hike and plot both the distributed lag coefficients and the cumulative effects. This event window is long enough to show parallel pre-trends, and short enough such that little of the minimum wage variation is dropped from the sample.[21] Given that the mean announcement of a minimum wage hike is around 3.21 quarters before the implementation, Figure 6 shows suggestive evidence that prices spike mostly during the announcement of minimum wage hikes, and trended slightly upwards after these events. Renkin et al. (2017) also found announcement effects and argue that this is consistent with models of price setting with nominal rigidities. More importantly, there is no evidence that the positively significant results shown earlier are driven by differential pre-trends. Figure 7 plots the observations (collapsed into 50 bins) used to estimate the cumulative effect 6 quarters before a minimum wage change and 4 quarters after a change, showing that the slope is initially flat but becomes positive. Although the timing of the price changes is not extremely sharp, this is consistent with the fact that announcement dates vary for each minimum wage hike and the possibility

[21] A lot of the variation is in 2015q1, and the latest available minimum wage data are in 2016q3, which is 6 quarters after 2015q1.

that menu costs lead to gradual adjustment in prices.

To get a clearer picture of the timing, I first run the distributed lag model using the announced minimum wage instead of the implemented minimum wage as in Renkin et al. (2017). For announcements that cover multiple minimum wage changes, I take the maximum across all changes as the announced minimum wage. I also drop states which index their minimum wage to the national CPI since there is no exact announcement timing for these states. Figure 6b confirms that prices rise right after the announcement of minimum wage. To further address concerns of differential pre-trends, I argue that it is more reliable to draw inference from minimum wage changes for which the implementation lag is short as well as use variation outside of the recession. Therefore, I run the distributed lag within the sample period of 2013-2015, since most of these minimum wage changes have much shorter implementation lags as shown in Table G2. Figure 6c shows sharper timing and parallel trends in the entire pre-period, and a similar cumulative effect of around 0.1.

There is reason to believe that the pass-through elasticity is heterogeneous across counties, since a minimum wage hike should have a larger effect where it is more binding. I employ the triple differences approach shown in equation 3 and examine how the pass-through elasticity changes with the Kaitz index, which is fixed to its value in the first quarter of 2006. I report the results with state-period fixed effects in Table 4. The interaction coefficient is large and strongly significant. Raising the Kaitz index by 0.1 increases the pass-through elasticity by about 0.0426. This implies that within the Kaitz index distribution, moving a county from the 25th percentile value of around 0.3 to the 75th percentile of around 0.5 raises the pass-through elasticity by about 0.085. Similar to Alonso (2016), I use alternative measures of how binding the minimum wage is in each county: the log average wage, the fraction of households earning below $25,000 annually (which is the closest bracket available in the ACS to the annual income of an average minimum wage worker), and the minimum wage annual income to median household annual income ratio. All of these measures give strongly significant results with consistent magnitudes. These results imply that the minimum wage does have distributional effects by acting as a regressive tax, raising prices by larger magnitudes in poorer regions. Therefore, the increase in real wages relative to nominal wages should be smaller for poorer workers.

Alternatively, I separate the sample by how binding the minimum wage is for each store using the county-level Kaitz index. Again, I define a store as "rich" or "poor" if it resides in a county with a pre-period Kaitz index below or above median respectively.[22] Summary

[22]Results are similar using the mean across the sample period instead. Results are robust to dividing counties into 4 quartiles rather than 2 quantiles according to their Kaitz index as shown in Appendix Table G5.

statistics for these two groups of counties in the pre-period are shown in Table 5. On average, rich counties are larger in population by about 4 times, which implies that there are far more grocery stores in rich counties than in poor counties, although the number of stores per capita is actually similar in poor counties. Importantly, both groups of counties are geographically dispersed and located in almost all states, providing sufficient variation in the minimum wage. Figure 8 maps out these counties.

I estimate equation 1 separately for both groups of counties by store type and show the results in Table 6. The estimated pass-through elasticity for grocery stores in poor counties is statistically significant at the 1% level[23] and economically significant. A 10% increase in the minimum wage raises grocery store prices in those counties by 0.84%. This magnitude is slightly larger the pass-through elasticity in restaurants estimated by Aaronson et al. (2008) of 0.7%. In Figure 9, these results are graphically displayed in a plot of the residualized log price index against the residualized log minimum wage with each store-year-quarter observation collapsed into 50 bins. The slope is steeper for poor counties.[24]

To examine the large pass-through elasticity for grocery stores, I conduct several robustness checks. First, I provide estimates separately in four periods in Table 7, since there may be concern that the results are driven by heterogeneous trends during the Great Recession. Indeed, the results are strong in the 2008-2009 recessionary period but potentially even stronger in the 2014-2015 recovery period. These results indicate that the mechanism inducing a high pass-through persists across different phases of the business cycle.

In Table 8, I estimate the pass-through elasticity by using minimum wage levels instead of logs,[25] dropping stores with average price indices below the 5th percentile or above the 95th percentile, monthly instead of quarterly observations, states that do not index the minimum wage to the CPI, counties that are interior or contiguous to state borders, accounting for local minimum wage ordinances, weighting observations by store sales or county population, aggregating store price indices to the county level using store sales before weighting the observations by county sales, and adding store-specific time trends. The point estimates remain roughly similar and statistically significant. In Table 9, I construct price indices using alternative methods as illustrated in Appendix B and show that my results are robust. I also show additional results using only city and county level minimum wage variation in Appendix C.

[23]The p-value is 0.0002, while the number of clusters remains high at 41. Even if a Bonferroni adjustment is made for multiple hypothesis testing across 6 independent hypotheses, the result is still statistically significant at the 1% level since $0.01/6 = 0.0017$.

[24]I also plot similar figures for drug and merchandise stores in Appendix Figure H3.

[25]The coefficient implies that a \$1 increase in minimum wage increases prices by 0.85%, which is in line with a pass-through elasticity of 0.058 since the federal minimum wage is \$7.25.

4.2.1 Sales and Results by Product Department

Next, I present results on the response of real and nominal sales to the minimum wage. While the theoretical prediction for the sign of the quantity response is ambiguous due to the interaction between supply and demand, an empirical estimate may provide a useful test of whether demand effects are also at work. Alonso (2016) finds that real sales, defined as sales with prices fixed to a particular time period, increases in response to minimum wage hikes. I present alternative results by defining real sales as nominal sales divided by the store specific price index and using this as a measure of quantities for an additional two years of data.[26] Table 10 shows the effect of the minimum wage on nominal and real sales for all store types with the sample again segmented by rich and poor counties. While both the nominal and real sales response are positive and higher in poor counties for grocery stores, only the nominal sales response is marginally statistically significant. The nominal and real sales response in poor merchandise stores is large and statistically significant, which is suggestive of demand effects given arguments below in Section 5. Overall, these coefficients are slightly smaller than those found in Alonso (2016), and the somewhat large standard errors make it difficult to draw any conclusions from these results about the interaction of supply and demand effects.[27]

Furthermore, I also construct price indices by product department as classified by Nielsen to understand how price and real sales responses differ across product departments as shown in Table 11, which presents results for grocery stores in poor counties. [28] Most of the results are driven by food, which generates most of the revenue in grocery stores in the sample, as prices and quantities both have positive responses, although the quantity response is marginally insignificant. There is a large price response in non-food grocery while the quantity response is negative and insignificant, and alcoholic beverages also experience a statistically significant increase in price.

4.2.2 Impact of Within-Chain Price Rigidity

To understand why the pass-through elasticity estimates are heterogeneous across store types, I first show the proportion of revenue generated by each of five product departments across store types in Table G6. Drug stores earn most of their revenue from health and

[26] Using sales of goods actually used to construct the price index gives very similar results.

[27] Deseasonalizing sales (and prices) gives almost identical results. I attempted two methods, using store-quarter-of-the-year fixed effects and the X-13ARIMA-SEATS Seasonal Adjustment Program available through the Census Bureau, both of which give similar results.

[28] I report additional results by store type in Appendix Tables G7, G8, and G9. There is evidence that prices and quantities increased in certain product departments in mass merchandise stores and the results are statistically significant.

beauty care products, while both grocery and merchandise stores earn most of their revenue from food, although grocery stores earn a lot more from food at around 77%. Combining these facts with the results in Section 4.2.1 provides a potential explanation for why other store types have statistically insignificant pass-through elasticities. If consumers respond to income increases mostly by changing demand for products such as food but not other types of products, then there would be smaller price responses in drug and merchandise stores, both of which do not derive the majority of their revenue from selling food.

Next, I follow DVG to measure the extent of price rigidity for each of the retail chains in the data. First, I pick the top UPC from each of 12 product modules with high revenue: canned soup, cat food, chocolate, coffee, cookies, carbonated soft drinks, yogurt, orange juice, bleach, toilet tissue, paper towel, and tooth cleaners. Next, I calculate the weekly correlation in prices for each store pair as a similarity measure, first demeaning the price at the store-quarter-product level before calculating the correlations over all weeks and products which are not missing both store pairs. For each chain, I define the flexibility measure as the percentage difference between the average correlation for store pairs within the same state and the average correlation for store pairs across different states. A chain is pricing more rigidly if the flexibility measure is closer to zero. Since there are multiple product modules, I take either the mean or median flexibility measure across product modules for each chain. DVG perform the same exercise using an alternative similarity measure.

The pass-through elasticity estimated from state minimum wage shocks should be affected by the extent to which chains are pricing rigidly. Chains that are located primarily in one state should also exhibit local pricing and react to local shocks, while chains that price rigidly and locate across many states should exhibit national pricing and will not react to local shocks. I plot the distribution of flexibility measures as well as number of states each chain is in across stores by store type in Figure 10. Both drug and merchandise stores belong to a few large chains that price rigidly, while a large amount of grocery stores belong to chains that price flexibly or chains that are located only in a few states. There are over 50 grocery chains while both drug and merchandise stores come from around 5 retail chains each.[29] This implies that most grocery stores are engaging in local pricing while drug and merchandise stores are not.

In Table 12, I show that the estimated pass-through elasticity decreases with the number of states the chain is in and increases for more flexible chains by interacting the minimum wage with these two variables in a sample with all store types. This is consistent with

[29]Data from the Economic Census also show that the grocery store industry is much less concentrated and less dominated by chains than the drug and merchandise store industries. For example, market share of the 4 largest firms is 30.7%, 54.4%, and 73.2% for grocery stores, drug stores, and merchandise stores respectively in 2007.

the previous finding that pass-through elasticity is small and insignificant in drug and merchandise stores but large and significant in grocery stores. Furthermore, I divide the grocery store sample into stores with local pricing and those without. I define stores as local pricing if it has a flexibility measure above or at the median, or if it belongs to a chain that is located in one or two states. I include chains with two states because almost all such chains earn over 90% of their revenue from one state. The minimum wage pass-through elasticity increases from 0.058 in the full sample to 0.083 in the flexible sample and the 95% confidence interval rules out estimates below 0.03. On the other hand, the estimate in the rigid sample is small, insignificant, and statistically different from the estimate in the flexible sample.[30] This further substantiates the claim that price rigidity, rather than difference in the type of goods sold, completely attenuates the effect of the minimum wage on prices for stores in rigid chains.

Overall, I conclude that a 10% increase in the minimum wage raises prices in grocery stores by about 0.6-0.8% but not in other types of stores because of within-chain price rigidity, and the response is statistically significant and larger in poorer counties. There is no clear evidence for changes in quantities sold. Results are not driven by differential pre-trends and pass a variety of robustness checks. I further interpret the results with theory derived in the next section.

5 Theory

To understand the determinants of the minimum wage pass-through elasticity and provide estimates based on theory to compare with the reduced-form empirical estimates, I first derive the *cost* pass-through elasticity under a range of assumptions about the degree of competition in the product market. This framework holds the demand side *fixed* and assumes the minimum wage only raises labor costs. I show that cost pass-through theory cannot fully explain my empirical findings, suggesting that demand side effects are also needed to explain the results. I then derive the *demand* pass-through elasticity under a range of assumptions about the degree of competition in the product market, holding the supply side *fixed* and assuming that the minimum wage only raises household income. I show that the theoretically calibrated magnitude of the demand pass-through elasticity is consistent with my reduced-form estimate.

[30] In addition, all results are robust to adding as a control variable the revenue-weighted average of minimum wages for stores in the same chain in other states.

5.1 Cost Pass-Through Elasticity

I first assume that the minimum wage only affects labor costs. I use unit-tax pass-through derivations from Weyl and Fabinger (2013), and convert them to minimum wage pass-through elasticities. The derivation is shown in Appendix D.1, which shows that multiplying the pass-through rate $\frac{dp}{dt}$ by the cost share of minimum wage labor s_{L_1} gives the minimum wage pass-through elasticity $\frac{\partial \ln p}{\partial \ln w_1}$. These formulas, under perfect competition, monopoly, and asymmetric imperfect competition, are presented below in equations 5, 6, and 7, respectively:

Perfect competition

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{s_{L_1}}{1 + \frac{\varepsilon_D}{\varepsilon_S}} \tag{5}$$

Monopoly

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{s_{L_1}}{1 + \frac{\varepsilon_D - 1}{\varepsilon_S} + \frac{1}{\varepsilon_{ms}}} \tag{6}$$

Symmetric imperfect competition

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{s_{L_1}}{1 + \frac{\theta}{\varepsilon_\theta} + \frac{\varepsilon_D - \theta}{\varepsilon_S} + \frac{\theta}{\varepsilon_{ms}}} \tag{7}$$

Under perfect competition, the pass-through elasticity depends only on the cost share, demand elasticity ε_D, and supply elasticity ε_S. If I further assume a constant returns to scale production function, the supply elasticity is infinite and the pass-through elasticity is simply the cost share. Under monopoly, an additional term ε_{ms} determines the pass-through elasticity. $ms \equiv -p'q$ is the marginal consumer surplus, so $\varepsilon_{ms} = ms/ms'q$ depends on the curvature of demand. Under symmetric imperfect competition, the pass-through elasticity also depends on the market-conduct parameter $\theta = \frac{p-mc}{p}\varepsilon_D$, which is the elasticity-adjusted Lerner index and is commonly used to measure the degree of competition. Furthermore, if market conduct varies with quantity, pass-through elasticity would depend on the elasticity of market conduct with respect to quantity. For example, pass-through elasticity is smaller if higher prices create more competitive conduct ($\varepsilon_\theta > 0$).

These formulas have several interesting empirical implications. First, pass-through elasticity should vary by store type because the minimum wage labor cost share varies by store type. Store types that have higher minimum wage labor cost share should have higher pass-through elasticities. Second, pass-through elasticity should vary by product group as well because the minimum wage labor content, elasticities of supply and demand, and the super-elasticities vary by product group, even within the same store. Product groups with lower elasticities of demand should have higher pass-through elasticities. Third, the competitive environment each store faces determines market conduct and impacts the pass-through elasticity, which implies that the pass-through elasticity should vary across locations with different degrees of competition, holding store type and product group constant.[31] Fourth, how income relates to the pass-through elasticity is ambiguous. On the one hand, individuals in poorer locations would probably face higher pass-through elasticities because more minimum wage workers live there, but on the other hand, lower income of most customers could possibly raise elasticities of demand, lowering the pass-through elasticity. MaCurdy (2015)'s result that the minimum wage is a regressive tax is driven by the fact that low income workers tend to consume a higher share of goods produced by minimum wage labor.[32] These formulas point to several additional determinants of the pass-through elasticity that drive distributional effects: which type of store a consumer shops in, the demographic characteristics of the consumers that shop there, and the competitive environment the store faces are all important factors to consider.[33]

To obtain some back-of-the-envelope estimates of the pass-through elasticity, I first obtain estimates of the pass-through rate from the existing literature. Nakamura and Zerom (2010) show that for coffee, the wholesale cost pass-through elasticity for retailers is 96% and incomplete pass-through of commodity prices is mostly driven by local costs and markup adjustment at the wholesale level, while Besanko et al. (2005) show that wholesale cost pass-through elasticity is more than 60% for 9 of 11 product categories, and the median wholesale cost pass-through elasticity for the 11 product categories they investigate is 83%. Based on the Annual Retail Trade Survey conducted by the US Census Bureau, the cost of goods sold as a percentage of sales, or the wholesale cost share, is around 73% for grocery stores, so the pass-through rate is around 80-100%. MaCurdy (2015) assumes complete pass-through for all industries.[34]

[31]However, many of these economic parameters, such as supply and demand elasticities as well as market conduct, are not easy to estimate, although I attempt to obtain some proxies for them.

[32]Although I can attempt to test that within the smaller range of goods available, it is very difficult to accurately match product groups with their minimum wage labor cost share.

[33]Future work could consider several extensions of these pass-through elasticity formulas. First, it is not clear how the labor market structure would impact the pass-through elasticity. For example, Aaronson and French (2007) point out that the pass-through elasticity is negative under labor market monopsony when the minimum wage lies within a certain range, but could be positive under monopsony outside that range. The pass-through elasticity formula derived only requires Shepherd's Lemma to hold. Second, the fact that sellers are multi-product retailers may imply that pass-through elasticities between products are interdependent, such that cross-price elasticities might be determinants of the pass-through elasticity.

[34]One interesting point to note is that there could be regional heterogeneity in the pass-through rate. To my knowledge, there does not seem to be any empirical evidence documenting this. However, theory suggests that the pass-through rate should be lower in poor regions such that heterogeneity in pass-through rate should not be the explanation for my findings. First, demand elasticities are generally larger in poor regions, which lowers the pass-through rate. Second, market conduct is usually lower in poor regions, which

I show the theoretical estimates of pass-through elasticity in Table 13 for different industries under different assumptions using data from the ACS. I use the ACS rather than the CPS because the ACS contains about 30 times more observations, which allows me to segment the population by the Kaitz index in their county of residence.[35] First, I assume no spillover effects. In this case, the minimum wage pass-through elasticity equals the minimum wage cost share, which is obtained by multiplying the pass-through rate with the payroll ratio (cost share of all labor, aka labor cost share) and the proportion of wages paid to minimum wage workers.[36] The first three industries are the store types available in the scanner data, while I also include restaurants because this industry has been extensively studied in the literature and is useful as a comparison. The estimates I obtain for restaurants are very similar to those obtained by Aaronson et al. (2008), who also obtain theoretical estimates in a similar fashion and show that their empirical estimates are almost identical, suggesting complete pass-through and perfect competition in labor markets. However, they do not account for spillover effects. There are a few points worth highlighting. First, the labor cost share is relatively low for retail stores at around 10%. Second, this implies that the estimated pass-through elasticity for retail stores is over 5 times smaller than that for restaurants. For instance, the estimated pass-through elasticity is 0.0082 for grocery stores but 0.045 for restaurants.

Second, the minimum wage could raise wages for workers earning above the minimum wage. AMS find spillovers up to approximately the 15th percentile of the wage distribution, which are already smaller than those estimated in previous work such as Lee (1999) (hereafter Lee). However, the minimum wage is never nominally binding above the 10th percentile for their sample period, and never binding above the 6th percentile for my sample period. Previous literature has often accounted for spillovers in an ad hoc manner by including the earnings of workers with wages some percentage above the minimum wage into the minimum wage labor cost share. I derive a reduced form way of incorporating existing estimates of spillover into the pass-through elasticity formula in Appendix D.2. The relevant formula is given in equation 8:

again lowers the pass-through rate. This holds unless the demand curve is very convex. Third, there is no clear prior on how the curvature of demand or supply elasticity should vary across regions.

[35] Results for the full sample were roughly similar using the CPS as shown in Appendix Table G10. There is no information on county of residence for some observations in the ACS probably due to confidentiality reasons. I can only use the observations for which the county of residence is known.

[36] The first term is obtained from averaging the ratio of total payroll to total receipts in 2007 and 2012 from the Statistics of US Businesses (SUSB). This payroll ratio is not available annually because it is obtained from the economic census, which is conducted every 5 years. The payroll ratio is very stable across the 2 periods. The second term is obtained by taking the ratio of total wages earned by workers earning at or below the minimum wage to total wages earned by all workers in a given year for the chosen industry.

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{dp}{dt} \frac{\sum_{i=1}^{n} w_i L_i}{pY} \sum_{i=1}^{n} s_i \varepsilon_{w_i,w_1} \tag{8}$$

There are n types of workers, and minimum wage workers are denoted as $i = 1$. The minimum wage pass-through elasticity $\frac{\partial \ln p}{\partial \ln w_1}$ equals the pass-through rate $\frac{dp}{dt}$ multiplied by the payroll ratio $\frac{\sum_{i=1}^{n} w_i L_i}{pY}$ and a weighted sum of the shares of wages earned by workers impacted by the minimum wage s_i, where the weights are given by the spillover elasticities ε_{w_i,w_1} for each type of worker. By representing each worker type by each percentile on the national wage distribution, these spillover elasticities can be obtained from estimates in AMS and Lee.[37] To obtain the share of wages earned by each percentile for each industry, I use the ACS to obtain percentiles in the national wage distribution as opposed to the within-industry distribution because the estimates from AMS and Lee are relevant for the national wage distribution.[38]

Using the procedure described above, I provide evidence suggesting that the minimum wage pass-through elasticity is not driven purely by supply shocks. Even if I use the largest estimates of spillover effects estimated in Lee, the calculated pass-through elasticity is much smaller than the empirical estimate I obtain for grocery stores in poor counties. The smallest lower bound of the 95% confidence interval of my estimates is around 0.035, whereas the largest estimate of cost pass-through elasticity is 0.024. Another important fact that emerges from Table 13 is that pass-through elasticities are not that different for poor counties if the minimum wage hike only impacted costs. In theory, it is possible that the minimum wage labor cost share is much higher in poor counties because more of the grocery workers are minimum wage workers. However, the data suggest that this is not the case. This can be easily reconciled if the share of workers in low wage industries is what determines the Kaitz index in each county rather than the within-industry share.[39]

Alternatively, another way to measure the spillover effects would be to use the QWI wage data and compare to the CPS data used in previous literature. The advantage of this

[37] I normalize the maximum elasticity among all percentiles to 1, and scale the other estimates proportionally. This implicitly assumes that the minimum wage binds at the percentile at which the spillover elasticity is highest, and implies that the relative rate of attenuation in spillover effects as one moves up the wage distribution is used, rather than the absolute magnitudes. If the absolute magnitudes are used without scaling, the shares obtained are even smaller than those obtained without assuming spillover effects. By scaling them up, I obtain an upper bound for what the pass-through elasticity would be if a minimum wage hike is a pure cost shock with spillovers.

[38] To calculate the national wage percentiles, I had to drop observations earning far below the minimum wage due to measurement error. However, I retain these observations in calculating the shares. This implies that the estimate I obtain is likely an upper bound.

[39] One caveat about the method used to derive the pass-through elasticity with spillovers is that it assumes that the spillover effect estimated with the national wage distribution applies to workers in these specific industries. This is required because there are no empirical estimates of the spillover effect within industries.

source of data is that earnings are not self-reported and derived directly from administrative records, such that they are less subject to measurement error. However, the disadvantage is that assumptions about the hours elasticity with respect to the minimum wage need to be made. Furthermore, the QWI only provides average wages, so that the LEHD restricted-use microdata would be more suited for the analysis. Nevertheless, the earnings elasticity provides useful information about spillover effects. I derive an expression for the earnings elasticity in equation 9. The derivation is shown in Appendix D.3.

$$\varepsilon_{\overline{w},w_1} = \sum_i s_i \varepsilon_{w_i,w_1} \tag{9}$$

For simplification and based on previous literature, I assume the hours elasticity ε_{h_i,w_i} is zero.[40] This expression is the weighted sum of shares earned by minimum wage affected workers shown in equation 8, providing an alternative way to obtain the pass-through elasticity. I use this formula to interpret the reduced form estimates of earnings elasticity in Section 4.1, and I also use further reduced form estimates by Kaitz index in Appendix Table G4. The earnings elasticity estimate using standard fixed effects is 0.117 and 0.184 for rich and poor counties respectively, and the 95% upper confidence bounds do not exceed 0.28. Theoretically, together with the fact that the labor cost share is 10% as mentioned above, this implies a pass-through elasticity of 0.028, lower than the 95% lower confidence bound of 0.035 for the reduced form estimate in Section 4.2. Therefore, I conclude that cost pass-through theory cannot explain the large magnitude of the minimum wage pass-through elasticity estimate or the dispersion in the estimate between rich and poor counties, suggesting that demand side effects play an important role.

5.2 Demand Pass-Through Elasticity

I now assume that the minimum wage only affects household income. Let market demand $Q^D(p, I)$ depend on price p and income I, and now assume instead that the minimum wage w_1 has an effect on demand only through income. Under perfect competition, I can obtain the demand pass-through elasticity by differentiating the equilibrium condition with respect to the minimum wage:

[40] This is a reasonable assumption given the paucity of estimates on the hours elasticity. Dube et al. (2007) shows that the hours elasticity is not statistically significantly different from zero.

$$
\begin{aligned}
Q^D(p, I) &= Q^S(p) \\
\frac{d\ln p}{d\ln w_1} &= \frac{\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1}}{1-\frac{\varepsilon_S}{\varepsilon_D}}.
\end{aligned} \tag{10}
$$

I show all steps of the derivations in Appendix Section D.4. The pass-through elasticity is now equal to $\frac{1}{1-\frac{\varepsilon_S}{\varepsilon_D}}$, which is 1 minus the pass-through rate , multiplied by the income elasticity of demand $\varepsilon_{Q^D,I}$, the inverse demand elasticity, and the income elasticity with respect to the minimum wage ε_{I,w_1}. Likewise, I can derive the demand pass-through formula under symmetric imperfect competition. First, I start from the profit-maximization condition and differentiate it with respect to income. In addition, I allow the demand elasticity to depend on income. I obtain an expression for the quantity response to income:

$$
\begin{aligned}
P(Q, I) + \theta\frac{\partial P(Q, I)}{\partial Q}Q - c'(Q) = 0 \\
\frac{dQ}{dI} = -\frac{\theta\frac{\partial^2 p}{\partial I\partial Q}Q + \frac{\partial p}{\partial I}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}.
\end{aligned} \tag{11}
$$

Next, I use to above expression to obtain the pass-through formula:

$$
\begin{aligned}
\frac{dp}{dw_1} &= \frac{\partial p}{\partial Q}\frac{dQ}{dI}\frac{dI}{dw_1} + \frac{\partial p}{\partial I}\frac{dI}{dw_1} \\
\varepsilon_\rho \equiv \frac{d\ln p}{d\ln w_1} &= \left(1 - \frac{dp}{dt}\right)\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1} + \left(\frac{dp}{dt}\right)\frac{\theta}{-\varepsilon_D}\varepsilon_{p',I}\varepsilon_{I,w_1}.
\end{aligned} \tag{12}
$$

There are two terms through which demand side effects could raise prices. First, if the minimum wage increases product demand among consumers due to increased income, a demand shift could raise prices if the pass-through rate is positive. This first term, which I denote as the shift effect, is the same as the term under perfect competition, except the pass-through rate is different because the market structure is different. Second, income could have a direct effect on demand elasticity. I denote this second term as the slope effect. These two terms imply that retailers could raise prices by raising markups even if marginal costs are flat. According to SV, marginal costs of retailers are not very responsive to local demand shocks. Around 75% of costs in a retail store are wholesale costs, which exhibit little geographic variation due to the tradable nature of retail goods as well as restrictions imposed by the Robinson-Patman Act. Almost all remaining costs are retail rents and labor costs, and SV show that these two components do not respond strongly to local demand shocks. Therefore, if the supply elasticity is large, the pass-through rate would be driven by both a positive market-conduct parameter and curvature in demand.

In Table 14, I theoretically calibrate the magnitude of the minimum wage pass-through elasticity assuming only demand effects using equation 20. I focus on the shift effect because the slope effect requires an estimate of the super-elasticity that is rarely estimated in the previous literature. I focus on food-at-home for these estimates since grocery stores derive over 75% of their revenue from food-at-home and estimates of MPC for food-at-home are most readily available. As summarized in Hoynes and Schanzenbach (2009), MPC estimates for food-at-home are around 0.03-0.17. I use the lower range of these estimates to calculate the income elasticities of demand for food-at-home, which are around 0.03-0.1 given an expenditure share of 0.1 for food-at-home. The demand elasticity estimate is obtained from Leung and Seo (2018) and consistent with Andreyeva et al. (2010). The pass-through rate estimate is also estimated in Leung and Seo (2018) and but slightly higher than those in Besanko et al. (2005) due to a high estimated curvature of demand. The income elasticity of minimum wage is calculated using derivations in Section D.3 and estimates from Dube (2017). The calibrated magnitudes show that the shift term of the demand effect alone can generate pass-through elasticities of about 0.02-0.06. Given differences in the income elasticity of the minimum wage across rich and poor counties, the magnitude of heterogeneity in my reduced-form estimates of pass-through elasticities are also in line with the heterogeneity in these theoretical calibrations. I also provide suggestive evidence for decreases in demand elasticities in Sections 6 and 7.1. First, I show that poorer households decrease shopping intensities when the minimum wage rises. Second, I find that product groups with lower demand elasticities have higher pass-through elasticities as consistent with the theory.

Overall, there are three mechanisms through which the minimum wage could impact prices and quantities sold in product markets. First, labor cost increases and shifts the supply curve upwards, raising prices and decreasing quantities sold. Second, consumer demand could shift outwards and raise quantities sold. Third, consumer demand elasticities could drop, leading retailers to markup retail prices and decrease quantities sold. The combined effects of these three mechanisms raise retail prices while the impact on quantities sold is ambiguous. Thee fact the effect of the minimum wage on real sales in merchandise stores is positively significant, as shown in Table 10, is suggestive of demand effects. Since merchandise stores price nationally, quantities will not decrease as much as grocery stores as a result of increased markups, while many of the products sold in merchandise stores, in particular food, are also sold in grocery stores. Out of the three mechanisms described above, only a demand shift can raise quantities sold.

However, it is important to note that demand-induced feedback is only one of several mechanisms that could potentially increase prices beyond the magnitude predicted under complete labor cost pass-through. First, minimum wage increases could also increase the labor costs of manufacturing goods sold in the retail sector. I argue that this mechanism is unlikely to be driving the results for two reasons. First, the share of minimum wage workers in manufacturing is very low based on the CPS at around 0.1%. Second, these goods are tradable such that most are not sold in the states in which they are produced. Another mechanism is that consumers could be substituting to food-at-home as prices for food-away-from-home rises, increasing the demand for groceries and possibly lowering the demand elasticity as well. The results illustrated above are also consistent with this mechanism. Quantifying the magnitude of this mechanism would require estimates of the cross-price elasticity of food-at-home with respect to food-away-from-home. Although there is no clear consensus on the magnitude of this elasticity, Richards and Mancino (2014) estimate a cross-price elasticity of food-at-home with respect to fast food prices at around only 0.06. Given existing estimates of the minimum wage pass-through to restaurant prices are around 0.07, a cross-price elasticity at least an order of magnitude larger would be needed to explain the real sales responses I find.[41]

6 Shopping Behavior

To provide evidence that stores raised markups due to lower demand elasticities among households in response to the minimum wage, I investigate the shopping behavior of households in response to the minimum wage using the Nielsen Consumer Panel. As shown below in equation 13, I regress shopping outcome Y_{it} for household i at time t on the minimum wage MW_{st} corresponding to the state s that household i lives in. I split the households into 2 quantiles based on their household income in 2006, denoting poorer households with the indicator variable $\mathbb{1}\{HHIncome_i \in LowerQuantile\}$, and interact the indicator with the minimum wage. This provides a measure for whether a household is affected by the minimum wage, since spillovers have effects up to around the 15th percentile of the wage distribution.[42] Control variables X_{it} such as the county level variables used in previous regressions are included along with household size fixed effects as well as household and period fixed effects.

[41] To my knowledge, there are no estimates of the elasticity of the demand elasticity for food-at-home with respect to food-away-from-home prices or food-at-home prices, which is a measure of curvature.

[42] The data only contain household income and does not contain wage information.

$$
\begin{aligned}
\ln Y_{it} = \alpha &+ \beta_1 \ln MW_{st} + \beta_2 \mathbb{1}\{HHIncome_i \in LowerQuantile\} \\
&+ \beta_3 \ln MW_{st} \times \mathbb{1}\{HHIncome_i \in LowerQuantile\} \\
&+ X_{it}'\gamma + \alpha_i + \alpha_t + \varepsilon_{it}
\end{aligned} \tag{13}
$$

Results are shown in Table 15 with and without control variables. The coefficient on the interaction between the minimum wage and log expenditure is negative although it is statistically insignificant. A priori, it seems reasonable to believe that expenditures should rise for poorer households but this is theoretically ambiguous as shown in the previous section. Furthermore, store level results do not show strong effects of the minimum wage on nominal sales. However, the coefficient on all 3 measures of shopping intensity are strongly significant, suggesting that compared to richer households, poorer households may not spend more as the minimum wage rises but they reduce their shopping intensity and become less price sensitive. There may be a concern that some of the estimated effects on below median households are positive in magnitude. This could be consistent with poorer households adjusting their shopping behavior due to increased prices. I argue that it is the difference in shopping intensities between the rich and poor households in response to minimum wage changes that suggests demand effects. I show robustness checks following SV in Appendix Table G11 by applying state-period fixed effects, interacting household characteristics with the log minimum wage, and adding product department-period fixed effects to a sample with household expenditures at the product department level. The positively significant results for the rich in coupon share and store brand share suggests that since richer households do not experience increased wages from the minimum wage hike but face higher prices, they raise their shopping intensities. On the other hand, poorer households might face even larger increases in prices, but their shopping intensities actually do not rise or even decrease because of increased income.

7 Heterogeneity and Policy Implications

7.1 Product Heterogeneity

To offer further evidence of heterogeneity in pass-through elasticities and support for increased markups, I use the components of the store price indices, which are constructed by product group, and estimate the regression shown in equation 1 to obtain pass-through elasticities by product group and store type for both rich and poor counties. These estimates can be used in a second stage regression and regressed on the determinants of the pass-through elasticity such as demand elasticities by product group. As suggested by Lewis and Linzer (2005),[43] I present both OLS results with heteroskedasticity robust errors as well as weighted least squares, where the weight is the inverse standard error of the estimates, in Table 16. I first regress the pass-through elasticities on indicators for store type. The store type indicators are positively significant with drug stores as the omitted group, which corroborates with previous evidence. I also include as a regressor the log of the absolute value of the estimated demand elasticities for each product group, which are obtained from Broda and Weinstein (2010). The coefficient is negative and significant in the OLS specification for poor counties, which implies that when demand is more inelastic, the pass-through elasticity is higher. This is consistent with supply side effects as shown by cost pass-through theory as well as demand side effects due to variable markups.[44] Lastly, I add the expenditure share in 2006 accounted for by households with below median household income in each product group according to the consumer panel. There are two opposing effects that the expenditure share proxies for. First, groups with more expenditure by the poor should have larger changes in demand elasticity since the minimum wage mostly affects those consumers and hence a higher pass-through elasticity. Second, groups with higher expenditure share might have lower initial levels of demand elasticities, which lowers the pass-through elasticity. The estimated coefficient is positive but insignificant. There is also very little variation in the expenditure share across product groups, which may explain the insignificant result. I present additional results by constructing store price indices segmented by two product characteristics, and describe these results in Appendix Section E.

7.2 Regional Heterogeneity

As analyzed in MaCurdy (2015), pass-through elasticities can vary by income group because of heterogeneity in consumption bundles across income groups. Results in Section 4.2 highlight regional heterogeneity as another mechanism that has not been emphasized in the previous literature. In Table 4, I also show the interaction using store sales, since smaller stores are more likely to be located in poorer regions and hire lower wage workers. The coefficient shows the expected sign and is strongly significant.

Furthermore, according to the cost pass-through elasticity formulas, increased competition should lead to a higher pass-through elasticity in general, since firms in more competitive environments cannot adjust their markups. However, the demand pass-through

[43] Lewis and Linzer (2005) argue that for estimated dependent variable models, using OLS with White errors is preferred to the WLS with inverse standard errors as weights in most cases.

[44] The standard markup formula has a positive second derivative with respect to the demand elasticity, which implies that for the same percentage drop in demand elasticity, a smaller initial demand elasticity creates a larger percentage increase in price.

elasticity derivations show that stores facing stronger competition are also more constrained in raising prices when demand elasticities change due to smaller markup adjustments. I interact the pass-through elasticities with a rough proxy for competition in the last specification of Table 4 using the number of grocery stores per population in each county. The coefficient is statistically insignificant, suggesting that these two forces roughly offset each other such that changing competition does not change the pass-through elasticity.

7.3 Policy Implications

Overall, these results have several interesting and important implications. First, while the existing literature has found that the minimum wage reduces nominal wage inequality, the reduction in real wage inequality is less substantial. For example, AMS find that for a 10% increase in the minimum wage, a worker earning the 10th percentile of the national wage distribution experiences around a 1.6% increase in wages relative to the median. Extrapolating earlier findings to the entire consumer basket, with caveats that other products may have smaller responses due to smaller MPCs and rigid pricing in national chains, the increase in real wages would be smaller by 0.3-0.9% in poor counties, which brings the increase in real wages relative to the median down to about 0.7-1.3%. Furthermore, the poor who are not working would only bear the higher costs of living without increases in income.

Second, within-chain price rigidity can substantially lower the impact of local minimum wage shocks on local prices. In stores that belong to chains that practice rigid pricing and locate in states across the US, the estimated pass-through elasticity is indistinguishable from zero. This also attenuates the increase in real wage inequality by negating demand feedbacks. Since grocery stores have an expenditure share of about 60% among the stores in the data,[45] this implies that chain rigidity lowers the pass-through elasticity to about 0.035 from 0.083 using only flexible pricing stores, which is a 58% decrease. However, stores that price nationally are still likely to react to national shocks such as a rise in the federal minimum wage, and extrapolating from the subsample of grocery stores that exhibit local pricing, a 10% rise in the federal minimum wage could raise prices nationally by 0.8%.

Third, these results imply that looking only at the nominal spending response of the minimum wage hike would hugely overstate its benefits for low wage workers, as the response in real spending is around half of the response in nominal spending.

Fourth, increasing residential segregation would magnify the regressive nature of the minimum wage tax, since low wage workers will be more likely to shop at the same stores

[45] This share is calculated from the Nielsen Consumer Panel using expenditures on store types available in the Nielsen Retail Scanner.

and experience bigger rises in cost of living. This mechanism should also apply to local goods and services with sufficiently high demand responses to income, and could arise even within counties if shopping locations are strongly segregated by the income of consumers. While there are no good data on the demographic characteristics of customers in each store, I provide suggestive evidence that income segregation does magnify this mechanism in Appendix F.

Fifth, there has been a global movement to increase the minimum wage to unprecedented levels in both Europe and the US. These results imply that the pass-through elasticity will become correspondingly larger and each minimum wage hike will have increasing effects on inflation. For example, if the national minimum wage in the US increases from \$7.25 to \$15, the national Kaitz index would be raised from around 0.3 to 0.6, assuming the average wage only increases slightly. Extrapolating out of sample with the triple difference results above, this implies that the pass-through elasticity would increase from 0.06 to roughly 0.19. The effect of each minimum wage hike is progressively stronger due to the non-linearity in the price response. A further 10% increase in minimum wage would raise grocery store prices by 1.9%

8 Conclusion

In this paper, I find evidence that the minimum wage increases prices in grocery stores but not in other store types because of rigid pricing within retail chains. A 10% minimum wage hike raises grocery store prices by about 0.58%. This finding holds across different phases of the business cycle and passes a variety of robustness checks. Furthermore, the pass-through elasticity is stronger in regions where the minimum wage is more binding. I present evidence that the minimum wage increases earnings of grocery store workers, but based on cost pass-through theory, this labor *cost* increase is not large enough to fully explain the rise in prices. I propose that *demand*-induced feedback leads to a larger pass-through elasticity by increasing income, lowering demand elasticities, and increasing retail markups. I support this claim with four pieces of evidence. First, I derive pass-through formulas for calibrations to show that demand effects can account for size of the reduced-form estimate. Second, I find suggestive evidence that poorer households lower shopping intensities when the minimum wage rises, consistent with lower price sensitivities. Third, I provide evidence that multi-product retailers raise prices for more demand inelastic product groups, consistent with retail markups. Fourth, I find that merchandise stores, which do not raise their prices in response to local minimum wage shocks due to within-chain price rigidity, exhibit large nominal and real sales responses to minimum wage increases.

Demand-induced feedback would also create significant dispersion in pass-through elasticity between rich and poor regions, which has important implications for real wage inequality, residential segregation, and future minimum wage increases. My results imply that due to regional heterogeneity in earnings, the reduction in real wage inequality caused by minimum wage hikes is smaller than the reduction in nominal wage inequality. Regional heterogeneity in earnings, combined with residential segregation by different income groups, could lead to income segregation in shopping locations. Thus, increasing residential segregation may lead to larger dispersion in the minimum wage pass-through elasticity between income groups. By using regions where the minimum wage is more binding, I extrapolate that the minimum wage could have significant inflationary impacts if it continues to rise relative to the average wage. While the effects of local minimum wage hikes on prices and real wage inequality are weakened by within-chain price rigidity, a rise in the federal minimum wage could have much larger effects.

As movements to increase the minimum wage to historic levels gain traction around the world, the impact of the minimum wage on the cost of living becomes more crucial. Better data on both prices and quantities of goods and services in other industries and countries are needed to inform the policy debate.

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Tables

Table 1: Share of workers paid at or below the minimum wage by industry, top 10

Industry	Proportion of all MW workers hired	Within-industry share of MW workers
Restaurants and other food services	0.296	0.289
Grocery stores	**0.040**	**0.114**
Elementary and secondary schools	0.036	0.032
Other amusement, gambling, and recreation industries	0.027	0.129
Colleges and universities, including junior colleges	0.022	0.049
Department stores and discount stores	**0.022**	**0.073**
Construction	0.020	0.021
Traveler accommodation	0.019	0.108
Private households	0.019	0.194
Child day care services	0.016	0.113

Notes: Pooled data from CPS MORG, 2006-2015. Shares are constructed using CPS sample weights. Almost all workers appear twice by construction of the rotating panel. Industries are classified according to the 2010 Census occupational classification used by the CPS. The second column refers to the share of all MW workers that work in a specific industry, whereas the third column refers to the share of MW workers within the specified industry.

Table 2: Minimum wage impact on labor markets by industry

Industry	(1) Drug	(2) Grocery	(3) Department	(4) Merchandise	(5) Restaurant
Earnings	0.0638	0.146***	0.0323	0.157***	0.252***
	(0.0390)	(0.0420)	(0.0466)	(0.0568)	(0.0341)
	115,663	122,081	66,330	115,499	122,698
Employment	0.0880	0.00491	-0.135	-0.245	-0.0755
	(0.0604)	(0.0955)	(0.178)	(0.184)	(0.0551)
	87,483	101,726	35,355	93,439	118,609
County FE	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X

Notes: Data from the QWI, 2006-2015. Coefficients are obtained from 10 separate regressions of the outcomes on the minimum wage under different specifications. Robust standard errors are in parentheses, clustered by state. Number of observations are given below the standard errors. *** p<0.01, ** p<0.05, * p<0.1. Log county population is added as a control variable. Industries include drug stores, grocery stores, department stores, other merchandise stores, and restaurants as classified by 4-digit NAICS (4461, 4451, 4521, 4529, and 7225).

Table 3: Effect of minimum wage on prices by store type

Store Type	(1) Drug	(2)	(3) Grocery	(4)	(5) Merchandise	(6)
VARIABLES	Log price index					
Log MW	-0.0469	-0.0632	0.0605*	0.0576**	-0.00623	-0.0121
	(0.0555)	(0.0500)	(0.0311)	(0.0255)	(0.0241)	(0.0214)
Log housing price		0.0461**		0.0181		0.0265**
		(0.0194)		(0.0140)		(0.0131)
Log unemployment rate		-0.00828		-0.000742		0.00245
		(0.00858)		(0.00606)		(0.00452)
Log population		0.00427		-0.0776***		-0.0593***
		(0.0315)		(0.0243)		(0.0212)
Log average wage		-0.0111		-0.00676		-0.00342
		(0.00989)		(0.00867)		(0.00742)
Observations	354,064	353,938	287,284	287,122	282,092	282,037
R-squared	0.873	0.875	0.928	0.929	0.895	0.896
Prob > F	0.403	0.000	0.058	0.002	0.797	0.064
Store FE	X	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X	X
Number of units	8853	8852	7183	7180	7054	7054
Number of clusters	48	48	48	48	49	49

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Pass-through elasticity estimates interacted with determinants, grocery stores

VARIABLES	(1) Kaitz index	(2) Log AW	(3) Fraction below 25K	(4) MW / median	(5) Log sales	(6) Log est. per cap
Interaction coefficient	0.426***	-0.124***	0.367***	0.438***	-0.0853***	-0.000954
	(0.0617)	(0.0171)	(0.0526)	(0.0579)	(0.00909)	(0.00971)
Observations	287,122	287,122	287,122	287,122	287,122	287,117
R-squared	0.949	0.949	0.949	0.949	0.951	0.948
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	7180	7180	7180	7180	7180	7180
Number of clusters	48	48	48	48	48	48
State-period FE	X	X	X	X	X	X

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. The interaction coefficient is obtained from a regression of the log price index on log minimum wage, the variable of interest, and its interaction with the minimum wage, along with control variables as well as store and period fixed effects. Adding state-period fixed effects implies that only within-state-period variation is used, i.e. county variation in the determinants of pass-through. The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county. Sales are constructed from Nielsen retail scanner data. Average wages (AW), the number of establishments per capita (Est. per cap) are obtained at the county level from the QWI and QCEW and matched to store type. Fraction of HHs earning below $25K (Fraction below $25K) and median household income are obtained from the ACS 5-year estimates.

Table 5: Summary statistics of rich and poor counties by Kaitz index, 2006 first quarter

Variable	Rich	Poor
	Mean	
Population	204859	55956
Kaitz index	0.32	0.43
Average weekly wage	691.73	521.15
Unemployment rate	5.27	6.01
Grocery stores per 100K population	26.00	24.55
Grocery stores	72.10	16.54
States	49	47
N	1098	1086

Notes: Data from QCEW. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties. The number of counties are not equal due to ties.

Table 6: Effect of minimum wage on prices by store type and Kaitz index

	(1)	(2)	(3)	(4)	(5)	(6)
Store Type	Drug		Grocery		Merchandise	
Counties	Rich	Poor	Rich	Poor	Rich	Poor
VARIABLES	Log price index					
Log MW	-0.0578	-0.101	0.0584**	0.0837***	-0.0123	-0.00306
	(0.0453)	(0.0680)	(0.0270)	(0.0201)	(0.0230)	(0.0198)
Observations	309,002	44,936	246,966	40,156	231,709	50,328
R-squared	0.872	0.894	0.928	0.941	0.894	0.912
Prob > F	0.000	0.038	0.005	0.003	0.119	0.252
Number of units	7730	1124	6177	1004	5799	1261
Number of clusters	48	40	48	41	48	45

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 7: Effect of minimum wage on prices by sample period, grocery stores

	(1)	(2)	(3)	(4)
Sample period	2006-07	2008-09	2010-13	2014-15
VARIABLES	Log price index			
Log MW	0.00697	0.0588***	0.0303	0.0759***
	(0.0111)	(0.0219)	(0.0387)	(0.0230)
Observations	57,390	57,432	114,864	57,436
R-squared	0.862	0.880	0.931	0.963
Prob > F	0.009	0.000	0.073	0.000
Number of units	7179	7179	7179	7180
Number of clusters	48	48	48	48

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. All of the federal minimum wage variation is announced in May 2007 and occurs in 2007-2009, along with many state changes during that period. There was very little minimum wage variation from 2010-2013 barring those from states that indexed their minimum wage to the national CPI. A new wave of minimum wage hikes began in 2014-2015 and were all initiated by states.

Table 8: Effect of minimum wage on prices for grocery stores, robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Specification	Default	Drop outliers	Monthly	No indexing states	Interior counties	Contiguous counties	Substate MW	Store revenue weights	Population weights	County observations	Time trend
VARIABLES	Log price index										
MW level	0.00845**										
	(0.00365)										
Log MW		0.0474**	0.0564**	0.0671**	0.0690**	0.0432***	0.0545**	0.0466*	0.0567**	0.0455*	0.0571***
		(0.0201)	(0.0249)	(0.0299)	(0.0319)	(0.0144)	(0.0241)	(0.0243)	(0.0226)	(0.0237)	(0.0171)
Observations	287,122	258,398	857,886	220,762	188,702	98,420	287,122	287,122	287,122	53,682	287,118
R-squared	0.929	0.939	0.929	0.928	0.927	0.934	0.929	0.928	0.919	0.964	0.994
Prob ¿ F	0.002	0.000	0.005	0.000	0.000	0.000	0.003	0.008	0.018	0.000	0.002
Number of units	7180	6461	7151	5521	4721	2464	7180	7180	7180	1344	7179
Number of clusters	48	48	48	38	44	48	48	48	48	48	48

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. MW level refers to using minimum wage levels as the regressor instead of log minimum wage. Dropping outliers refers to dropping stores with average price indices below the 5th percentile or above the 95th percentile. Monthly observations refers to a regression using a monthly price index instead of quarterly. No indexing states drops states that index their minimum wage to the national CPI. Contiguous and interior counties refers to stores in counties that are contiguous to counties in another state across the border and those that are not. Include substate MW matches stores to their local minimum wage (city or county level) if such local ordinances are in place. County observations refers to aggregating the price indices to the county level with store revenue weights and then weighting each county observation by total store revenue in that county. Time trend refers to adding store-specific time trends.

Table 9: Effect of minimum wage on prices for grocery stores, alternative price indices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Method	Alternative weights	Tornqvist	One-stage	Geometric	Fixed	Fixed posted price	County base
VARIABLES	Log price index						
Log MW	0.0576**	0.0526**	0.0566**	0.0576**	0.0595*	0.0567*	0.0506**
	(0.0246)	(0.0228)	(0.0239)	(0.0236)	(0.0317)	(0.0335)	(0.0214)
Observations	295,562	295,562	295,562	295,562	295,562	295,322	55,270
R-squared	0.926	0.920	0.924	0.857	0.874	0.900	0.956
Prob > F	0.015	0.017	0.013	0.020	0.002	0.024	0.002
Number of units	7391	7391	7391	7391	7391	7385	1382
Number of clusters	48	48	48	48	48	48	49

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store (or county whenever appropriate) and period fixed effects are included. Details on each of the index construction methods used above are shown in Appendix B. For the county-level specification, the regression is weighted by county revenue.

Table 10: Effect of minimum wage on real and nominal sales by store type and Kaitz index

	(1)	(2)	(3)	(4)	(5)	(6)
Store Type	Drug		Grocery		Merchandise	
Counties	Rich	Poor	Rich	Poor	Rich	Poor
Log real sales	0.0516	0.161	-0.0147	0.0695	0.00473	0.221**
	(0.0707)	(0.134)	(0.0590)	(0.0763)	(0.0425)	(0.0831)
Log sales	-0.00614	0.0591	0.0437	0.153*	-0.00761	0.218**
	(0.0580)	(0.0953)	(0.0551)	(0.0771)	(0.0586)	(0.0946)
Observations	309,002	44,936	246,966	40,156	231,709	50,328
Number of units	7730	1124	6177	1004	5799	1261
Number of clusters	48	40	48	41	48	45

Notes: Coefficients are obtained from 12 separate regressions (by store type and Kaitz index) of the two outcomes, log real sales and log nominal sales, on the minimum wage along with control variables as well as store and period fixed effects. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 11: Effect of minimum wage on prices and real sales for grocery stores in poor counties by product department

	(1)	(2)	(3)	(4)	(5)
Product Department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise
Log price index	0.0333	0.0575**	0.168***	0.112**	-0.0623
	(0.0277)	(0.0266)	(0.0444)	(0.0529)	(0.0461)
Log real sales	-0.0762	0.111	-0.167	0.350	0.183
	(0.141)	(0.0705)	(0.140)	(0.351)	(0.167)
Observations	40,276	40,236	40,076	39,436	40,236
Number of units	1007	1006	1002	986	1006
Number of clusters	41	41	41	39	41

Notes: Coefficients are obtained from 10 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with control variables as well as store and period fixed effects. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 12: Effect of chain rigidity on pass-through elasticities

	(1)	(2)	(3)	(4)
Store Type	All		Grocery	
VARIABLES	Log price index			
Log MW	0.0156	-0.0350	0.0828***	0.00318
	(0.0278)	(0.0344)	(0.0259)	(0.0261)
States in chain x Log MW	-0.00140***			
	(0.000443)			
Flexibility measure x Log MW		0.197*		
		(0.110)		
Observations	923,057	889,925	206,644	80,478
R-squared	0.877	0.877	0.931	0.935
Prob > F	0.020	0.065	0.000	0.012
Number of units	23081	22340	5167	2013
Number of clusters	49	49	48	39
Local pricing			X	

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Local pricing refers to stores with flexible pricing or stores in chains located in one or two states.

Table 13: Theoretical estimates of minimum wage pass-through elasticity, labor cost effect

Industry	Labor cost share	Weighted share of wages earned by MW affected workers			Spillover adjustment	Pass-through rate	MW pass-through elasticity		
		Poor counties	Rich counties	All			Poor counties	Rich counties	All
Grocery Stores	0.101	0.084	0.073	0.081	None	1	0.0085	0.0074	0.0082
Grocery Stores	0.101	0.162	0.164	0.181	AMS	1	0.0163	0.0165	0.0181
Grocery Stores	0.101	0.210	0.207	0.228	Lee	1	0.0211	0.0208	0.0229
Health Stores	0.120	0.034	0.025	0.028	None	1	0.0041	0.0031	0.0034
Health Stores	0.120	0.101	0.095	0.103	AMS	1	0.0120	0.0114	0.0123
Health Stores	0.120	0.129	0.117	0.128	Lee	1	0.0154	0.0141	0.0153
Department Stores	0.108	0.097	0.069	0.074	None	1	0.0104	0.0074	0.0080
Department Stores	0.108	0.202	0.171	0.188	AMS	1	0.0218	0.0185	0.0202
Department Stores	0.108	0.273	0.222	0.246	Lee	1	0.0294	0.0240	0.0265
Restaurants	0.297	0.181	0.137	0.150	None	1	0.0538	0.0407	0.0447
Restaurants	0.297	0.283	0.253	0.273	AMS	1	0.0841	0.0752	0.0811
Restaurants	0.297	0.340	0.305	0.324	Lee	1	0.1010	0.0905	0.0964

Notes: Pooled data from ACS, 2006-2015. Industries are classified according to the NAICS. Labor cost shares are from 2007 and 2012 SUSB. Shares are constructed using ACS sample person weights. Spillover adjustments are made based on theoretical derivations and using spillover elasticity estimates from Autor, Manning, and Smith (2016) (AMS) and Lee (1999), normalized by the maximum percentile. Pass-through rates are taken from estimates in previous literature. The minimum wage pass-through elasticity is a multiple of the labor cost share, the weighted share of wages earned by MW affected workers, and the pass-through rate as shown by theory.

Table 14: Theoretical estimates of minimum wage pass-through elasticity, demand effect

MPC	Income elasticity	Demand	Income elasticity of MW			Pass-through	Demand pass-through elasticity		
	of demand	elasticity	Poor counties	Rich counties	All	rate	Poor counties	Rich counties	All
0.03	0.3	0.709	0.102	0.067	0.0833	0.5	0.0216	0.0142	0.0176
0.065	0.65	0.709	0.102	0.067	0.0833	0.5	0.0468	0.0307	0.0382
0.1	1	0.709	0.102	0.067	0.0833	0.5	0.0719	0.0472	0.0587

Notes: MPC estimates are collected from the previous literature. Income elasticities of demand are calculated from the MPC estimates assuming an expenditure share of 0.1 for food-at-home. Demand elasticity and pass-through rate are estimated in Leung and Seo (2018) and consistent with the previous literature. Income elasticity of minimum wage is calculated from estimates from Dube (2017) and derivations in Section D.3.

Table 15: Effect of minimum wage on shopping behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Log expenditure		Coupon share		Deal share		Store brand share	
Log MW	0.00964	-0.0148	0.00893***	0.00917***	0.00623	0.00850	0.0136**	0.0150**
	(0.0330)	(0.0295)	(0.00236)	(0.00219)	(0.00949)	(0.0109)	(0.00663)	(0.00606)
Log MW x Below median	-0.0212	-0.0181	-0.00705***	-0.00678***	-0.0456***	-0.0440***	-0.00936**	-0.00957**
	(0.0203)	(0.0204)	(0.00161)	(0.00160)	(0.00671)	(0.00659)	(0.00372)	(0.00365)
Observations	1,539,441	1,539,076	1,539,443	1,539,078	1,539,441	1,539,076	1,539,441	1,539,076
R-squared	0.729	0.731	0.767	0.767	0.850	0.850	0.688	0.688
Prob > F	0.571	0.000	0.000	0.000	0.000	0.000	0.008	0.008
Number of units	61437	61431	61437	61431	61437	61431	61437	61431
Number of clusters	49	49	49	49	49	49	49	49
Controls		X		X		X		X

Notes: Robust standard errors are in parentheses, clustered at the state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Observations are weighted by sampling weights. Household and period fixed effects are included. Control variables include log housing price, log county unemployment rate, log county population, log county average wage, and household size fixed effects. Below median refers to an indicator variable for households with a household income below the median in 2006. Coupon share denotes the share of expenditures made using a coupon, deal share denotes the share of expenditures on goods on sale, and store brand share denotes the share of expenditures on goods that are store brand.

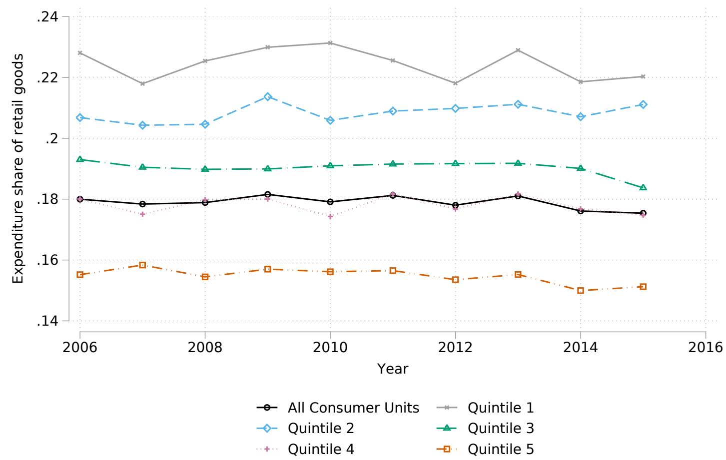
Table 16: Determinants of pass-through elasticity

	(1)	(2)	(3)	(4)
Counties	Rich		Poor	
VARIABLES	Pass-through elasticity estimate			
Grocery	0.0948***	0.0765***	0.0615**	0.0728***
	(0.0152)	(0.0102)	(0.0261)	(0.0143)
Merchandise	0.0692***	0.0352***	0.0567***	0.0264**
	(0.0146)	(0.00885)	(0.0218)	(0.0122)
Log demand elasticity	-0.00409	-0.00278	-0.0179**	-0.00673
	(0.00540)	(0.00337)	(0.00769)	(0.00520)
Poor revenue share	0.0251	0.0767	0.0615	0.0101
	(0.140)	(0.0971)	(0.234)	(0.137)
Constant	-0.0571***	-0.0415***	0.00985	-0.00834
	(0.0206)	(0.0110)	(0.0302)	(0.0163)
Observations	283	283	274	274
R-squared	0.157	0.160	0.052	0.089
Prob > F	0.000	0.000	0.003	0.000
OLS Robust	X		X	
WLS		X		X

Notes: Second stage regression where estimated store type by product group pass-through elasticities are regressed on regressors of interest, with drug stores being the omitted group. Demand elasticities by product group are obtained from Broda and Weinstein (2010). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Poor revenue share is the revenue share in 2006 accounted for by households with below median household income in each product group according to the consumer panel. Pass-through elasticities estimated with fewer than 10 states are dropped due to lack of power in the estimate, and results are robust to using other reasonable numbers as thresholds.

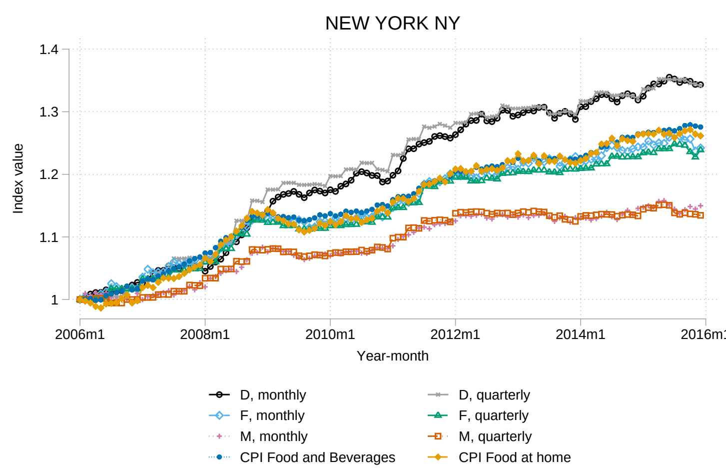
Figures

Figure 1: Expenditure shares on retail goods by income quintile and year



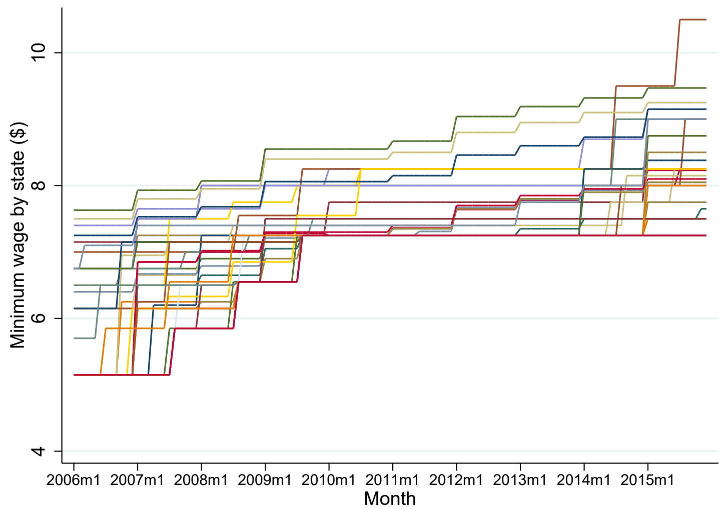
Notes: This figure plots expenditure shares on retail goods by income quintile from 2006-2015, which range from an average of 15.48% for top quintile consumer units to 22.44% for bottom quintile consumer units. Data from the CEX are used and selected categories are all sold by Nielsen retail stores, which include alcoholic beverages, drugs, food at home, toys, audio and visual equipment, medical supplies, household furnishings and equipment, housekeeping supplies, personal care products and services, and tobacco.

Figure 2: Comparison of Nielsen price indices with CPI



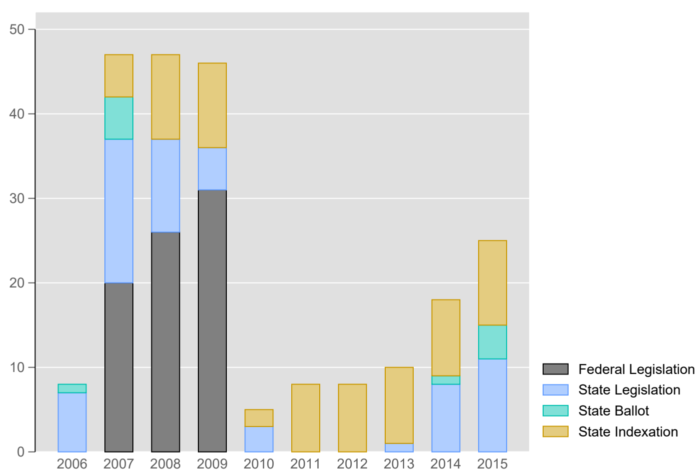
Notes: This figure plots city-level price indices from 2006-2015 constructed using Nielsen retail scanner data against those used by the BLS to construct the CPI. D, F, and M correspond to Nielsen price indices for drug stores, grocery stores, and mass merchandise stores respectively. Nielsen price indices are first constructed at the store level, and aggregated to the city level by taking a sales-weighted average.

Figure 3: Minimum wage over time for all states, 2006-2015



Notes: This figure plots the state-effective minimum wage for each state in each month from 2006-2015. States that do not have a state minimum wage are bounded by the federal minimum wage.

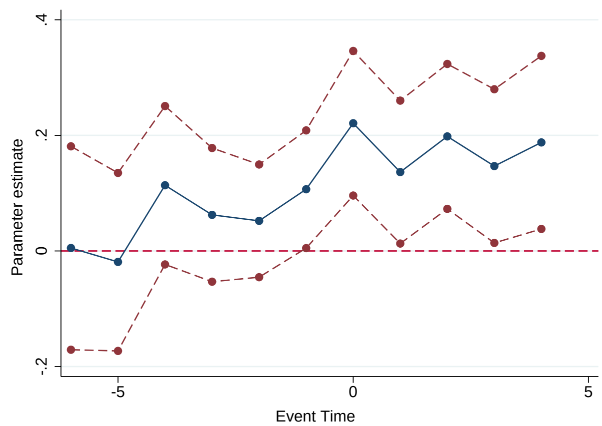
Figure 4: Minimum wage changes over time by type for all states



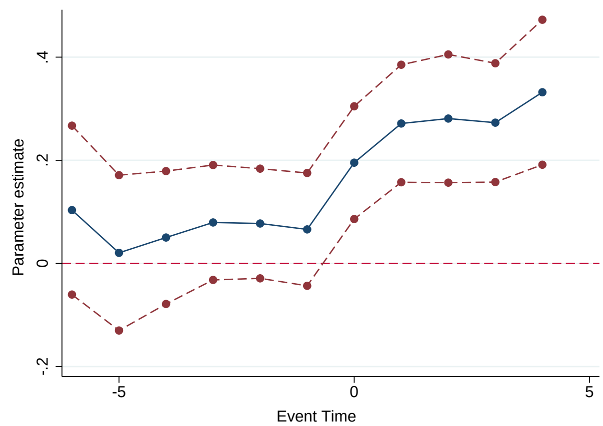
Notes: This figure plots the total number of changes in the minimum wage across states in each year, segmented by the type of change. There are 4 types of minimum wage changes: federal legislation, state legislation, state ballot (where voters decide whether the minimum wage should be increased), and subsequent changes due to indexation to the national CPI (with the exception of Colorado which indexes their minimum wage to the city-level CPI).

Figure 5: Cumulative effects of minimum wage on labor markets

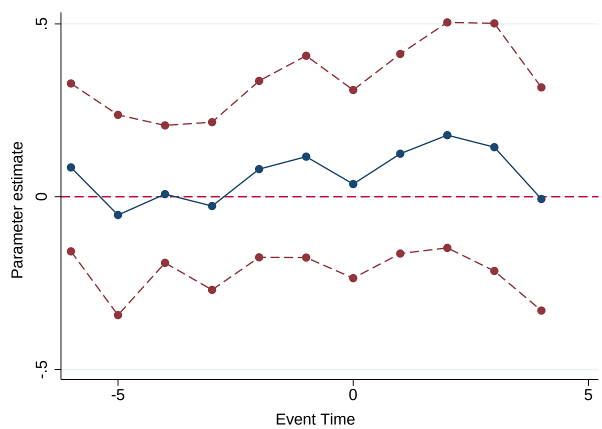
(a) Earnings, grocery stores



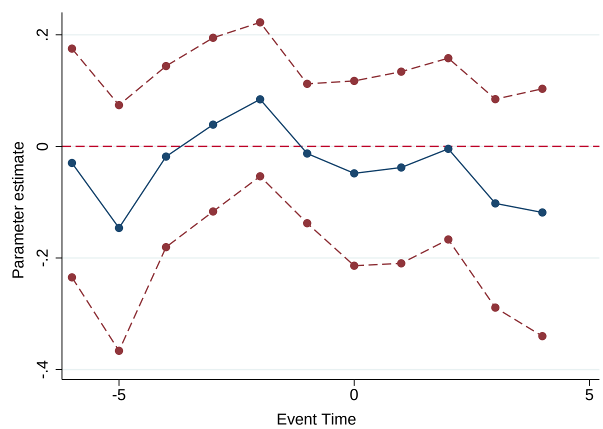
(b) Earnings, restaurants



(c) Employment, grocery stores



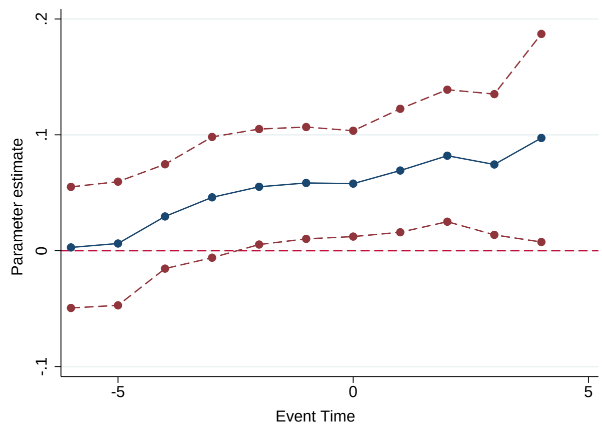
(d) Employment, restaurants



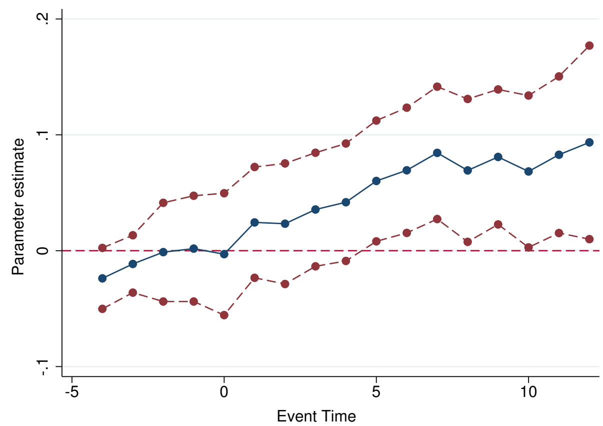
Notes: This figure plots the sum of estimated coefficients for each period, along with the 95% confidence intervals, from regressions using a distributed lag model, where log earnings and log employment are regressed on log minimum wage. County and period fixed effects are included. The event window starts from 6 quarters before a minimum wage change and ends 4 quarters after the change. Regressions are run separately for both grocery stores and restaurants.

Figure 6: Cumulative effect of minimum wage on prices for grocery stores

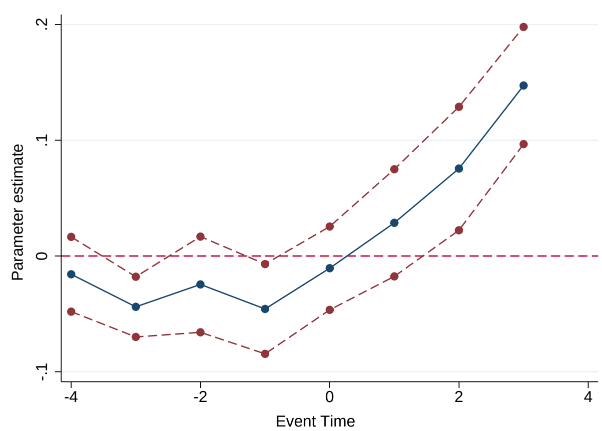
(a) Implemented minimum wage, 2006-2015



(b) Announced minimum wage, 2006-2015



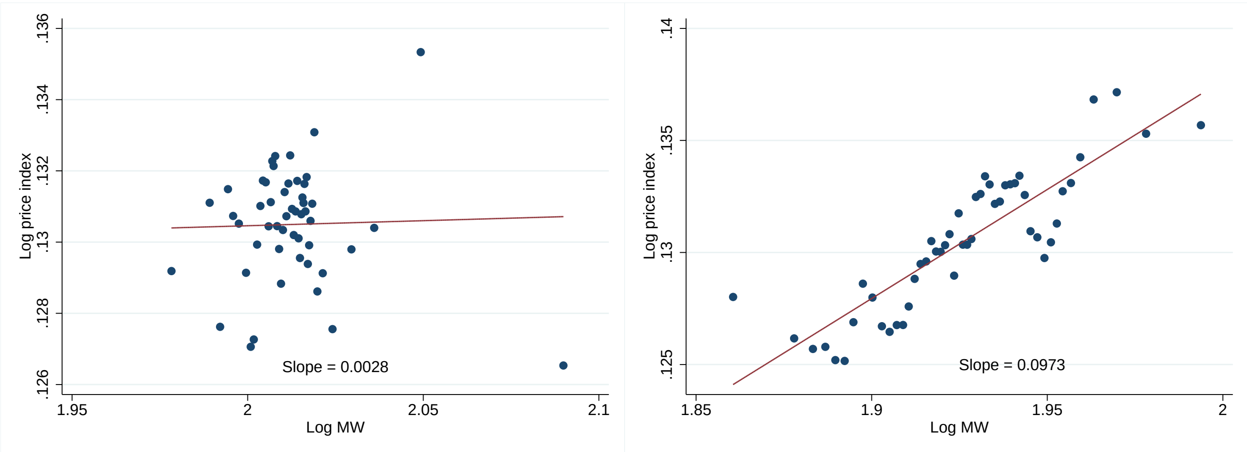
(c) Implemented minimum wage, 2013-2015



Notes: This figure plots the sum of estimated coefficients for each period, along with the 95% confidence intervals, from regressions using a distributed lag model, where log price index is regressed on log minimum wage. Control variables as well as store and period fixed effects are included. In panel (a), the effect of the implemented minimum wage is estimated. The event window starts from 6 quarters before a minimum wage change and ends 4 quarters after the change. Prices rise 3-4 quarters before implementation in the 2006-2015 sample, suggesting announcement effects. Panel (b) confirms this by estimating the effect of the announced minimum wage for states that do not index their minimum wage to the national CPI, with the event window shifted to retain observations. When focusing on 2013-2015 when implementation lags were much shorter, the timing of the effect is sharper. Event windows are chosen to retain 2015q1 minimum wage variation, since the latest minimum wage data are available until 2016q3.

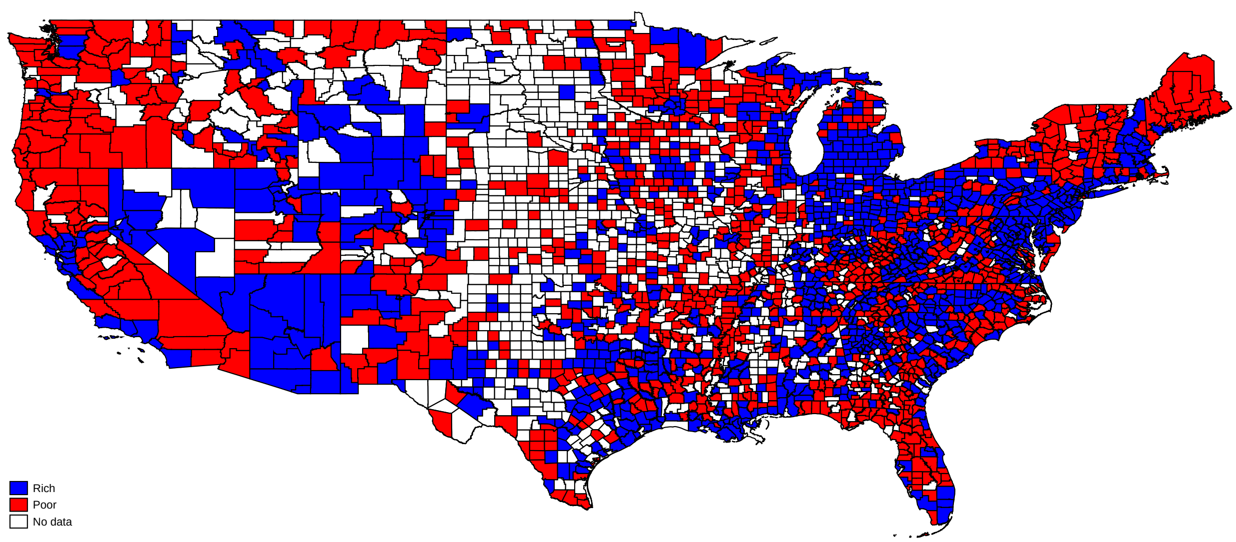
Figure 7: Cumulative effect of minimum wage on prices, grocery stores

(a) 6 quarters before minimum wage change (b) 4 quarters after minimum wage change



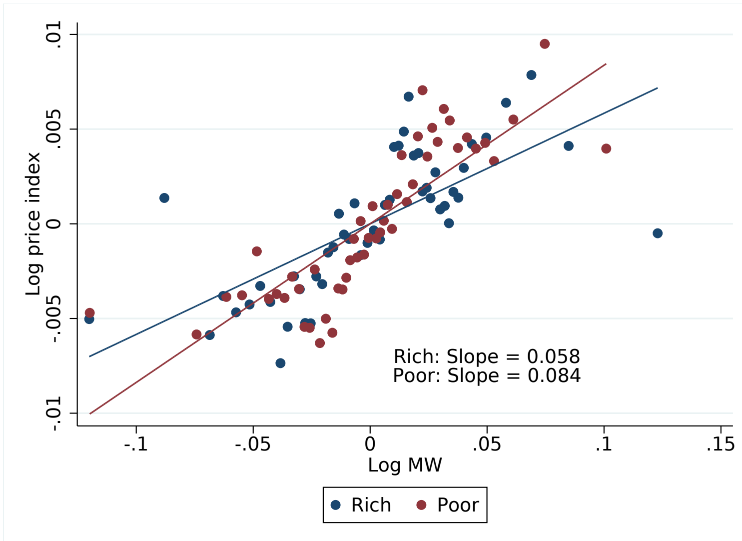
Notes: This figure plots the observations used to estimate the cumulative effect of the minimum wage on prices as shown in Figure 6. Two coefficients from the same regression are shown: Panel (a) shows the cumulative effect 6 quarters before a minimum wage change, while Panel (b) shows the cumulative effect 4 quarters after a change. For each store-year-quarter observation, the residualized log minimum wage (6 quarters ahead or 4 quarters after) is calculated and grouped into 50 quantiles. The x-axis displays the mean of the residualized log minimum wage in each quantile. The y-axis shows the mean of the residualized log price index in each quantile. The line of best fit is obtained from the regression using all observations, and its slope is reported on the graph. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Figure 8: Rich and poor counties by Kaitz index, 2006 first quarter



Notes: This figure maps out counties that contain stores present in the Nielsen retail scanner data, denoting them as rich and poor as measured by the Kaitz index in the first quarter of 2006. The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Figure 9: Log price index on log minimum wage, grocery stores in rich and poor counties

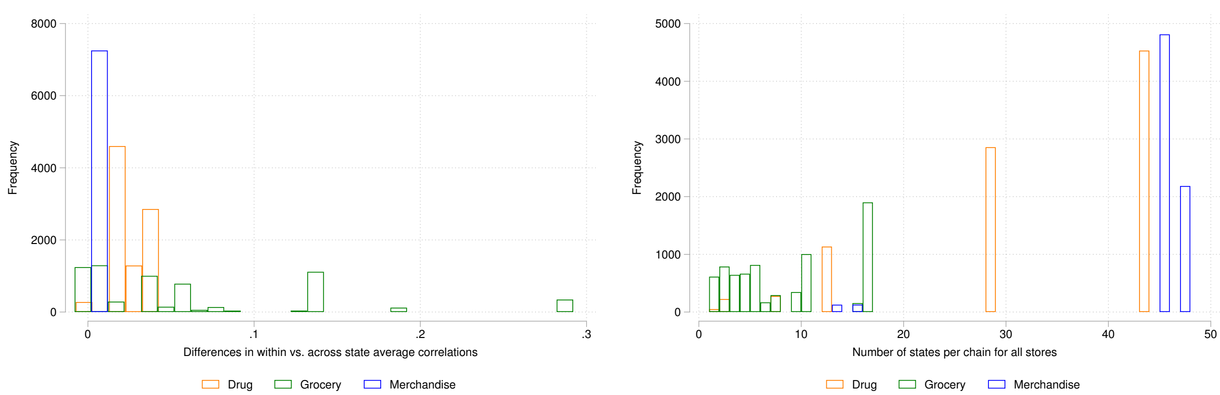


Notes: This figure plots the log price index against the log minimum wage by rich and poor counties as measured by the Kaitz index. Both variables are residualized by regressing on a set of controls, store fixed effects, and period fixed effects. For each store-year-quarter observation, the residualized log minimum wage is calculated and grouped into 50 quantiles. The x-axis displays the mean of the residualized log minimum wage in each quantile. The y-axis shows the mean of the residualized log price index in each quantile. This is done separately for samples containing rich and poor counties, and the line of best fit is obtained from the regression using all observations in each sample, and the slopes are reported on the graph. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties. The sample is restricted to grocery stores.

Figure 10: Distribution of flexibility measures across store types

(a) Flexibility measures

(b) Number of states in each chain for all stores



Notes: This figure plots the chosen flexibility measure, differences in within vs. across state average correlations, as illustrated in Section 4.2.2, and also the number of states in each chain for all stores.

Appendix

A Comparisons with Contemporaneous Work

There are several differences between Renkin et al. (2017) and my paper. First, the authors use the Symphony IRI scanner dataset which has a smaller amount of stores, with 3,187 grocery stores across 41 states from 2001-2012. However, there are few minimum wage changes between 2001-2005. In contrast, I use the Nielsen Retail Scanner dataset from 2006-2015 which contains 35,000 stores and includes drug, grocery, and mass merchandise stores. The sample period contains a much larger number of minimum wage changes after the recession as shown in Figure 4, which could explain why the estimated pass-through elasticity is higher in my paper. As shown in Table 7, the estimated pass-through elasticity is slightly larger in the recovery period. Second, they use first differences of the log price index rather than the level as the dependent variable to allow for store-specific time trends. I show in Table 8 that my results are robust to adding store-specific time trends. Third, I provide suggestive evidence that a higher pass-through elasticity could be explained by demand effects. Fourth, the availability of other store types in my data enables me to further verify the effect of price rigidity within retail chains on the minimum wage pass-through elasticity.

Another contemporaneous paper is Ganapati and Weaver (2017). In their appendix, they construct price indices in a manner similar to the methodology I use, but with several major alterations and argue that the results in this paper are not robust. First, they aggregate their observations to the county-product-quarter level or county-product-store type-quarter level while I aggregate to the store-product-quarter level. To directly verify their claims, I construct the price index using this alternative method as mentioned in Appendix B and show that the results remain robust to this method as well as a range of alternative price index construction methods in Table 9.

In addition, in their preferred specification, instead of constructing a price index which measures the cost of living, Ganapati and Weaver (2017) draw a random 1% sample (or weighted by revenue) of 5,000 goods in dry grocery in grocery stores that appear in more than one state for more than one year, and run regressions with observations at the county-product-quarter level with county-product and product-time fixed effects. They find that the estimated pass-through elasticity is insignificant and point estimates are close to zero. Their approach will likely generate different results from using a price index because in their regression, each county-product-quarter observation selected into the sample is weighted equally, while the price index I construct aggregates products weighted by their quantity sold in the first stage and revenue in the second stage. Their specification estimates the impact of the minimum wage on prices for the average *county-product* in their random or weighted sample while my specification estimates the impact of the minimum wage on the *cost of living* for a consumer buying a consumption bundle sold in an average store. To illustrate that this is the case, I replicate their preferred specification using an unweighted 1% sample of goods in dry grocery in grocery stores. I show in Table G12 that the regression coefficients depend strongly on the weighting scheme used. When products are weighted by their total revenue, the point estimate becomes larger but insignificant, and the point estimates become larger and significant when the regression is weighted by county revenue, population, or county-product revenue. In Figure H4, I show that these weights are highly unequal and

Pareto distributed, which suggests that weighting all counties equally may mask substantial heterogeneity in coefficients across regions. The coefficients are also much larger when only county-product observations that appear in every period are used, since there are a lot of missing values for products that have zero sales in a period. These results are robust across a large number of alternative 1% samples. On the other hand, I show that my regression results are robust to weighting by store revenue, county population, or aggregating to the county level using store-level price indices in Table 8.

B Construction of Price Indices

Beraja et al. (2015) adopt a two-stage procedure that is very similar to the one used by the BLS in constructing the CPI, and introduce some improvements enabled by scanner data. A viable alternative would have been to use an exact price index as defined in Diewert (1976) for the CES unit-cost function by applying Sato (1976) and Vartia (1976) weights, which would be theoretically founded. These exact price indices can also account for new product varieties within the CES framework as demonstrated in Feenstra (1994) and implemented in Broda and Weinstein (2010). However, I did not choose this alternative for two reasons. First, I wish to make the indices more comparable to the CPI for easier comparison. Second, theoretically founded indices that account for product turnover require estimation of parameters that are very computationally intensive given the size of the dataset.

In the first stage, the index is constructed at both the monthly and quarterly level for each product group (125 groups) and store. Stores that do not appear throughout the entire sample period are dropped, retaining around 23,500 stores in the sample. Therefore, the results are not affected by store entry and exit. Among the stores that are in the sample in 2006, 84% remain throughout the entire sample period. Although the scanner data are weekly, they are aggregated up to the monthly or quarterly level to decrease missing values and reduce chain drift, as pointed out by Ivancic et al. (2011). Each base observation is a monthly or quarterly unit value for each store-product, i.e. monthly or quarterly revenue divided by the total number of units sold in that period, which is equivalent to a quantity-weighted average price. Products are defined as UPC codes. Alternatively, weekly prices can be sampled from each store-product-period. Only goods that appear consistently across an entire year are included such that around 50% and 70% of all sales are used in constructing store-level monthly and quarterly indices respectively.[46] Quantities are directly observed and used as weights, which is a major advantage relative to the CPI, which collects price quotes at the store level but not quantities, so that they use quantities that are lagged 3-4 years and are obtained from the BLS CEX. Quantity weights are updated yearly to avoid chain drift, and the weights (denoted as $q_{i,y}$) are lagged one year to ensure that price changes are not a result of changing consumption patterns in response to current prices or shocks. The CPI weights are updated every two years, which is less frequent than the scanner index. Hence, the CPI is more subject to substitution bias and the basket is less relevant over time. The price index $P^L_{j,t,y}$ at time t and year y for product group j for each store is shown below in equation 14:

[46] Prices of goods that did not sell within a given week are not recorded in retail scanner data. Therefore, aggregating up to to the monthly or quarterly level decreases missing values. Furthermore, products that are not bought by consumers are inherently not an important part of the consumer basket.

$$P^L_{j,t,y} = P^L_{j,t-1,y} \times \frac{\sum_{i \in j} p_{i,t} q_{i,y-1}}{\sum_{i \in j} p_{i,t-1} q_{i,y-1}} \tag{14}$$

Each unique item is defined by its UPC code. Prices and quantities are observed for each store and UPC pair, which is denoted as product i. By construction, changes in the price index only reflect relative changes in prices for a given bundle and are unaffected by price levels. Therefore, product switching among consumers to more expensive bundles does not change the price index for given price levels.

The second stage is similar to the first stage and aggregates the product group-specific price indices for each store using expenditure shares $s_{j,y-1}$ that are lagged one year and fixed within year. To follow the CPI more closely, a Tornqvist price index can also be constructed using the average expenditure shares between two periods as weights. While I present results using the first method, both methods give almost identical results and are shown in equation 15:

$$\frac{P_t}{P_{t-1}} = \sum_{j=1}^{N} s_{j,y-1} \left(\frac{P^L_{j,t,y}}{P^L_{j,t-1,y}} \right) \tag{15a}$$

$$\frac{P_t}{P_{t-1}} = \prod_{j=1}^{N} \left(\frac{P^L_{j,t,y}}{P^L_{j,t-1,y}} \right)^{\frac{s_{j,t}+s_{j,t-1}}{2}} \tag{15b}$$

I also construct a range of price indices using alternative methods. The first index weights each product group using the expenditure shares of only products chosen to construct the price index, i.e. products that satisfy the consistency criterion illustrated above, as opposed to all products in the data. The second index uses the Tornqvist index mentioned above. The third index constructs the price index in one stage instead of two stages. The fourth index uses a weighted geometric average in the first stage similar to SV instead of a weighted arithmetic average. The fifth index uses weights that are fixed over time at the base period to ensure that the results are not driven by shifts in the consumption bundle over time as opposed to actual price changes. To construct such an index, only products that appeared consistently over the entire sample period can be used. The sixth index again uses fixed weights but also base observations that are sampled from the last observable posted price for each store-product-quarter.[47] The seventh index uses base observations that are first aggregated to the county-store type level. All indices are highly correlated and results are robust to using any of the above methods. I show these indices for New York City in Figure H2, which are nearly identical across construction methods.

[47]In the raw data, the posted price is actually a weekly unit value for Saturday-ending weeks. DVG highlight that this creates a slight aggregation bias but the bias is relatively small for state-level shocks in their calibrations.

C City and County Level Minimum Wage Variation

Some cities as well as counties began to implement local minimum wage ordinances in 2013-2014. Very few studies have exploited this new wave of minimum wage changes. An exception is Allegretto and Reich (2015), who study the response of restaurant prices to a minimum wage hike in San Jose in 2013 by using neighboring restaurants outside the city boundaries as a control group. I exploit the wide geographic coverage of retail stores in my data by using minimum wage variation from 24 cities or counties, which are denoted as substates. Appendix Figure H5 shows the substate minimum wage variation for California and New Mexico, the two states with the most local minimum wage ordinances. In California, San Francisco has continually adopted a higher minimum wage than the state, while San Jose was second in the state to adopt a local minimum wage ordinance, raising minimum wages from \$8 to \$10 in March 2013. 12 other cities soon followed with minimum wage hikes in 2014. I choose the sample to include all stores in substates with their own local minimum wage ordinances, which restricts the sample to 6 states. Results are shown in Appendix Table G13 for all store types using 2 different specifications. In the first specification, store and period fixed effects are included. In the second specification, I add state-period fixed effects to control for state-specific time trends and use only within-state minimum wage variation across cities. The sample is further restricted to 17 substates in California and New Mexico. While pass-through elasticity estimates for drug and merchandise stores were shown to be insignificant using state level variation, the point estimates are large and statistically significant when estimated using substate variation. Adding state-period fixed effects attenuates these results, possibly due to the lack of sample size, but pass-through elasticity estimates for grocery stores actually become significant. The point estimate also increases to 0.047 and is only slightly smaller than the estimate of 0.058 using state level variation.

A major concern when using substate minimum wage variation is that there are not enough substates for valid inference. In my results, standard errors are clustered at the level of variation, i.e. by substate. Since the number of clusters is rather small, I also use wild bootstrap for inference. The results for grocery stores using only state-period fixed effects is significant only at the 15% level. Therefore, I caution against over-interpreting these results and emphasize the robustness in the point estimate for grocery stores between using substate and state variation. Until a sufficient amount of substates implement their own local minimum wage ordinances, it remains difficult to draw valid inference from a research design using only substate variation.

D Derivations

D.1 Cost Pass-Through Elasticity Without Spillovers

Denote w_1 and L_1 as the minimum wage and the number of minimum wage workers respectively.

Shepherd's Lemma: $\frac{\partial C}{\partial w_1} = L_1$

$$\frac{dp}{dw_1} = \frac{dp}{dC}\frac{\partial C}{\partial w_1} = \frac{dp}{dC}L_1$$

$$\begin{aligned}\frac{dp}{dw_1}\frac{w_1}{p} &= \frac{dp}{dC}\frac{C}{p}\frac{p}{C}L_1\frac{w_1}{p} \\ &= \frac{dp}{dC}\frac{C}{p}\frac{w_1L_1}{C}\end{aligned} \tag{16}$$

Unit tax: $\frac{\partial C}{\partial t} = Y$

$$\frac{dp}{dt} = \frac{dp}{dC}\frac{\partial C}{\partial t} = \frac{\partial p}{\partial C}Y$$

$$\begin{aligned}\frac{dp}{dC}\frac{C}{p} &= \frac{dp}{dt}\frac{t}{p}\frac{p}{t}\frac{1}{Y}\frac{C}{p} \\ &= \frac{dp}{dt}\frac{t}{p}\frac{C}{tY}\end{aligned} \tag{17}$$

Equations 16 and 17 $\Rightarrow$

$$\begin{aligned}\frac{d\ln p}{d\ln w_1}\frac{C}{w_1L_1} &= \frac{d\ln p}{d\ln t}\frac{C}{tY} \\ \frac{d\ln p}{d\ln w_1} &= \frac{dp}{dt}\frac{w_1L_1}{pY} \\ &= \frac{dp}{dt}s_{L_1}\end{aligned}$$

D.2 Cost Pass-Through Elasticity with Spillovers

There are n types of workers, and for each type there are L_i workers. Let $i = 1$ denote the minimum wage workers. Spillover effects imply that $\Rightarrow \frac{\partial w_i}{\partial w_1} > 0$ for $i > 1$, and assume the minimum wage w_1 exogenously affects wages of other workers, such that the cost function can be written as:

$$C = \sum_{i=1}^{n} w_i\left(w_1\right)L_i + rK$$

By Shepherd's Lemma,

$$\frac{\partial C}{\partial w_1} = \sum_{i=1}^{n} L_i\frac{\partial w_i}{\partial w_1}$$

Using the unit-tax pass-through formula,

$$
\begin{aligned}
\frac{d\ln p}{d\ln w_1} &= \frac{dp}{dt}\frac{w_1}{pY}\frac{\partial C}{\partial w_1} \\
&= \frac{dp}{dt}\sum_{i=1}^{n}\frac{w_i L_i}{pY}\varepsilon_{w_i,w_1} \\
&= \frac{dp}{dt}\frac{\sum_{i=1}^{n} w_i L_i}{pY}\sum_{i=1}^{n}\frac{w_i L_i}{\sum_{i=1}^{n} w_i L_i}\varepsilon_{w_i,w_1} \\
&= \frac{dp}{dt}\frac{\sum_{i=1}^{n} w_i L_i}{pY}\sum_{i=1}^{n} s_i\varepsilon_{w_i,w_1}
\end{aligned}
$$

where $\varepsilon_{w_i,w_1} = \frac{\partial w_i}{\partial w_1}\frac{w_1}{w_i}$ and $s_i = \frac{w_i L_i}{\sum_{i=1}^{n} w_i L_i}$

D.3 Earnings Elasticity

There are n types of workers, and for each type there are l_i workers. Each of them earn an hourly wage rate of w_i and work h_i hours. Let $i = 1$ denote the minimum wage workers. To simplify the analysis, I assume no employment effects, i.e. $\frac{\partial l_i}{\partial w_1} = 0$. This is supported by the empirical evidence in Section 3. Furthermore, spillover effects imply that $\Rightarrow \frac{\partial w_i}{\partial w_1} > 0$ for $i > 1$, and I assume $h_i = h_i(w_i), w_i = w_i(w_1)$.

The average earnings $\overline{wh}$ is given by

$$
\overline{wh} = \frac{\sum_i l_i w_i h_i}{\sum_i l_i}
$$

The earnings elasticity can then be derived as

$$
\begin{aligned}
\varepsilon_{\overline{wh},w_1} &= \frac{\partial \overline{wh}}{\partial w_1}\frac{w_1}{\overline{\overline{wh}}} = \sum_i \underbrace{\frac{l_i w_i h_i}{\sum_i l_i w_i h_i}}_{s_i}\left(\frac{\partial w_i}{\partial w_1}\frac{w_1}{w_i} + \frac{\partial h_i}{\partial w_1}\frac{w_1}{h_i}\right) \\
&= \sum_i s_i\left(\varepsilon_{w_i,w_1} + \varepsilon_{h_i,w_1}\right) \\
&= \sum_i s_i\varepsilon_{w_i,w_1}\left(1 + \varepsilon_{h_i,w_i}\right)
\end{aligned}
$$

Note that s_i denotes the share of earnings earned by minimum wage workers over the entire wage bill. Without spillover effects, the earnings elasticity is simply given by

$$
\varepsilon_{\overline{w},w_1} = s_1\left(1 + \varepsilon_{h_1,w_1}\right)
$$

These formulas imply that a large hours elasticity and a large spillover effect raises the earnings elasticity.

D.4 Demand Pass-Through Elasticity

Let market demand $Q^D(p,I)$ depend on price p and income I, and now let us assume instead that the minimum wage w_1 has an effect on demand only through income. Under perfect competition, I can obtain the demand pass-through elasticity by differentiating the equilibrium condition with respect to benefits:

$$
\begin{aligned}
Q^D(p,I) &= Q^S(p)\\
\frac{\partial Q^D(p,I)}{\partial p}\frac{dp}{dw_1}+\frac{\partial Q^D(p,I)}{\partial I}\frac{dI}{dw_1} &= \frac{\partial Q^S(p)}{\partial p}\frac{dp}{dw_1}\\
\rho \equiv \frac{dp}{dw_1} &= \frac{\frac{\partial Q^D(p,I)}{\partial I}\frac{dI}{dw_1}}{\frac{\partial Q^S(p)}{\partial p}-\frac{\partial Q^D(p,I)}{\partial p}}\\
\varepsilon_\rho \equiv \frac{d\ln p}{d\ln w_1} &= \frac{\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1}}{1-\frac{\varepsilon_S}{\varepsilon_D}}.
\end{aligned}
\tag{18}
$$

Likewise, I can derive the demand pass-through formula under symmetric imperfect competition. First, I start from the profit-maximization condition and differentiate it with respect to income. In addition, I allow the demand elasticity to depend on income. I obtain an expression for the quantity response to income:

$$
\begin{aligned}
P(Q,I)+\theta\frac{\partial P(Q,I)}{\partial Q}Q-c'(Q) &= 0\\
MR(Q,I)-MC(Q) &= 0\\
\left(\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}\right)\frac{dQ}{dI}+\frac{\partial MR}{\partial I} &= 0\\
\frac{dQ}{dI} &= -\frac{\frac{\partial MR}{\partial I}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}\\
\frac{dQ}{dI} &= -\frac{\theta\frac{\partial^2 p}{\partial I\partial Q}Q+\frac{\partial p}{\partial I}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}.
\end{aligned}
\tag{19}
$$

Next, I use to above expression to obtain the pass-through formula:

$$\begin{aligned}
\frac{dp}{dw_1} &= \frac{\partial p}{\partial Q}\frac{dQ}{dI}\frac{dI}{dw_1} + \frac{\partial p}{\partial I}\frac{dI}{dw_1} \\
&= \left(1 - \frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\right)\frac{\partial p}{\partial I}\frac{dI}{dw_1} + \left(-\frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\theta\frac{\partial^2 p}{\partial b \partial Q}Q\frac{dI}{dw_1}\right) \\
\varepsilon_\rho \equiv \frac{d\ln p}{d\ln w_1} &= \left(1 - \frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\right)\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1} + \frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\frac{\theta}{-\varepsilon_D}\varepsilon_{p',I}\varepsilon_{I,w_1} \\
&= \left(1 - \frac{dp}{dt}\right)\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D}\varepsilon_{I,w_1} + \left(\frac{dp}{dt}\right)\frac{\theta}{-\varepsilon_D}\varepsilon_{p',I}\varepsilon_{I,w_1}. \qquad (20)
\end{aligned}$$

E Results by Product Characteristics

To test the hypothesis of whether products more likely to be consumed by the poor have higher pass-through elasticities, I first show using the consumer panel that expenditure shares by households with different household income levels vary across certain product characteristics. I select the sample to include only households in the pre-period of 2006. Similar patterns hold for later years. I divide households in the data into four household income quartiles after controlling for household size. Results are similar without controlling for household size. Next, for each product group, I calculate the total expenditure recorded by households on organic and non-organic food products as defined by the USDA, as well as store brand and name brand. I do the same for each household income quartile and divide it by the total to obtain expenditure shares by household income quartile within each product group. In Appendix Table G14, I show the average expenditure shares across product groups. Richer households account for a much larger share of expenditures in organic food products than the poor, while poorer households account for a slightly larger share of expenditures in store brand products.

Next, I construct store-level price indices using the same method described in Section B, but further divide the sample of products according to the two product characteristics. I then estimate the pass-through elasticity by running a separate regression for each of these characteristics as shown in Appendix Table G15. The estimated pass-through elasticity and real sales response is positively significant for non-organic food and similar to previous estimates for food. This is expected since non-organic food accounts for about 99% of food sales in the data. The estimates for organic food are statistically insignificant, consistent with the idea that prices and real sales of products consumed mostly by the rich are unaffected by minimum wage shocks. On the other hand, the estimated pass-through elasticity is slightly smaller for store brand products even though these products are more likely to be consumed by the poor. This may be caused by a higher demand elasticity among those products, offsetting the impact of a likely larger demand response in those products. This may also be driven by consumers switching from store brands to name brands as shown in Section 6.

F Income Segregation

As mentioned above, income segregation creates dispersion in pass-through elasticity since poor consumers who experience an increase in income from minimum wage hikes tend to go to the same stores while the rich consumers who are unaffected go to other stores. To investigate this claim, I obtain county-level measures of income segregation made available by Chetty and Hendren (2016), who construct Reardon's rank-order index of income segregation (Reardon 2011) at the county level using census-tract level data. To understand the impact of income segregation across counties, consider a poor county and a rich county. Assume that residential income segregation leads to consumption segregation by income at stores. A poor county that is more segregated is more likely to contain stores with higher pass-through elasticities on average while a rich county that is more segregated is likely to contain stores with lower pass-through elasticities on average. Therefore, the interaction between the log minimum wage, the Kaitz index, and the income segregation measure should be positive. Results are shown in Appendix Table G16. The interaction coefficient is positive and statistically significant as expected, providing support for the hypothesis that income segregation generates dispersion in pass-through elasticity.

G Tables

Table G1: Type and source of minimum wage changes, 2006-2015

	Source		
Type	State	Federal	Total
Ballot	11	0	11
Indexation	71	0	71
Legislation	63	77	140
Total	145	77	222

Notes: Data collected manually from online sources such as federal and state government websites, news articles, and the National Conference of State Legislatures etc. All sources are documented and available upon request.

Table G2: Summary statistics for minimum wage changes, 2006-2015

Variable	Mean	Std. Dev.	Median	Min	Max
2006-2010					
MW change (dollars)	0.58	0.33	0.70	-0.03	1.80
MW change (%)	0.10	0.06	0.11	0.00	0.35
Implementation lag (quarters)	4.13	3.76	4.69	0.00	18.57
2011-2013					
MW change (dollars)	0.19	0.10	0.15	0.06	0.37
MW change (%)	0.02	0.01	0.02	0.01	0.05
Implementation lag (quarters)	0.08	0.42	0.00	0.00	2.14
2014-2015					
MW change (dollars)	0.48	0.37	0.42	0.08	1.25
MW change (%)	0.06	0.05	0.05	0.01	0.17
Implementation lag (quarters)	1.80	1.63	1.02	0.24	7.05

Notes: Data collected manually from online sources such as federal and state government websites, news articles, and the National Conference of State Legislatures etc. All sources are documented and available upon request. Implementation lags refers to the number of quarters it took for the minimum wage to be implemented after the announcement of the legislation.

Table G3: Minimum wage impact on labor markets by industry, pooled case studies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Industry	Drug		Grocery		Department		Merchandise		Restaurant	
Earnings	0.00233	-0.0288	0.139***	0.0177	0.0344	-0.0896	0.208**	0.0554	0.280***	0.225***
	(0.0785)	(0.0777)	(0.0449)	(0.0373)	(0.0547)	(0.0738)	(0.0797)	(0.0664)	(0.0292)	(0.0285)
	88,240	83,092	92,362	90,772	50,067	30,984	87,667	82,570	92,302	90,690
Employment	0.0474	-0.150**	-0.0908	-0.0620	-0.143	0.120	-0.130	0.115	-0.133**	-0.0201
	(0.0652)	(0.0626)	(0.0812)	(0.0751)	(0.262)	(0.202)	(0.210)	(0.209)	(0.0627)	(0.0741)
	65,615	50,958	78,025	67,780	27,314	12,430	70,611	57,434	89,881	86,386
Hires	0.215**	0.0602	-0.379**	-0.224**	-0.123	0.1000	-0.277	0.173	-0.370***	-0.199**
	(0.0994)	(0.146)	(0.147)	(0.0926)	(0.268)	(0.217)	(0.180)	(0.197)	(0.0974)	(0.0988)
	58,943	42,966	77,462	66,918	27,361	12,458	69,744	56,396	89,592	85,738
Separations	0.207*	-0.0185	-0.289**	-0.194*	-0.146	0.0174	-0.224	0.166	-0.393***	-0.198**
	(0.112)	(0.142)	(0.135)	(0.101)	(0.271)	(0.205)	(0.180)	(0.180)	(0.0927)	(0.0916)
	58,586	42,530	77,278	66,660	27,312	12,428	69,593	56,164	89,358	85,398
Turnover	0.144*	0.157	-0.253	-0.144	0.00593	-0.0695	-0.124	0.0107	-0.251**	-0.167**
	(0.0803)	(0.123)	(0.167)	(0.0897)	(0.127)	(0.116)	(0.159)	(0.155)	(0.0958)	(0.0821)
	56,278	39,968	76,896	66,122	27,242	12,364	68,998	55,460	89,004	84,740
County FE	X	X	X	X	X	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X	X	X	X	X	X
County pair x period FE		X		X		X		X		X

Notes: Data from the QWI, 2006-2015. Coefficients are obtained from 50 separate regressions of the outcomes on the minimum wage under different specifications. County pair-period FE refers to contiguous county pair-period fixed effects. Robust standard errors are in parentheses, clustered by state. Number of observations are given below the standard errors. *** p<0.01, ** p<0.05, * p<0.1. Log county population is added as a control variable. Industries include drug stores, grocery stores, department stores, other merchandise stores, and restaurants as classified by 4-digit NAICS (4461, 4451, 4521, 4529, and 7225).

Table G4: Minimum wage impact on labor markets by industry and Kaitz index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Industry	Drug		Grocery		Department		Merchandise		Restaurant	
Counties	Rich	Poor	Rich	Poor	Rich	Poor	Rich	Poor	Rich	Poor
Earnings	0.0489	0.0856	0.117**	0.184***	0.0155	0.123**	0.0724	0.265***	0.204***	0.312***
	(0.0376)	(0.0579)	(0.0447)	(0.0487)	(0.0542)	(0.0574)	(0.0514)	(0.0752)	(0.0314)	(0.0361)
	59,866	55,797	61,724	60,357	43,066	23,264	60,307	55,192	61,971	60,727
Employment	0.0437	0.142*	0.0558	-0.0709	-0.138	-0.227	-0.225	-0.264	-0.0910*	-0.0791
	(0.0625)	(0.0811)	(0.104)	(0.123)	(0.131)	(0.338)	(0.158)	(0.254)	(0.0514)	(0.0677)
	52,163	35,320	56,154	45,572	27,707	7,648	53,563	39,876	60,722	57,887
County FE	X	X	X	X	X	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X	X	X	X	X	X

Notes: Data from the QWI, 2006-2015. Coefficients are obtained from 20 separate regressions of the outcomes on the minimum wage by industry and Kaitz index. Robust standard errors are in parentheses, clustered by state. Number of observations are given below the standard errors. *** p<0.01, ** p<0.05, * p<0.1. Log county population is added as a control variable. Industries include drug stores, grocery stores, department stores, other merchandise stores, and restaurants as classified by 4-digit NAICS (4461, 4451, 4521, 4529, and 7225). The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table G5: Effect of minimum wage on prices by Kaitz index quartile, grocery stores

	(1)	(2)	(3)	(4)
Kaitz quartile	1	2	3	4
VARIABLES	Log price index			
Log MW	0.0553*	0.0645***	0.0778***	0.0828**
	(0.0287)	(0.0221)	(0.0173)	(0.0347)
Observations	196,266	50,700	26,316	13,840
R-squared	0.925	0.938	0.945	0.935
Prob > F	0.039	0.000	0.001	0.004
Number of units	4912	1270	658	346
Number of clusters	45	46	40	27

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Store location is known at the county level. For each county in the sample, I measure the Kaitz index in 2006q1 and calculate the quartile each county is in, which is denoted as the Kaitz quartile.

Table G6: Proportion of revenue earned by each product department by store type

Store Type	Product department				
	Health & Beauty Care	Food	Non-Food Grocery	Alcohol	General Merchandise
Drug	0.479	0.216	0.171	0.052	0.083
Food	0.053	0.765	0.094	0.065	0.023
Merchandise	0.219	0.312	0.225	0.008	0.237

Notes: This table lists the proportion of revenue earned by each product department by store type across all stores from 2006-2015.

Table G7: Effect of minimum wage on prices and real sales by product department and Kaitz index, drug stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Counties	Rich					Poor				
Product Department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise
Log price index	-0.0100	-0.0441	-0.0768	-0.0898	-0.0556*	0.0181	0.0254	-0.293	-0.150**	-0.00437
	(0.0234)	(0.0276)	(0.0763)	(0.0795)	(0.0312)	(0.0304)	(0.0278)	(0.174)	(0.0621)	(0.0504)
Log real sales	-0.0413	-0.0310	0.283**	0.392	-0.107*	0.0130	0.0607	0.486	0.435	-0.0932
	(0.0555)	(0.0624)	(0.128)	(0.351)	(0.0628)	(0.0963)	(0.141)	(0.321)	(0.267)	(0.120)
Observations	309,842	309,802	304,802	102,458	309,642	45,136	45,136	44,536	24,552	45,136
Number of units	7751	7750	7625	2563	7746	1129	1129	1114	614	1129
Number of clusters	48	48	48	36	48	40	40	40	22	40

Notes: Coefficients are obtained from 20 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with fixed effects and controls by product department and Kaitz index in drug stores. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table G8: Effect of minimum wage on prices and real sales by product department and Kaitz index, grocery stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Counties	Rich					Poor				
Product Department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise
Log price index	0.00584	0.0459*	0.106**	0.0380	-0.0594**	0.0333	0.0575**	0.168***	0.112**	-0.0623
	(0.0160)	(0.0229)	(0.0433)	(0.0675)	(0.0223)	(0.0277)	(0.0266)	(0.0444)	(0.0529)	(0.0461)
Log real sales	0.0397	0.0131	-0.152	-0.114	0.161*	-0.0762	0.111	-0.167	0.350	0.183
	(0.0945)	(0.0578)	(0.0996)	(0.229)	(0.0833)	(0.141)	(0.0705)	(0.140)	(0.351)	(0.167)
Observations	247,806	248,206	245,766	243,286	247,646	40,276	40,236	40,076	39,436	40,236
Number of units	6198	6208	6147	6085	6194	1007	1006	1002	986	1006
Number of clusters	48	48	48	48	48	41	41	41	39	41

Notes: Coefficients are obtained from 20 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with fixed effects and controls by product department and Kaitz index in grocery stores. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table G9: Effect of minimum wage on prices and real sales by product department and Kaitz index, mass merchandise stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Counties			Rich					Poor		
Product Department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise
Log price index	-0.0198**	-0.0117	-0.0226**	0.0974	0.0858	-0.0145	-0.0187	-0.0261**	0.0285	0.116**
	(0.00864)	(0.0140)	(0.0105)	(0.0708)	(0.0540)	(0.00966)	(0.0154)	(0.0117)	(0.0934)	(0.0444)
Log real sales	0.000282	-0.0301	0.00231	-1.511*	-0.0867**	0.147**	0.359**	0.198*	0.645	-0.0212
	(0.0484)	(0.0606)	(0.0575)	(0.831)	(0.0394)	(0.0717)	(0.158)	(0.115)	(0.726)	(0.0691)
Observations	232,189	232,229	230,193	28,476	232,189	50,488	50,488	49,968	6,360	50,488
Number of units	5811	5812	5761	712	5811	1265	1265	1252	159	1265
Number of clusters	48	48	48	36	48	45	45	45	21	45

Notes: Coefficients are obtained from 20 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with fixed effects and controls by product department and Kaitz index in mass merchandise stores. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table G10: Theoretical estimates of minimum wage pass-through elasticity

Industry	Labor cost share	Weighted share of wages earned by MW affected workers	Spillover adjustment	Pass-through rate	MW pass-through elasticity
Grocery Stores	0.1005	0.0461	No	1	0.00463
Grocery Stores	0.1005	0.1284	AMS	1	0.01291
Grocery Stores	0.1005	0.2078	Lee	1	0.02088
Health Stores	0.1198	0.0204	No	1	0.00244
Health Stores	0.1198	0.0563	AMS	1	0.00675
Health Stores	0.1198	0.1004	Lee	1	0.01203
Department Stores	0.1078	0.0309	No	1	0.00333
Department Stores	0.1078	0.1133	AMS	1	0.01222
Department Stores	0.1078	0.2004	Lee	1	0.02160
Restaurants	0.2972	0.1780	No	1	0.05291
Restaurants	0.2972	0.2385	AMS	1	0.07087
Restaurants	0.2972	0.3436	Lee	1	0.10210

Notes: Pooled data from CPS MORG, 2006-2015. Shares are constructed using CPS sample weights. Almost all workers appear twice by construction of the rotating panel. Industries are classified according to the 2010 Census occupational classification used by the CPS. Labor cost shares are from 2007 and 2012 SUSB. Shares are constructed using ACS sample person weights. Spillover adjustments are made based on theoretical derivations and using spillover elasticity estimates from Autor, Manning, and Smith (2016) (AMS) and Lee (1999), normalized by the maximum percentile. Pass-through rates are taken from estimates in previous literature. The minimum wage pass-through elasticity is a multiple of the labor cost share, the weighted share of wages earned by MW affected workers, and the pass-through rate as shown by theory.

Table G11: Effect of minimum wage on shopping behavior

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Log expenditure			Coupon share			Deal share			Store brand share		
Log MW			-0.0249			0.0112***			0.0104			0.0275***
			(0.0363)			(0.00263)			(0.0132)			(0.00905)
Log MW x Below Median	-0.0176	-0.00669	0.0217	-0.00764***	-0.00643***	-0.00796***	-0.0488***	-0.0414***	-0.0496***	-0.00880**	-0.00865**	-0.0227***
	(0.0202)	(0.0197)	(0.0232)	(0.00170)	(0.00157)	(0.00174)	(0.00737)	(0.00644)	(0.00804)	(0.00364)	(0.00350)	(0.00445)
Observations	1,539,076	1,539,076	3,461,760	1,539,078	1,539,078	3,461,992	1,539,076	1,539,076	3,461,760	1,539,076	1,539,076	3,461,760
R-squared	0.733	1.000	0.707	0.768	1.000	0.584	0.852	1.000	0.703	0.690	1.000	0.445
Prob > F	0.482	0.009	0.020	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.030	0.000
Number of units	61431	61431	27984	61431	61431	27984	61431	61431	27984	61431	61431	27984
Number of clusters	49	49	49	49	49	49	49	49	49	49	49	49
State-period FE	X			X			X			X		
Additional controls		X			X			X			X	
Product department-period FE			X			X			X			X

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Observations are weighted by sampling weights and product expenditure share when applicable. Control variables, household and period fixed effects are included. Control variables include log housing price, log county unemployment rate, log county population, log county average wage, and household size fixed effects. Below median refers to an indicator variable for households with a household income below the median in 2006. Coupon share denotes the share of expenditures made using a coupon, deal share denotes the share of expenditures on goods on sale, and store brand share denotes the share of expenditures on goods that are store brand. Additional controls are household characteristics interacted with the minimum wage variable. These household characteristics include race, marital status, age, and education of the household head(s). Product department-period FE is applied to the regression that segments household expenditure by product department and uses balanced households.

Table G12: Effect of minimum wage on prices, 1% sample of products in dry grocery in grocery stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Weights		Product revenue	County revenue	County population	County-product revenue		Product revenue	County revenue	County population	County-product revenue
VARIABLES	Log price									
Log MW	0.00456	0.0240	0.0259**	0.0243**	0.0412*	0.0179	0.0240	0.0532**	0.0444**	0.0528*
	(0.0136)	(0.0208)	(0.0118)	(0.0118)	(0.0232)	(0.0178)	(0.0208)	(0.0198)	(0.0209)	(0.0270)
	[-.0138,.0181]	[-.0232,.0465]	[.00160,.0421]	[-.00160,.0426]	[-.00389,.0701]	[-.0219,.0360]	[-.0318,.0593]	[.000764,.0683]	[-.00509,.0688]	[-.0269,.0827]
Observations	10,573,758	10,573,758	10,573,758	10,561,865	10,573,758	2,306,678	10,573,758	2,306,678	2,306,189	2,306,678
R-squared	0.973	0.975	0.974	0.973	0.984	0.981	0.975	0.984	0.983	0.985
Prob > F	0.739	0.254	0.034	0.045	0.082	0.320	0.254	0.010	0.038	0.056
Number of units	747072	747072	747072	744875	747072	57656	747072	57656	57656	57656
Number of clusters	49	49	49	49	49	49	49	49	49	49
County-Product FE	X	X	X	X	X	X	X	X	X	X
Product-Period FE	X	X	X	X	X	X	X	X	X	X
Balanced						X	X	X	X	X

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Point estimates and standard errors are for a particular 1% random sample of products. 95% intervals are in brackets, constructed using 100 different random 1% samples from the entire set of products. Weights refer to regression weights using each of the specified variables, which are totals across the sample period. Balanced refers to retaining only county-product observations that appear in every period in the sample.

Table G13: Effect of minimum wage on prices by store type, substates only

	(1)	(2)	(3)	(4)	(5)	(6)
Store Type	Drug		Grocery		Merchandise	
VARIABLES	Log price index					
Log MW	0.0935***	0.0599	0.0373	0.0469**	0.0874***	0.0271
	(0.0256)	(0.0377)	(0.0254)	(0.0167)	(0.0218)	(0.0234)
Observations	18,948	14,320	20,880	15,160	7,292	4,360
R-squared	0.808	0.937	0.905	0.923	0.876	0.888
Prob > F	0.018	0.012	0.073	0.000	0.000	0.001
Number of units	474	358	522	379	183	109
Number of clusters	24	17	24	17	16	9
State-period FE		X		X		X

Notes: Robust standard errors are in parentheses, clustered by substate. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included.

Table G14: Average expenditure shares by product characteristic and income quartile across product groups

Characteristic	Non-organic	Organic	Store Brand	Name Brand
	Average Expenditure share			
Income Quartile				
1	0.258	0.172	0.292	0.241
2	0.235	0.199	0.245	0.230
3	0.243	0.247	0.233	0.247
4	0.265	0.382	0.230	0.282
Number of groups	59	59	105	105

Notes: This table presents average expenditure shares across product groups by product characteristics and household income quartile in 2006. Results are similar for later years in the sample period. Household income quartiles are constructed after controlling for household size fixed effects and results are robust to skipping this correction.

Table G15: Effect of minimum wage on prices and real sales for grocery stores by product characteristic

Product Characteristic	(1) Non-organic	(2) Organic	(3) Store Brand	(4) Name Brand
Log price index	0.0407**	-0.0307	0.0309	0.0517**
	(0.0172)	(0.0348)	(0.0244)	(0.0210)
Log real sales	0.125**	-0.00555	0.0203	0.0163
	(0.0552)	(0.197)	(0.0644)	(0.0533)
Observations	645,366	645,366	791,178	791,178
Number of units	5379	5379	6595	6595
Number of clusters	48	48	48	48

Notes: Coefficients are obtained from 8 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with control variables as well as store and period fixed effects. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table G16: Effect of income segregation on minimum wage pass-through elasticity

	(1)	(2)
VARIABLES	Log price index	
Log MW	-0.00835	
	(0.0607)	
Income segregation	5.105**	4.448**
	(2.037)	(1.678)
Log MW x Income Segregation	-1.129**	-0.707*
	(0.485)	(0.379)
Kaitz Index	-0.297	-0.119
	(0.479)	(0.318)
Log MW x Kaitz Index	0.301*	0.249***
	(0.156)	(0.0825)
Kaitz Index x Income Segregation	-15.80**	-14.02**
	(6.287)	(5.228)
Log MW x Income Segregation x Kaitz Index	3.179**	2.055*
	(1.427)	(1.154)
Observations	287,122	287,122
R-squared	0.932	0.949
Prob > F	0.000	0.000
Number of units	7180	7180
Number of clusters	48	48
State-period FE		X

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county and fixed to the value in 2006q1. The measure of income segregation used is Reardon's rank-order index from Reardon (2011) and constructed at the county level in Chetty and Hendren (2016).

H Figures

Figure H1: Comparison of Nielsen price indices with CPI

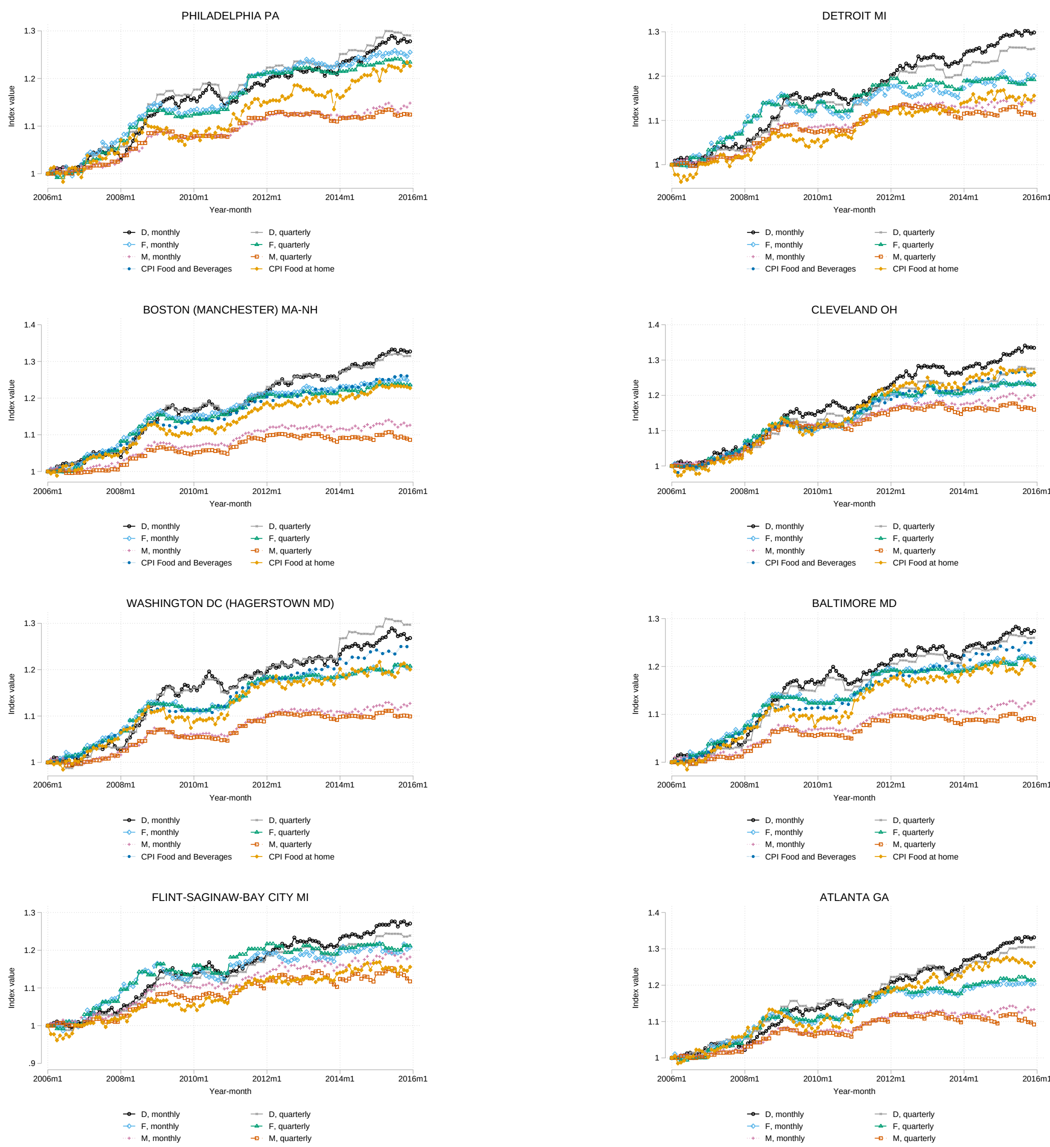
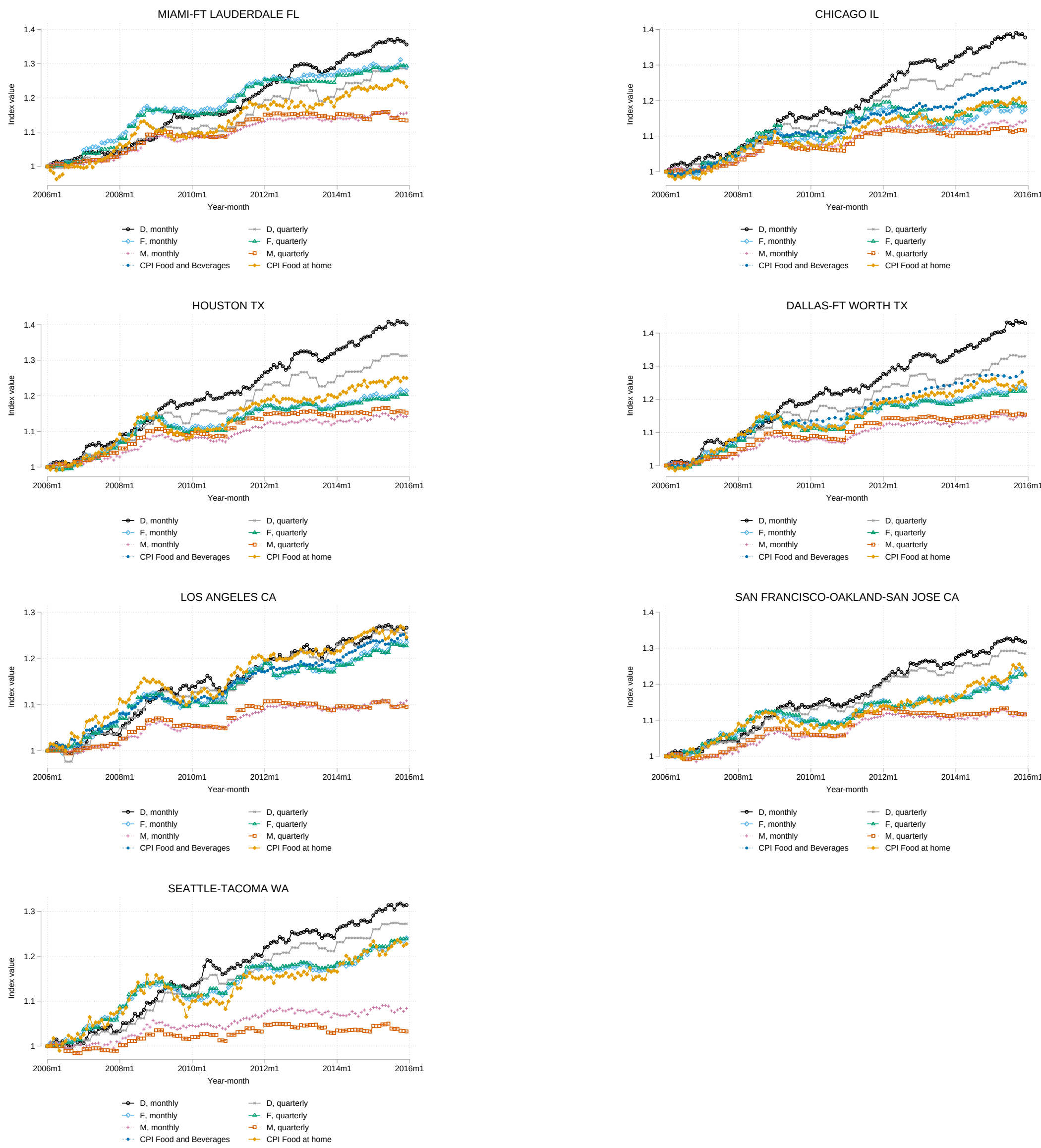
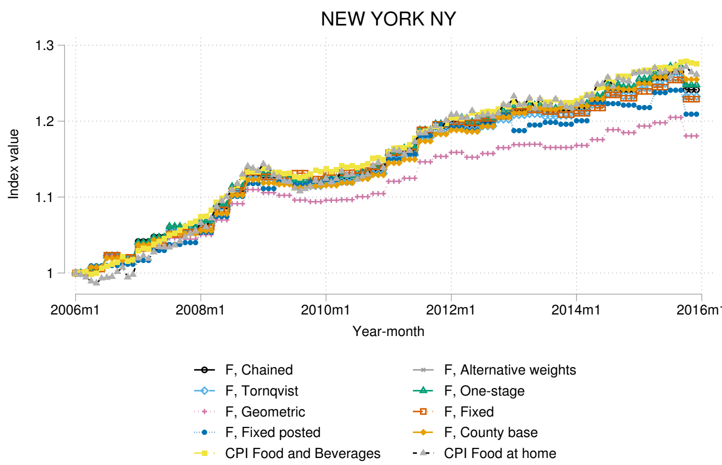


Figure H1: Comparison of Nielsen price indices with CPI



Notes: This figure plots city-level price indices from 2006-2015 constructed using Nielsen retail scanner data against those used by the BLS to construct the CPI. D, F, and M correspond to Nielsen price indices for drug stores, grocery stores, and mass merchandise stores respectively. Nielsen price indices are first constructed at the store level, and aggrgated to the city level by taking a sales-weighted average. City-level CPI price indices are publicly available and published by the BLS.

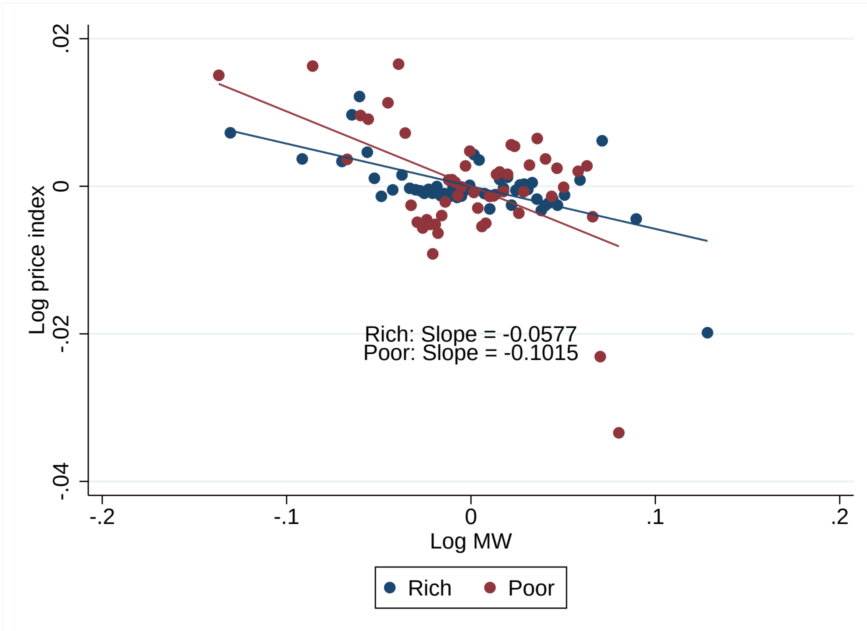
Figure H2: Comparison of Nielsen price indices with CPI, grocery stores



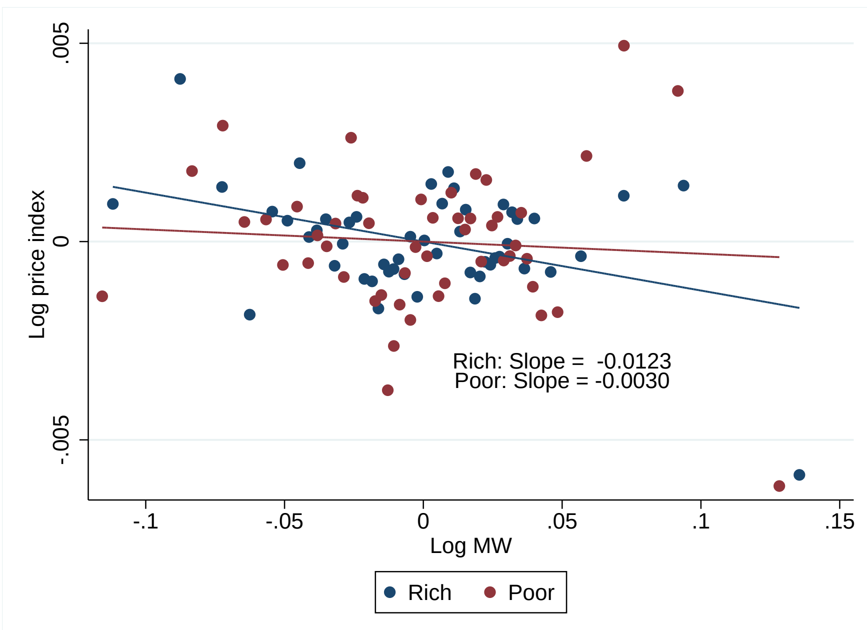
Notes: This figure plots city-level price indices from 2006-2015 constructed using Nielsen retail scanner data with alternative methods against those used by the BLS to construct the CPI. F correspond to Nielsen price indices for drug stores, grocery stores, and mass merchandise stores respectively. Nielsen price indices are first constructed at the store level, and aggregated to the city level by taking a sales-weighted average.

Figure H3: Log price index on log minimum wage, drug and merchandise stores in rich and poor counties

(a) Drug

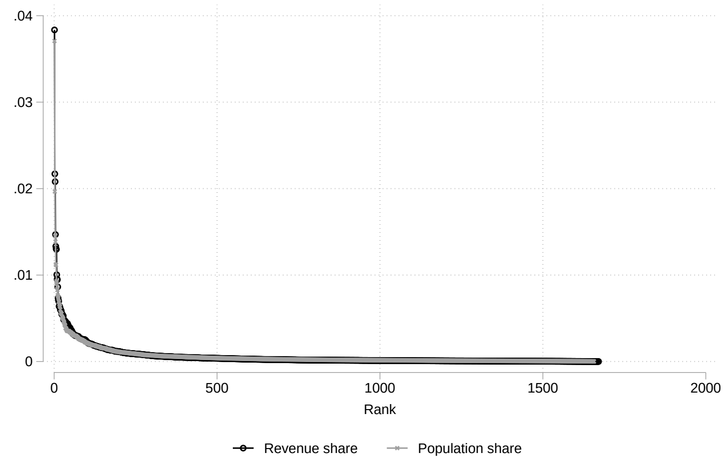


(b) Mass merchandise



Notes: This figure plots the log price index against the log minimum wage by rich and poor counties as measured by the Kaitz index. Both variables are residualized by regressing on a set of controls, store fixed effects, and period fixed effects. For each store-year-quarter observation, the residualized log minimum wage is calculated and grouped into 50 quantiles. The x-axis displays the mean of the residualized log minimum wage in each quantile. The y-axis shows the mean of the residualized log price index in each quantile. This is done separately for samples containing rich and poor counties, and the line of best fit is obtained from the regression using all observations in each sample, and the slopes are reported on the graph. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties. The sample is restricted to drug and mass merchandise stores.

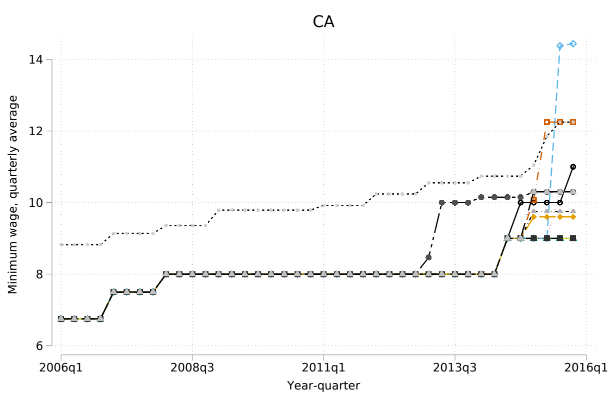
Figure H4: County revenue and population shares against their ranks



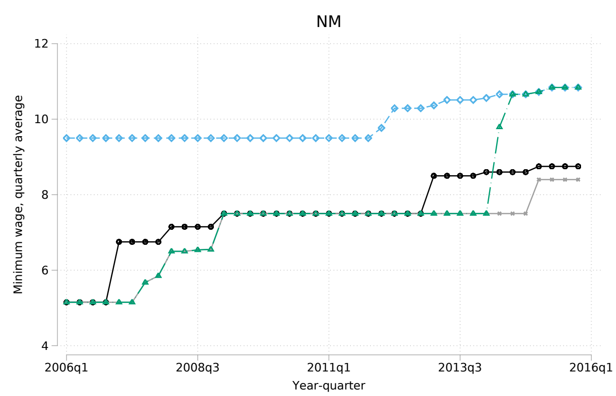
Notes: This figure plots total revenue generated by each county in a 1% random sample of products in dry grocery in grocery stores, as well as the total population in each county in the chosen sample. Both of these variables are Pareto distributed, implying that the shares have a very unequal distribution.

Figure H5: Substate minimum wages in California and New Mexico

(a) California



(b) New Mexico



Notes: This figure plots the minimum wages for substates in California and New Mexico with their own local minimum wage ordinances. Substates in California include Berkeley, El Cerrito, Emeryville, Los Angeles County, Mountain View, Oakland, Pal Alto, Richmond, Sacramento, San Diego, San Francisco, San Jose, Santa Clara, and Sunnyvale. Substates in New Mexico include Albuquerque, Las Cruces, Santa Fe, and Santa Fe County.

Minimum Wage Channels of Adjustment*

BARRY T. HIRSCH, BRUCE E. KAUFMAN, and TETYANA ZELENSKA

We analyze the effects of minimum wage increases in 2007–2009 using a sample of restaurants from Georgia and Alabama. Store-level payroll records provide precise measures of compliance costs. We examine multiple adjustment channels. Exploiting variation in compliance costs across restaurants, we find employment and hours responses to be variable and in most cases statistically insignificant. Channels of adjustment to wage increases and to changes in nonlabor costs include prices, profits, wage compression, turnover, and performance standards.

Introduction

The minimum wage (MW) and its effect on employment is the most researched and debated policy issue in American labor economics (Hamermesh 2010).[1] At first glance, this seems odd. The U.S. minimum wage is low by international standards, currently covers a tiny percentage of the workforce, and in real terms is not high compared to historic values. Economists remain fascinated with the minimum wage, however, partly because controversy continues on its empirical effects and, also, because the subject provides an opportunity to test some of the field's most basic models and propositions. We contribute to this debate by shifting analysis to what we call MW "channels of adjustment" (CoA). The CoA approach examines MW empirical effects along

*The authors' affiliations are, respectively, Department of Economics, Andrew Young School of Policy Studies, Georgia State University, Atlanta, Georgia, and IZA Bonn. Email: *bhirsch@gsu.edu*; Department of Economics, Andrew Young School of Policy Studies, Georgia State University, Atlanta, Georgia; Centre for Work, Organization, and Wellbeing, Griffith University, Brisbane Australia. Email: *bkaufman@gsu.edu*; and Innovations for Poverty Action. Email: *tzelenska@poverty-action.org*.
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[1] A December 2014 electronic search of the EconLit database on the subject "minimum wage," limited to journal articles since January 2000 and North America, brings up 138 research studies (plus 28 more for "living wage").

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multiple dimensions, with an eye toward adjustment channels in which competing models can yield different predictions.

The event we examine is the three-step increase in the U.S. minimum wage, from $5.15 to $5.85 in July 2007, to $6.55 in July 2008, and to $7.25 in July 2009, an increase of $2.10 (41 percent). Among the CoA examined are employment, hours, prices, profits, training, work effort, human resource practices, operational efficiencies, and internal wage structure. The empirical evidence comes from a unique multi-part dataset collected for quick-service restaurants located in Georgia and Alabama and owned by three franchisees. Our investigation starts with the most intensively examined adjustment channel, changes in employment and hours. We rely on large exogenous variation across the restaurants in the "bite" of the MW to identify causal effects on employment and hours. Although the dataset is neither large nor problem free, it nonetheless provides advances in several respects. First, our measure of the MW payroll "gap" (the treatment) is calculated from *individual* worker payroll data provided by the franchisees, an improvement over store-level averages or industry-level aggregates used in other studies (Card and Krueger 1994; Dube, Naidu, and Reich 2007), allowing us to measure precisely each restaurant's compliance costs resulting from each MW hike. Second, the data are from store-level electronic payroll records and thus relatively free of measurement error. Third, the data extend over 3 years, containing information before and after each of the three MW increases, and thus reflect both short- and medium-run adjustments.

Information on other channels of adjustment comes from data provided by franchise owners, a separately administered written survey of restaurant managers, and qualitative/anecdotal data collected from field-level interviews with restaurant owners and managers. We also construct a statistical profile of workforce characteristics based on a survey of individual employees. In what follows, we describe data sources, survey instruments, estimation strategies, evidence on employment and nonemployment adjustment channels, and implications for MW analyses. Given space constraints, we focus throughout on empirical analysis and only at the end, and then very briefly, consider theoretical implications.

Empirical Analysis: Setting the Stage

For space reasons the voluminous literature on the MW is not surveyed, apart from a broad overview of issues related to estimation of MW employment effects. The early MW literature consisted primarily of national time-series studies examining employment responses to changes in the federal MW. Such studies typically found negative but small teenage employment elasticities with respect to the MW, on the order of –0.1 to –0.3 with more recent studies located toward the lower end (for surveys, see Brown 1999; Brown, Gilroy, and Cohen 1982; Card and Krueger 1995; Neumark and Wascher 2008).

More recent studies rely on cross-sectional as well as time variation in the MW (see Belman and Wolfson [2014] for an in-depth review). Cross-section variation stems from two principal sources. One is the difference in prevailing wages across markets; hence MW has greater bite in low-wage markets. The second is growth in the number of states with state minima exceeding the federal minimum. Thus, introduction of federal MW increases should not impact (or barely impact) markets with high state minima, while having larger impacts on other states.

Two broad groups of cross-section, quasi-experimental studies have emerged. The first uses the Current Population Survey (CPS) or other national survey data to examine how employment responses vary with exposure to state and federal MW laws. Applying panel techniques to estimate employment effects for teenagers or low-skilled workers, these studies typically (but not always) obtain evidence of adverse employment effects, with employment elasticities on the order of –0.2 to –0.3 (Burkhauser, Couch, and Wittenburg 2000: Neumark and Wascher 2007; Sabia 2009; see Orrenius and Zavodny [2007] for an exception).

Recent nationwide MW studies focus on controls for unobserved heterogeneity in employment growth and the presence of spatial correlation across states (Allegretto, Dube, and Reich 2011) and state borders (Dube, Lester, and Reich 2010). Addison, Blackburn, and Cotti (2012) examined county-level employment in the restaurant-and-bar sector from 1990–2006 and include trends in employment. Each of these studies find minimal MW employment effects, concluding that negative effects found in prior studies result from heterogeneity in employment trends. Addison, Blackburn, and Cotti (2013) examined the 2007–2009 MW increases using three national datasets and found little evidence for employment effects or a "recession multiplier" of such effects. Neumark, Salas, and Wascher (2014) sharply questioned results from these studies, arguing they are sensitive to specification and that in avoiding one potential set of problems, researchers are discarding much of the information that can provide valid identification of MW effects. Allegretto et al. (2013) responded to these criticisms.

A second group of cross-section studies, of which our work is an example, uses either a unique data source with special advantages and/or data that permit study of MW differences in nearby markets. Best known is Card and Krueger (1994), who uncovered small positive or insignificant employment effects in a sample of fast-food restaurants in New Jersey where the MW was raised through state law, relative to the stores in nearby Pennsylvania where the MW did not change. Studies following in this tradition typically have found small and insignificant employment effects from the MW. For instance, Dube, Naidu, and Reich (2007) investigated the effects of a citywide minimum wage in San Francisco, relative to the neighboring Alameda County, and did not detect any significant employment loss attributable to the mandate.

In a paper more closely related to our study, Giuliano (2013) used 1996–1998 individual employee personnel records from seven hundred stores of a nationwide retail chain, exploiting geographic variation in state minimum wages and the employment impact of federal minimum wage increases.[2] Consistent with dynamic monopsony theories linking higher wages to fewer vacancies, she found little evidence of negative employment effects, but did find differential employment responses for types of workers (teenagers versus adults) and across geographic areas (high- versus low-income areas). While the MW had little effect on adults, it increased teen labor force participation and attracted more qualified teens, increasing their employment at the chain's stores.[3]

Note must also be given to the findings of three meta-regression analyses of MW employment effects, all of which conclude that once publication bias is removed the wage elasticity is close to zero (Belman and Wolfson 2014; De Linde Leonard, Doucouliagos, and Stanley 2014; Doucouliagos and Stanley 2009). Thus, despite an extensive body of empirical work of increasingly high quality, there is still considerable disagreement over the sign and strength of MW employment effects. Although we do not solve the puzzle, we present new evidence consistent with an approximately zero employment effect and utilize a "channels of adjustment" framework, originally advanced by institutional economist Richard Lester (1960, 1964) to explain this finding.

Data and Sample Description

We use complementary datasets for a sample of eighty-one quick-service restaurants (QSRs) in Georgia and Alabama. The primary data used to investigate employment effects come from restaurants' biweekly electronic payroll records on individual employees, collected by the authors for the period January 2007 through December 2009. Because the QSR sector has a low-wage workforce and neither Georgia nor Alabama has a binding state MW law, these restaurants are good candidates for investigating the effects of the three-step federal minimum wage increases. If MW laws have substantive and relatively uniform effects on employment, these should be evident among the most-affected businesses in our

[2] In contrast to our study, Giuliano (2013) had a large nationwide sample that is company rather than franchisor owned. She does not have information on individual hours worked.

[3] A similar result was found in Ahn, Arcidiacono, and Wessels (2011). The authors developed a search model with endogenous labor supply. They found evidence that employment for teens from more-privileged families pushes out those less privileged.

survey. We explored other MW channels of adjustment qualitatively using our survey of store managers, which in turn we supplemented by data obtained from confidential employee surveys and information from semi-structured interviews with store owners and a sample of managers.

Payroll data. The restaurants in our sample belong to a national fast-food chain and are operated by three franchise owners who agreed to provide payroll data for our study under condition of strict confidentiality.[4] Although our sample is nonrandom, it is likely to be representative because the products offered at fast-food restaurants are uniform and employees' skill sets are highly similar. Sampled establishments display considerable geographic and city size variation: twenty of our eighty-one restaurants are located in twelve eastern Alabama counties close to the Georgia border and the rest are located in twenty-three Georgia counties scattered across central and southern Georgia; likewise, some restaurants are in small rural communities or along interstate highways while others are in medium- or large-size cities. The spatial differences provide variation in the expected impact of the minimum wage across stores and time periods.

Electronic payroll data provide the following information for each worker for two pay periods per month: restaurant I.D., individual worker I.D., job title, regular hours worked and regular pay, overtime hours and overtime pay, and total pay. The straight-time wage rate was reported directly or calculated by dividing regular pay by regular hours. Payroll data for managers were not available and they are excluded from our analysis. Fringe benefits for hourly employees were close to nil and are not analyzed.

We have complete payroll records for establishments over 3 years (seventy-two "biweekly" pay periods), commencing in January 2007.[5] Six stores enter our sample later (one store opened in May 2007 and one in January 2008; four more stores were acquired by the owner in May 2007). None of the stores went out of business during the study period. Also provided were data on monthly percentage changes in sales.[6]

Pay setting and employment at restaurants follows a two-tier process. Owners establish the basic parameters of pay policy for all stores as a group,

[4] We refer to "owners" throughout the paper; however, one of the three was the chief operating officer rather than owner. Attempts to gain data from other franchise owners and the national chain were unsuccessful.

[5] Although we refer to the payroll records as biweekly, there were two per month with slight variation in the number of days, but averaging just over fifteen.

[6] We requested data on the monthly *level* of sales but for confidentiality reasons two owners provided only *changes* in sales. Monthly percent changes in sales are transformed into log points to make them additive over time.

TABLE 1

SUMMARY STATISTICS: INDIVIDUAL-LEVEL BIWEEKLY PAYROLL RECORDS

Payroll Record	N	Mean	St. Dev.
2007			
Hourly wage rate	63,164	6.28	0.95
Regular hours	63,716	49.18	22.58
Overtime hours	63,716	0.74	3.38
2008			
Hourly wage rate	64,764	6.68	0.84
Regular hours	65,291	49.27	22.30
Overtime hours	65,290	0.63	3.15
2009			
Hourly wage rate	63,484	7.15	0.69
Regular hours	63,972	49.18	21.67
Overtime hours	63,972	0.45	2.76

principally by giving each store manager a constraint on total labor cost as a percent of sales. Each manager then works out a starting wage and wage hierarchy that stays within the target while fitting in with local labor-market conditions. Owners, as a first principle, prefer to enforce the target while letting managers search out their own solutions for hiring, scheduling, turnover, filling vacancies, and morale problems. Nonetheless, owners allow case-by-case deviation from the target if warranted by the local labor market.

Descriptive statistics for payroll records data are presented in Table 1. The sample contains about 64,000 individual-level observations on biweekly pay. The average hourly wage for the first year is $6.28, increasing to $6.68 and $7.15 during the next two years (January to December, with MW increases on July 1).[7] Regular hours worked are stable across the study period. Overtime work declined; the share of employees with overtime decreased from 12 percent in 2007 to 7 percent in 2009 (not shown) and average overtime fell from 0.74 to 0.45 hours.[8] About 15 percent of our restaurants (twelve of eighty-one) are located in the Atlanta metro area, where wages and cost of living are higher. These restaurants have fewer employees per establishment, but they have higher wages, longer hours, and are more likely to be full time.[9]

[7] None of the stores utilized the youth or training minimum wage, which sets a lower minimum wage for new employees under age 21 for the first 60 to 90 days of employment.

[8] A decline in overtime hours can result from a business downturn or as a response to minimum wages. If average hourly earnings and hours are determined jointly in an implicit contract, then a MW mandate raising the straight-time wage would reduce use of overtime hours in order to (roughly) maintain average hourly earnings. For theory and evidence, see Trejo (1991) and Barkume (2010).

[9] In June 2009, prior to the July 1 MW increase from $6.55 to $7.25, the mean hourly wage for workers in the Atlanta subsample was $8.03, compared to $6.94 for hourly employees elsewhere.

Although limited in its geographic focus, our dataset possesses several advantages. First, hours and wages are measured at the individual rather than establishment level. The data allow us to construct precise measures of each restaurant's compliance cost from the MW, referred to as *GAP*, since we know each worker's wage at the time of the MW increase. A second advantage is the presence of information on regular and overtime hours worked. Finally, payroll data should be accurate because they are collected for tax-reporting reasons. The payroll data were recorded prior to and independent of our research.

Payroll data are supplemented with county-level data from the Quarterly Census of Employment and Wages (QCEW), produced by the Bureau of Labor Statistics (BLS). Measures from the QCEW are used to control for business and labor-market fluctuations at the local level. We extract 2007–2009 data on total employment (as well as number of establishments and wages) for all industries and then separately for Accommodation and Food Services (NAICS sector 72) and Retail Trade (NAICS 44–45). We also compile data at the 3-digit (Food Service and Drinking Places, NAICS 722) and 4-digit (Limited-service Eating Places, NAICS 7222) levels. We used population estimates from the U.S. Census Bureau to compute annual population density at the county level.

Manager and employee surveys. To examine a broader range of firm behavior, we use data collected from written surveys of managers and a survey of employees (not seen by employers). Questionnaires were administered in mid-July through early-August 2009.[10]

Managers first were asked a series of open-ended questions about cost-saving strategies in different areas of business operation, including human resource (HR) practices, operational efficiency, nonlabor costs, and customer service. The goal was to let managers express their own views about the MW mandate. For a second section of the survey, we designed a comprehensive list of possible cost-saving responses to the MW. The list was based partly on alternative theoretical models and partly on face-to-face discussions with managers and franchisee owners. The goal was to document which internal adjustments used by managers might be most effective in mitigating cost increases from the MW.

A portrait of employee demographics can be seen in Table 2, based on the separate survey given to employees (they can be linked to their store but, for confidentiality concerns, not to their individual payroll records). Hourly employees are disproportionately female (66 percent) and black (64 percent).

[10] The questionnaires are available from the authors on request. The employee surveys were anonymous and cannot be matched to individual payroll records. The manager survey response rate was 81 percent (sixty-six of the eighty-one managers) and employee response rate 62 percent (1649 of 2640 returned surveys that answered at least one question).

TABLE 2

Worker Characteristics from the Employee Surveys

Variable	N	Mean	St. Dev.
Gender (female=1)	1649	0.657	0.475
Race			
White	1595	0.207	0.405
Hispanic	1595	0.082	0.275
Black	1595	0.644	0.479
Asian	1595	0.053	0.225
Other	1595	0.014	0.117
Marital status			
Single	1451	0.686	0.464
Married	1451	0.175	0.380
Divorced/widowed	1451	0.054	0.226
Living with partner	1451	0.085	0.280
No. of children under 18	1625	0.958	1.300
Age	1628	28.194	10.719
School in Fall (=1)	1623	0.340	0.474
Level of schooling			
Some high school	1611	0.273	0.446
High school grad/GED	1611	0.475	0.500
Some college	1611	0.220	0.414
College graduate	1611	0.032	0.175
Health insurance (=1)	1618	0.406	0.491
Country of origin			
U.S.	1551	0.917	0.276
Mexico	1551	0.050	0.219
Other	1551	0.033	0.178
Wage in June 2009	1555	6.987	1.416
Average hours per week	1568	29.510	8.309
Tenure at store (months)	1571	26.812	39.992
No other jobs	1592	0.832	0.374
Total annual family income			
Less than 10,000	1541	0.382	0.486
10–20,000	1541	0.263	0.440
20–50,000	1541	0.276	0.283
>50,000	1541	0.079	0.115
Vote “yes” for MW (=1)	1607	0.912	0.283

Relatively few are Hispanic (8 percent). The average age is 28 and only 23 percent are teenagers (not shown). Respondents report low family incomes; 38 percent with annual family income less than $10,000 and an additional 26 percent with income between $10,000 and $20,000. Other worker attributes can be seen in the table.[11]

[11] Although economists are often opposed to raising the minimum wage, these employees are strongly supportive. The employee survey asked respondents if they would have voted “Yes” or “No” to raise the MW from $6.55 (2008) to $7.25 (2009). Nine out of ten marked “Yes.”

Minimum Wage Compliance Costs and Descriptive Evidence from Payroll Records

In this section, we describe our measure of restaurants' MW compliance costs, followed by presentation of descriptive evidence relating these costs to changes in wages, employment, and hours.

Measuring MW compliance costs (GAP). Our identification strategy uses the minimum wage policy change and its effect on the establishment-level wage bill as a source of exogenous variation. In order to identify the effect of MW increases on wages, employment, and hours worked, we construct the variable *GAP*, a measure of MW compliance costs. Although other studies have used related measures (Card and Krueger 1994; Dube, Naidu, and Reich 2007; Giuliano 2013; Katz and Krueger 1992; plus earlier studies cited in Brown, Gilroy, and Kohen 1982), our measure has advantages due to the availability of store payroll data on individual workers' wages and hours.

Specifically, GAP_{jt} is constructed using data from individual worker i at restaurant j at pay period t. It measures a restaurant's compliance costs by the log change in unit j's wage bill resulting from a MW increase, assuming individual workers' hours h stay fixed between periods $t–1$ and t (before and after the minimum wage increase). The *GAP* for restaurant j at time t is defined as:

$$GAP_{jt} = 1 + \left(\left[\sum MW_{ijt}h_{ijt-1} - \sum W_{ijt-1}h_{ijt-1}\right] / \sum W_{ijt-1}h_{ijt} - 1\right),$$

where the parenthetical term is summed over i workers for whom $W_{ijt-1} < MW_t$ (i.e., those for whom MW_t is binding) while set at 0 for workers for whom $W_{ijt-1} \geq MW_t$. The numerator in the parenthetical term is the change in the wage bill between time periods $t–1$ and t if hours remain the same, while the denominator is the initial wage bill. *GAP* is calculated by summing each employee's additional earnings (wages times hours) required for MW compliance. Adding 1 to the parenthetical term converts the measure from a proportion to a wage ratio (say 1.10 or 10 percent). The natural log of *GAP* is a "proportion" (e.g., ln(1.10) = 0.095) based on an intermediate base between the initial wage and subsequent minimum wage. If restaurant j in period $t–1$ were paying all employees above the new MW effective in period t, then *GAP* = 1 (and ln*GAP* = 0). We use ln*GAP* in double-log models to estimate employment and hours elasticities with respect to *GAP* and, separately, to instrument the wage and obtain wage elasticity estimates.

Table 3 presents summary statistics for *GAP* expressed as a percentage (by subtracting 1 and multiplying by 100), defined at the establishment level as an average over March–May of each year prior to the July 1 minimum wage increases. As larger shares of workers were affected each year, the compliance

TABLE 3

MW Compliance Cost, Share Affected, and Payroll Increase

	2007	2008	2009
Compliance cost (mean GAP) as % of payroll			
Mean	2.6	4.6	6.8
S.D.	2.1	2.6	2.6
Min	0.0	0.0	0.1
Max	8.2	9.2	10.6
Share of workers affected by MW increases (as %)			
Mean	49.2	71.5	82.2
S.D.	29.2	26.0	20.3
Min	0	0	6.6
Max	93.5	97.9	100
Total increase in payroll (%)	6.0	6.6	7.9

Notes: Means are calculated for the same 81 restaurants in 2008 and 2009, and 79 in 2007. Excluded in 2007 was a store that opened in January 2008 and another that opened in May 2007 with unusually high employment during the opening period. See the text for description of *GAP*. The payroll increase measures hold total work hours constant.

cost of the MW grew, from 2.6 percent of payroll as a result of the July 2007 MW, to 4.6 percent in 2008, and to 6.8 percent in 2009. Total payroll increases realized, holding hours constant, were 6.0 percent, 6.6 percent, and 7.9 percent.

Our gap measure is similar but not identical to those in prior studies. In their seminal paper, Card and Krueger (1994) defined *GAP* as a proportional increase in the store's initial *starting* wage necessary to meet the mandated rate. According to their definition, *GAP* reduces to the wage ratio of the new minimum wage rate and the wage for a new employee several months before the increase (MW_t / W_{t-1}). This variable provides imperfect information on compliance cost because we do not know how many workers are affected by the MW or by how much, nor do we know the work hours over which employees will be awarded the higher wage. Rather, Card and Krueger's "wage gap" is a proxy for a relatively high- or low-cost store location. Recognizing such issues, Dube, Naidu, and Reich (2007) defined their measure as a share of workers affected by the MW increase, as used previously in Card (1992) (we include this measure in Table 3). Although the share measure accounts for the *quantity* dimension of the MW (i.e., the number of workers whose wages must rise), not accounting for the *price* dimension (i.e., by how much wages must increase) may generate an imprecise measure of the cost shock from a MW mandate. Similarly, using establishment-level averages of wages and hours may provide a noisy measure because workers with wages well above the new MW would effectively "cancel out" affected workers (i.e., businesses with equivalent average wages will have different compliance costs). As our *GAP* measure is based on individual data we are able to capture both quantity and cost per employee.

FIGURE 1

Average Hourly Earnings, Employment, and Hours, January 2007–December 2009

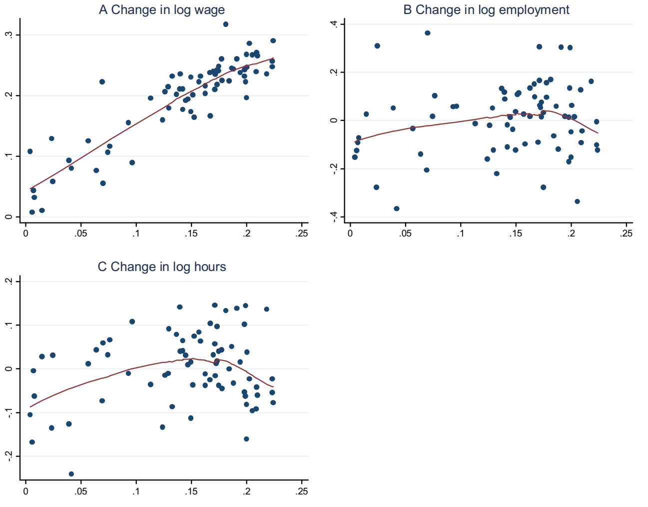
Notes: Shown are establishment averages for biweekly hourly earnings, biweekly employment, and regular and overtime daily hours for three groups of stores by biweekly pay period. "Least" affected stores include those where *GAP* is in the bottom 25 percentiles, "Middle" the 25th–75th percentiles, and "Most" percentiles 75 and over. Vertical lines mark MW increase dates for increases to $5.85, $6.55, and $7.25 in July 2007, 2008, and 2009.

Descriptive evidence on wages, employment, and hours worked. Evidence on how wages, employment, and biweekly hours varied with respect to the minimum wages is visually summarized in Figures 1 and 2. Figure 1 shows how wages, employment, and hours varied over the 3-year period among groups of restaurants least affected, moderately affected, and most affected by MW increases. Figure 2 shows the size of long-run (early 2007 to late 2009) individual restaurant changes in wages, employment, and hours with respect to their MW compliance costs. Both sets of figures illustrate clear-cut systematic effects of the MW mandates on wages, coupled with responses in employment and work hours that are variable and far less systematic.

Average employment changes across all establishments would mask variation associated with differences in pre-MW wage levels. In Figure 1, establishments are divided into three groups—those least, moderately, and most affected by the MW increases based on the size of *GAP* in March–May 2007, prior to the initial July 2007 increase. The "least" affected group represents the lowest twenty-five percentiles of *GAP* and includes all zero *GAP* stores (no compliance costs from the 2007 MW). The "moderately" affected group has *GAP* values between the twenty-fifth and seventy-fifth percentiles of *GAP*,

FIGURE 2

Three-Year Log Changes in Wages, Employment, and Hours with Respect to MW GAP



Notes: The vertical axis measures log changes from March–May 2007 to October–December 2009. The horizontal axis measures MW compliance costs using the cumulative 3-year ln*GAP*.

while the "most" affected group has *GAP* compliance costs in the seventy-fifth and over percentiles.

Figure 1 provides plots of biweekly establishment-level averages in observed hourly earnings and employment over the 3-year period, plus average daily regular and overtime hours. In panel A, there are three distinct jumps in average hourly earnings in July of each year, coinciding with the three MW hikes. The least affected group of stores is largely unaffected since the average wage there was slightly above $7 during the whole study period. Importantly, there is no evidence of pre-adjustments in hourly wages prior to MW increases (confirmed by owners). Small increases around January–February in 2007 and 2008 are attributed to performance-based raises for a portion of the workforce. The final MW increase in July 2009 has the strongest effect across all three groups. The three federal MW hikes had substantial exogenous effects on average hourly earnings and the intensity of these treatments differed substantially across restaurants. Estimates of arguably causal employment and hours effects can be based on outcome differences between restaurants facing different compliance costs.

Panels B–D of Figure 1 show differences in outcomes between the least, middle, and most affected restaurants. Panel B presents average employment.

Despite jumps in the average hourly earnings observed in panel A, we do not observe reductions in employment that correspond with the size of compliance costs. Overall patterns appear roughly similar among the three groups. Although we can see a small decline in employment after the July 2008 hike, employment is fairly stable both within and across the three groups of establishments facing varying cost shocks. The least affected stores have fewer employees on average due to greater use of full-time workers among Atlanta-area establishments. Some seasonal fluctuations in the average employment are also evident; there are systematic increases in employment (and turnover) twice a year (in June–July and December–January), which owners attribute to long vacation leaves (some of which show up as turnover in our data) and voluntary turnover.

Panel C shows average daily regular hours per worker across the three groups of establishments.[12] Although regular hours fluctuate on a pay-period basis, there is no apparent trend in average hours worked over the study period, nor are there apparent differences in trend among the three groups. Panel D shows overtime hours. Although average daily overtime is minimal (a small fraction of an hour), there are differences among the three groups of restaurants. In the two most affected groups, overtime is practically nonexistent, but is higher on average in the least affected group where there are more full-time workers. There is a decline over time in overtime hours, due primarily or in part to weak business conditions.

Figure 2 provides panels showing 3-year "long-run" changes in wages, employment, and total hours (regular plus overtime) with respect to the cumulative 3-year compliance costs (the sum of ln*GAP*). As seen in the figures, restaurants' 3-year compliance costs varied from close to zero to about 25 percent, with a sizable share of the restaurants having wage bill mandates between about 13 and 23 log points. In each panel, a nonparametric curve (using smoothed local regressions) is fitted to the array of restaurant data points.[13] As expected, the wage–GAP relationship is upward sloping with a tight fit between changes in ln*W* and ln*GAP*. As seen previously in Table 3, increases in restaurants' total wage bills exceeded legally required increases, but not by much.

In contrast to the pattern on wages, panels B and C of Figure 2 indicate little systematic relationship between MW compliance costs and the changes in

[12] Average daily hours are shown to account for slight differences in length of the twice-a-month payroll periods.

[13] The curves are fitted from locally weighted regressions using the "lowess" command in Stata (Cleveland 1979). Figures showing untransformed rather than log values of wages, employment, and hours are highly similar.

employment and total hours. Rather than sloping downward with respect to costs, changes in employment average close to zero across the range of compliance costs. For total hours (panel C), in the businesses with the highest compliance costs (about 18 + log points), one sees a corresponding decline in hours, but throughout the rest of the distribution (gaps below 18 log points) one sees more of a positive than a negative hours–wage relationship.

The graphical analysis shows that despite substantial differences in wage gains due to the three-step MW increases, there are rather limited differences in the employment or hours responses between those restaurants most and least affected by the mandates. Below we examine MW effects on employment and hours within a formal regression framework.

Estimating MW Employment and Hours Effects From Payroll Records

The empirical analysis of MW effects on employment and total work hours uses biweekly establishment averages created from the payroll data. We initially estimate the following reduced-form equations, estimated separately for each of the three MW increases, with MW effects based on restaurant-specific compliance costs of the MW (i.e., ln*GAP*). We then estimate "structural" equations in which log wages are instrumented by the ln*GAP* prior to each MW increase. Using a similar approach, we then provide estimates of MW employment and hours effects over the full three year period.[14]

$$\ln E_{jct} = \alpha + \psi \ln GAP_{jt}MW_t + \tau \ln GAP_{jt} + \delta MW_t + \varsigma \Delta \ln Sales_{jt-1} + \gamma Z_{ct} + \theta F_j + \varepsilon_{jt} \tag{1a}$$

$$\ln h_{jct} = \alpha + \psi \ln GAP_{jt}MW_t + \tau \ln GAP_{jt} + \delta MW_t + \varsigma \Delta \ln Sales_{jt-1} + \gamma Z_{ct} + \theta F_j + \varepsilon_{jt} \tag{1b}$$

The outcome variables are $\ln E_{jt}$, the log of the average employment in store j during period t (biweekly), and the log of aggregate hours h_{jt}, the sum of regular plus overtime hours. MW_t is the time treatment period set to 1 for the 5–6 months *after* each MW increase (i.e., August–January in 2007 and 2008; August–December in 2009). We provide estimates with and without store fixed

[14] This approach is similar to that used by Card (1992), whose state-level analysis presents both reduced-form employment estimates, based on each state's fraction of teens affected by the MW, and structural estimates, in which the fraction affected by MW is used to instrument wage change.

effects (FE); the fixed effects F_j absorb any impacts specific to the business owner and county location, as well as the standalone store-level measure, $\ln GAP_{jt}$. The error term is ε_{jt}.

The key variable is the interaction term $\ln GAP \bullet MW$, whose coefficient ψ measures the impact of the cost increase from the MW mandate on establishment employment (hours) averaged over the months following the increase. The parameter ψ provides a measure of the employment (hours) elasticity with respect to the exogenous required wage change. We next estimate a structural model that includes the predicted wage; $\ln GAP$ proves to be a strong instrument (F test>100). Based on standard models, competitive theory predicts $\psi < 0$, while monopsony and institutionalist/behavioral theories allow for values of ψ that are negative, near zero, or positive (e.g., Manning 2003; Kaufman 2010).[15]

Regressions without store FE include $\ln GAP_{jt}$ entered separately, which controls for differences in employment (hours) levels between high- and low-impacted restaurants prior to the MW mandate. $\Delta \ln Sales_{jt-1}$ is the lagged monthly change in log sales, included to reflect demand shocks (reflected in prices, product mix, and transactions) not captured by other controls. The lag enables us to exclude the current month and avoid simultaneity between concurrent employment and sales. The vector Z_{ct} includes time-varying county-level characteristics reflecting labor market supply and demand factors; specifically, county-level population density and total private sector employment minus employment in the Accommodations and Food Services and Retail sectors.

A few econometric issues warrant mention. The error term is likely to be correlated for each restaurant over time; hence, all standard errors are clustered on individual stores. Apart from clustering, some degree of independence in employment, pay, and price setting across each owner's stores mitigates concern about franchise-specific effects that would overstate the precision of our estimates (Donald and Lang 2007). The possibility of heterogeneous trends across stores is a potential source of bias if correlated with $\ln GAP$, a concern mitigated by controls for time-varying county-level demand-supply shifters.

Regression evidence. The models estimating MW effects on wages, employment, and hours for each of the three MW increases are shown in Table 4. Average wages, employment, and hours are measured in each biweekly pay period for the

[15] Elasticity estimates in the MW literature are far smaller than are elasticity estimates in the larger labor demand literature (Hamermesh 1993). This is not surprising given that a small proportion of workers (including teens) are affected by any given MW increase and, even among those affected, compliance costs are low for those whose current wage is close to the mandated wage. Estimates of ψ from our structural model can be interpreted as wage elasticities; those from the reduced-form model approximate elasticities to the extent that wage increases mimic legally required increases. Thus, elasticity estimates here are potentially larger than in much of the MW literature.

TABLE 4

WAGE, EMPLOYMENT, AND AGGREGATE HOURS EFFECTS OF THE 2007, 2008, AND 2009 MW INCREASES

	ln(Wage)			ln(Employment)				ln(Total Hours)			
Description	(1) OLS No FE	(2) OLS No FE	(3) OLS Store FE	(4) OLS No FE	(5) OLS No FE	(6) OLS Store FE	(7) IV Store FE	(8) OLS No FE	(9) OLS No FE	(10) OLS Store FE	(11) IV Store FE
A. 2007:											
lnGAP'07 x MW'07	0.968*** (0.149)	1.102*** (0.144)	1.148*** (0.105)	1.034* (0.590)	0.446 (0.618)	0.099 (0.500)	0.087 (0.422)	0.606 (0.423)	0.298 (0.429)	–0.187 (0.307)	–0.163 (0.261)
lnGAP'07	–3.860*** (0.380)	–3.502*** (0.336)		3.589** (1.637)	1.921 (1.484)			1.976** (0.875)	0.570 (0.900)		
MW'07 (Aug07–Jan08)	0.021*** (0.006)	0.015*** (0.005)	0.012*** (0.004)	–0.043** (0.021)	–0.033 (0.020)	–0.011 (0.017)	–0.012 (0.021)	–0.019 (0.016)	–0.017 (0.016)	0.004 (0.011)	0.006 (0.014)
Δln(sales), lagged 1 month		0.017** (0.007)	0.010*** (0.003)		0.014 (0.047)	0.044 (0.045)	0.043 (0.043)		0.183*** (0.053)	0.189*** (0.053)	0.191*** (0.051)
County pop density (log)		0.021** (0.010)	0.043 (0.114)		0.020 (0.039)	–0.772 (0.679)	–0.776 (0.668)		0.024 (0.033)	–1.578*** (0.540)	–1.571*** (0.518)
County other private emp (log)		0.005 (0.011)	0.018 (0.034)		–0.112** (0.043)	0.044 (0.155)	0.043 (0.150)		–0.088** (0.034)	–0.155 (0.116)	–0.152 (0.113)
Constant	1.925*** (0.015)	1.748*** (0.069)	1.303 (0.814)	3.394*** (0.053)	4.490*** (0.296)	8.155* (4.643)	8.043* (4.440)	7.352*** (0.032)	8.164*** (0.212)	19.277*** (3.876)	19.490*** (3.732)
Observations	2013	1843	1843	2013	1843	1843	1843	2013	1843	1843	1843
R-squared	0.624	0.706	0.975	0.106	0.272	0.890	0.889	0.074	0.262	0.840	0.839
B. 2008:											
lnGAP'08 x MW'08	1.117*** (0.053)	1.105*** (0.052)	1.115*** (0.052)	–0.268 (0.344)	–0.192 (0.350)	–0.292 (0.348)	–0.262 (0.302)	0.165 (0.175)	0.177 (0.181)	0.147 (0.177)	0.132 (0.156)
lnGAP'08	–2.788*** (0.191)	–2.634*** (0.147)		5.654*** (1.241)	3.946*** (1.242)			2.763*** (0.879)	1.279 (0.909)		
MW'08 (Aug08–Jan09)	0.002 (0.003)	0.001 (0.003)	0.002 (0.003)	–0.020 (0.019)	–0.017 (0.019)	–0.011 (0.018)	–0.010 (0.018)	–0.046*** (0.009)	–0.043*** (0.009)	–0.039*** (0.009)	–0.040*** (0.009)
Δln(sales), lagged 1 month		–0.030*** (0.008)	–0.025*** (0.005)		0.043 (0.050)	0.024 (0.031)	0.018 (0.031)		0.141*** (0.037)	0.121*** (0.025)	0.124*** (0.024)
County pop density (log)		0.019** (0.008)	0.068 (0.134)		–0.038 (0.058)	–1.791*** (0.668)	–1.773*** (0.659)		–0.032 (0.042)	–0.848** (0.363)	–0.857** (0.352)
County other private emp (log)		–0.007 (0.008)	0.014 (0.020)		–0.052 (0.063)	0.046 (0.075)	0.049 (0.071)		–0.037 (0.045)	0.061 (0.082)	0.059 (0.080)
Constant	2.011*** (0.011)	1.969*** (0.043)	1.245 (0.917)	3.225*** (0.071)	4.059*** (0.394)	14.654*** (4.553)	14.980*** (4.542)	7.284*** (0.052)	7.912*** (0.276)	12.148*** (2.912)	11.983*** (2.868)
Observations	2104	2100	2100	2104	2100	2100	2100	2104	2100	2100	2100
R-squared	0.784	0.821	0.952	0.243	0.354	0.922	0.923	0.156	0.291	0.912	0.912

TABLE 4 (cont.)

Description	ln(Wage)			ln(Employment)				ln(Total Hours)			
	(1) OLS No FE	(2) OLS No FE	(3) OLS Store FE	(4) OLS No FE	(5) OLS No FE	(6) OLS Store FE	(7) IV Store FE	(8) OLS No FE	(9) OLS No FE	(10) OLS Store FE	(11) IV Store FE
C. 2009:											
lnGAP'09 x MW'09	0.859*** (0.028)	0.871*** (0.029)	0.856*** (0.031)	0.671** (0.307)	0.570* (0.331)	0.652** (0.307)	0.762** (0.359)	0.417** (0.201)	0.352* (0.206)	0.405** (0.197)	0.473** (0.227)
lnGAP'09	–1.921*** (0.105)	–1.778*** (0.104)		6.951*** (1.231)	5.215*** (1.160)			3.461*** (0.853)	1.929** (0.865)		
MW'09 (Aug09–Dec09)	0.014*** (0.002)	0.014*** (0.002)	0.013*** (0.002)	–0.038* (0.022)	–0.036 (0.022)	–0.041* (0.022)	–0.051* (0.027)	–0.025* (0.015)	–0.022 (0.015)	–0.024 (0.016)	–0.031* (0.018)
Δln(sales), lagged 1 month		–0.030*** (0.006)	–0.025*** (0.005)		–0.010 (0.036)	–0.012 (0.032)	0.007 (0.034)		0.167*** (0.027)	0.178*** (0.025)	0.190*** (0.026)
County pop density (log)		0.003 (0.004)	0.021* (0.012)		0.010 (0.051)	0.159 (0.151)	0.376 (0.242)		–0.014 (0.039)	0.031 (0.066)	–0.018 (0.103)
County other private emp (log)		0.003 (0.004)	–0.033** (0.014)		–0.086 (0.053)	–0.148 (0.185)	–0.123 (0.178)		–0.050 (0.039)	–0.060 (0.080)	–0.045 (0.075)
Constant	2.070*** (0.008)	2.007*** (0.027)	2.119*** (0.078)	2.993*** (0.094)	3.935*** (0.334)	4.092*** (1.037)	0.978 (0.827)	7.142*** (0.066)	7.836*** (0.248)	7.713*** (0.444)	6.959*** (0.456)
Observations	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940	1940
R-squared	0.836	0.849	0.915	0.399	0.478	0.940	0.936	0.234	0.352	0.910	0.907

NOTES: Robust standard errors in parentheses, clustered at the store level. *** p<0.01, ** p<0.05, * p<0.1

6 months after (5 months after for 2009), with the MW_t treatment set to 1, and for the 6 months before each of the three increases ($MW_t = 0$). Store-specific MW compliance costs are accounted for by *GAP*. Estimates of φ, the coefficient on ln*GAP*•*MW* are shown in line 1 of each panel of the table and provide estimates of the wage, employment, and hours *GAP* elasticities. Panel A of Table 4 provides results based on the 2007 MW increase, panel B the 2008 increase, and panel C the 2009 increase. Each outcome equation is shown first with no controls, next with controls other than store FE, then with store FE, and then using instrumental variables (IV). Because ln*GAP* is such a strong predictor of store-specific wage changes, the structural IV results, which are our preferred results, are highly similar to OLS. As one might expect based on descriptive evidence seen in Figures 1 and 2, we find minimal evidence for negative MW employment and hours effects. In 2007 and 2008, elasticity estimates are small, mixed in sign, and never statistically significant, while in 2009, we obtain positive elasticity estimates.

Focusing first on the 2007 MW increase, wage elasticities with respect to *GAP* are close to one and precisely estimated, the estimate with store FE being 1.1. The employment and hours elasticities with respect to *GAP* are close to zero and not precisely estimated. The employment point estimate for the OLS model with store FE is 0.10, while the IV estimate using the predicted wage is 0.09, each with standard errors close to 0.5. The hours point estimate using ordinary least squares (OLS) and store FE is –0.19 and the IV estimate is −0.16; neither is close to statistical significance.

For the 2008 MW increase, wage elasticities are a precisely estimated 1.1, but the employment and hours estimates are again small and imprecisely estimated. The OLS and IV employment elasticity estimates with store FE are –0.29 and −0.26, while the corresponding total hours estimates are 0.15 and 0.13. Given the large standard errors and the flipping of signs between 2007 and 2008, we do not attach weight to finding different signs for employment and total hours, an outcome that could occur were there meaningful changes in hours per worker.

In 2009, the wage–*GAP* elasticity is 0.9. In contrast to 2007 and 2008, we find more substantive and significant employment/hours elasticity estimates. Both sets of estimates are positive, however, approximately 0.7 for employment and 0.5 for total hours. As mentioned previously (see footnote 15), these elasticities are with respect to the required compliance costs or, using IV, the wage, and thus are expected to be larger in magnitude than are teen elasticities common in the literature.

In Table 5, we examine "long-run" MW employment and hours effects based solely on information before and after the three MW increases. Our judgment, based on discussion with owners, is that the bulk of employment and hour adjustments within the stores (and associated adjustments, such as menu price changes) can occur relatively quickly and are clustered at the date of the MW change. However, some economists (e.g., Hamermesh 1995) argue

TABLE 5

THREE-YEAR WAGE, EMPLOYMENT, AND AGGREGATE HOUR CHANGES

	ln(Wage)			ln(Employment)				ln(Total Hours)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Description	OLS No FE	OLS No FE	OLS Store FE	OLS No FE	OLS No FE	OLS Store FE	IV Store FE	OLS No FE	OLS No FE	OLS Store FE	IV Store FE
Cumulative lnGAP x Oct–Dec '09	1.051*** (0.101)	1.052*** (0.090)	1.059*** (0.081)	0.580 (0.758)	0.584 (0.695)	0.415 (0.355)	0.392 (0.328)	0.295 (0.519)	0.297 (0.453)	0.288 (0.176)	0.277* (0.167)
Cumulative lnGAP	–1.399*** (0.084)	–1.344*** (0.082)		1.715*** (0.452)	1.290*** (0.435)			0.674** (0.320)	0.303 (0.313)		
Oct–Dec '09	0.045*** (0.017)	0.044*** (0.015)	0.048** (0.018)	–0.077 (0.123)	–0.083 (0.109)	–0.045 (0.067)	–0.063 (0.081)	–0.042 (0.087)	–0.048 (0.075)	–0.081** (0.035)	–0.096** (0.042)
County pop density (log)		0.008* (0.004)	0.084 (0.232)		0.019 (0.036)	–1.363 (1.062)	–1.396 (1.031)		0.019 (0.026)	-0.091 (0.588)	-0.115 (0.571)
County other private emp (log)		0.001 (0.005)	0.063 (0.046)		-0.068 (0.042)	–0.170 (0.224)	–0.195 (0.222)		–0.060** (0.027)	–0.384*** (0.088)	–0.398*** (0.086)
Constant	2.016*** (0.014)	1.955*** (0.039)	0.539 (1.260)	3.237*** (0.073)	3.879*** (0.295)	14.230* (7.792)	14.018* (7.705)	7.298*** (0.054)	7.851*** (0.186)	12.160*** (3.782)	11.972*** (3.716)
Observations	146	146	146	146	146	146	146	146	146	146	146
R-squared	0.946	0.952	0.983	0.269	0.315	0.920	0.917	0.117	0.206	0.941	0.940

NOTES: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Robust standard errors in parentheses. Included are two observations for each restaurant, one with Mar–May 2007 values and the other with Oct–Dec 2009 values. Estimated MW effects identical to a change model with one observation per store. In addition to stores not opened in early 2007, two stores were excluded due to large outlier residuals, high leverage, and extreme changes in employment (one positive and the other negative).

that MW employment effects in many studies are biased downward because they do not allow sufficient time for full adjustment.[16] With this idea in mind, and in keeping with the approach used in Table 4, we include two observations for each restaurant, average employment (and total hours) in January–March 2007 and October–December 2009, and then include store FE. The ln*GAP* measure represents the cumulative *GAP* (compliance costs) from the three MW increases, while the dummy *MW* is set to 1 for end-period observations. The coefficient on ln*GAP*•*MW* provides the elasticity estimates; ln*GAP* is absorbed by store FE. The wage and gap elasticity estimates with store FE shown in Table 5 are identical to those obtained using a simple difference model with one observation per restaurant.

As seen in Table 5, the 3-year employment and hours elasticity estimates are positive, but not precisely estimated. Our preferred employment elasticity, from the structural IV model, is 0.39 (the OLS estimate is 0.42). The IV hours elasticity estimate is 0.28, marginally significant at the .10 level (the OLS estimate is 0.29). Given the descriptive evidence seen in Figure 2, coupled with results shown in Table 4 for the annual MW increases, these long-run results are not surprising.

The pattern seen in Figure 2 between the 3-year change in employment and *GAP* (or wages) suggests that the relationship may be nonlinear, consistent with a monopsony explanation. We estimate an added nonlinear specification in which we add a squared ln*GAP*•*MW* interaction term. When we do so, we consistently get a positive coefficient on ln*GAP*•*MW* and a negative coefficient on the squared term, for both the employment and hours equations. For example, coefficients (all significant) for these two terms from the full specification with store fixed effects are 2.15 and –7.68 in the employment equation, and 1.88 and –6.87 in the hours equation. Apart from the squared term, the specifications are identical to Table 5, columns (6) and (10).

Despite well-measured data and a tight correspondence between MW compliance costs and observed wage increases, we find no compelling evidence for negative employment or hours effects for these quick-service restaurants in response to the three MW increases during 2007–2009. Although the weight of the evidence suggests somewhat positive rather than negative MW effects, both the sample size and statistical precision of the estimates are too low to allow us to draw such a strong conclusion, absent strong priors favoring

[16] In the concluding section, we briefly discuss long-run adjustments involving restaurant closures and entry. Walter Wessels has suggested to us that it is difficult a priori to determine the timing of employment responses to a multi-part MW increase. Firms can evaluate the present value of future MW increases while changes in staffing can either lead or lag wage changes, making longer-run estimates preferable. It should be noted, however, that interviews with owners indicate they deliberately implement both employment and price adjustments at the time of a MW change so employees and customers understand the rationale, thus preserving goodwill and preventing perceptions of opportunistic profiteering.

monopsony and/or institutional models of the labor market. Whatever the average response to the MW, it is clear from our results that there is considerable store-specific heterogeneity (in shocks and/or behavior), making it difficult to observe systematic responses to minimum wage mandates.[17]

Alternative Channels of Adjustment

The finding of generally small and insignificant impacts of the minimum wage on employment in this and other studies challenges the predictions of standard economic models. But if adjustments in employment and hours are minimal, what changes do occur? We address this question by exploring a wide range of channels of adjustment (CoA). Evidence is obtained from alternative sources. In addition to use of the payroll data and other information from the franchise owners, we rely on information from manager surveys and personal interviews. Some of these CoA have received attention in earlier MW studies; others are largely undocumented.

Wages, nonlabor costs, and prices: Insight from back-of-the-envelope calculations. As reported previously, calculations from the payroll data show that the direct MW cost of compliance (i.e., raising wages just to the minimum, absent other pay increases) was 2.6 percent, 4.6 percent, and 6.8 percent of total payroll (hours constant) for the 3 years. We add to this the estimated increase in employer payroll taxes.[18] If there were constant returns-to-scale in production and full pass through, the increase in price due to higher wages should be proportional to the share of labor in total factor cost. We do not have a direct measure of labor's share, but use a guesstimate of 24 percent based on data from a franchise owner showing that nonmanagerial payroll was 18 percent to 20 percent of total sales.[19] Taking each year's MW compliance costs (including payroll taxes)

[17] Neumark, Salas, and Wascher (2013) cite our earlier long-run estimates (absent store FE) and characterize their imprecision as the product of "uninformative data." We lean toward the alternative interpretation that the data are informative, illustrating that wage–employment relationships involve multiple channels of adjustment and that there exists greater flexibility and heterogeneity among establishments than suggested by textbook theory.

[18] Social Security and Medicare plus federal and state unemployment insurance (UI) costs are about 13 percent of payroll. Owners were surprisingly vocal about the burden of higher payroll taxes necessitated by the MW.

[19] Labor's share of total costs exceeds its share of sales. Aaronson, French, and MacDonald (2008: 695) provide evidence that limited-service restaurants have a 30–35 percent share of labor to total costs. Our estimate of the price pass-through needed to offset cost increases is insensitive to the assumed labor's share once we account for nonlabor cost.

and labor's share, the cumulative 3-year increase in total costs directly due to MW compliance is an estimated 3.9 percent.

If MW compliance were the only increase in costs over 3 years, it might be readily handled through price and nonprice channels of adjustments. Restaurants faced other sources of labor costs, however, including pay increases for workers above the MW, and substantial changes in nonlabor costs. We calculate an extended measure of labor costs for our restaurants taking into account MW compliance costs plus additional increases in pay. We do this by examining the total increase in the wage bill (holding hours constant) over the 3 years, including estimated payroll tax increases. A cumulative 25.0 percent increase in total labor costs boosted total costs (labor plus nonlabor) by an estimated 5.7 percent.

Although important, increases in wages were not the principal source of cost increases during this period. For the approximately 76 percent share of costs due to nonlabor inputs and managers' compensation, we assume these rose at the same rate as the BLS Producer Price Index for "finished consumer foods" during 2007–2009. On a percentage basis, these costs increased roughly half as fast as did labor costs, but they had a larger impact on total costs given their large share. Taking the weighted average of the labor and nonlabor costs, we calculate that average per-unit costs rose 15.4 percent during the 3-year period.[20]

To what extent were restaurant owners able to offset these higher labor and nonlabor costs through increases in menu prices? We cannot measure overall price changes, but did obtain price hikes from the owners for the single most popular menu item (homogeneous among the stores), a combo meal made up of a sandwich, small fries, and a drink. The price increase for this item over the 3 years, averaged across all restaurants (each given an equal weight) was 10.9 percent. Although less than the estimated 15.4 percent increase necessary for full pass through, a ballpark estimate is that about two thirds of total cost increases were offset by higher prices. Had the MW been the sole source of increasing costs, and had the economic downturn in 2009 not so seriously affected sales, it seems likely that the restaurants could have passed most to all of the higher labor cost through to consumers.[21] Of course, such an extensive

[20] Franchised stores buy all food and related supplies from corporate-designated wholesalers. An owner provided us with data on the annual percentage price change for a typical food supply "basket." This yields a cost estimate highly similar to that using the Produce Price Index (PPI). Whereas MW compliance costs were low in 2007 but climbed in 2008 and 2009, nonlabor costs exhibited the opposite pattern (many input prices fell in the 2008–2009 recession), making it easier (all else the same) for stores to handle the 2009 MW hike.

[21] Addison, Blackburn, and Cotti (2013) provide evidence that teenagers in the food service and drinking sector were most affected by the 2007–2009 MW increases in those states with high unemployment rates.

pass-through of costs is most likely when labor and nonlabor cost changes are similar across competitors.[22]

For this sample of QSRs, higher prices rather than cuts in employment and hours seems to be the more important CoA from higher wages due to the MW. That said, we also find evidence of second-order but nontrivial adjustments through a variety of other channels.

Compression and the internal wage structure. Cost increases following minimum wage hikes can be mitigated by awarding smaller-than-normal pay raises to workers with wages above the minimum and by permitting the internal wage structure to become more compressed. Although managers stated that they desired to preserve wage differences between the less experienced and more senior workers, it became increasingly difficult to do so over the 3 years. As seen in Table 3, required compliance costs for the three MW increases were 2.6 percent, 4.6 percent, and 6.8 percent of payroll in 2007, 2008, and 2009. Overall payroll increases (holding hours constant) following the MW increases were 6.0 percent, 6.6 percent, and 7.9 percent, implying that pay increases beyond those legally required declined from roughly 3.0 to 2.0 to 1.0 percent of payroll over the 3 years.

Although we cannot determine the extent to which wages across the distribution would have increased absent the MW mandates, evidence that the internal pay distribution became more compressed is strong, as seen in prior literature (e.g., Dube, Naidu, and Reich 2007). Average within-store pay dispersion (measured by the coefficient of variance [CV] of dollar wages) was 0.156 in March–May 2007, but steadily decreased following the three July MW increases, to 0.139, 0.102, and 0.070 in October–December 2007, 2008, and 2009, respectively. Increased compression is also observed among workers for whom the MW was not binding, with the CV for nonaffected workers being 0.141, 0.125, and 0.104 following the three increases.

Owners confirm that the size and extent of performance-based raises were reduced as they sought to contain increasing costs during a period of weak demand. When we compare wage increases among workers for whom the MW was and was not binding, we find that workers above the MW received far smaller relative (and absolute) pay increases. Average pay increases among affected

[22] Our evidence on prices is consistent with Aaronson, French, and MacDonald (2008), who provide comprehensive nationwide evidence on price changes among restaurants in response to the two-step MW increase in 1996–1997 from $4.25 to $4.75 to $5.15. Prices increase more for limited-service than for other restaurants and vary geographically as expected based on state minimum wages and market wage levels. Aaronson, French, and MacDonald (2008) interpret their evidence as largely inconsistent with monopsony while supportive of the competitive model.

workers were 22, 39, and 62 cents in 2007, 2008, and 2009, respectively, whereas workers whose wages were at or above the legally required level received average increases of 16, 22, and 21 cents during those same years.

Insight regarding pay compression also is gained by measuring the share of workers with pay at or near the MW. Prior to the July 2007 MW increase from $5.15 to $5.85, 41 percent of workers had wages at or within 50 cents of the MW (i.e., a wage between $5.15 and $5.65 in May 2007). By October 2009, following the third MW increase, those with wages at or near the minimum (a wage between $7.25 and $7.75) had increased to 89 percent, with 35 percent paid exactly $7.25. In short, following the third MW increase, most workers were paid close to the MW.

We also examine pay compression by asking how wages among nonaffected workers (those with wages above the minimum) varied with respect to store compliance costs. Recall that our first-stage wage regressions (see Table 4, column 3) produced precisely estimated store-level wage-GAP elasticities close to 1.0 for each of the years. When we estimate this same model, but use as our dependent variable the store-level wage among those at or above each MW (rather than for all workers), we obtain wage-GAP elasticities that are positive (i.e., consistent with MW spillovers) but close to zero and insignificant—0.026 (0.208), 0.063 (0.099), and 0.125 (0.076) for the 3 years (standard errors in parentheses).

Evidence from these restaurants clearly shows that cost increases following MW increases were constrained by limiting pay increases for higher wage workers and thus allowing store pay distributions to become increasingly compressed. Pay increases above the minimum were largely unrelated to MW compliance costs, however, suggesting that nonlabor cost increases and weak demand during the recession played an important role. Evidence from Card and Krueger (1995: 164) and our interviews suggest that in future years extreme pay compression is likely to be mitigated as managers provide smaller raises for workers at or close to the minimum and more substantial raises to more senior workers.

Separations and turnover. Labor turnover is costly to firms. If higher pay reduces turnover, firms can partially offset MW costs by savings in hiring and training (Arrowsmith et al. 2003). Higher wages, per the dynamic monopsony model (Manning 2003), may also reduce vacancies and lead to a higher level of employment and hours. Empirical evidence on the MW and job attachment link generally finds lower turnover from the MW (e.g., Dube, Naidu, and Reich 2007; Reich, Hall, and Jacobs 2005; as well as Fairris and Reich [2005] for living wage ordinances). Using data on employment flows, Gittings and Schmutte (2014) showed that MW employment effects depend in part on turnover, the MW decreasing employment in low turnover sectors and increasing employment in high turnover sectors.[23] They argue that turnover is a cost channel for firms and that reduced turnover from MW lowers the marginal cost of employment.

Below we first examine how store-level turnover differs with respect to MW compliance costs. We then turn to an analysis of individual worker employment duration. For our store-level analysis, we provide formal as well as descriptive analysis on employee separation rates—the share of the total workforce observed in pay period t but not in period $t + 1$. By this measure, if all workers (other than new hires) are present in two consecutive pay periods, the separation rate is zero. Such separation rates are high—approximately 8 percent each pay period—but there is a downward trend, particularly pronounced after the second MW increase in July 2008. Over the 3-year period, separation rates fell from roughly 10 percent to 5 percent. Rates are not stable across months, with spikes in summer and in December–January. We provide a similar measure for new hires per pay period, and a measure of turnover that is simply the sum of the separation and hires divided by two times employment from the previous payroll period.

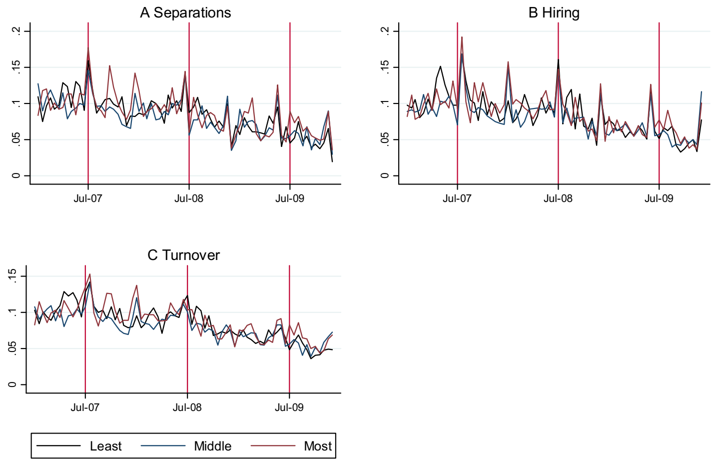
In Figure 3, these rates are shown separately for restaurants that are least, moderately, and most affected by the MW increases (based on the cumulative GAP). As expected, the separations, hiring, and turnover rates closely track each other over time. Moreover, it is difficult to discern any obvious differences in levels or patterns between these three groups of restaurants.

More formally, in work not shown, we estimated separation rate regressions that mimic the approach seen in Table 3 for employment and hours. The dependent variable is the share of the workforce observed in pay period t but not observed in period $t + 1$. In addition to store fixed effects (which absorb the lnGAP measure), we include a dummy for the post-MW period and the interaction of the post-MW period with compliance costs (ln*GAP•MW*), with the coefficient on this interaction term identifying the effect of the MW mandate on separations. A second specification adds two variables—the change in store sales (lagged) and change in county private employment. We obtain the expected negative coefficients of MW compliance costs (which closely predict wage increases) for the 2008 and 2009 increases (the former but not the latter being significant), but obtain positive and insignificant coefficients for 2007. Although the 2008 and 2009 results support standard theory, we are reluctant to give these results substantial weight given the far less plausible positive coefficients in 2007.

[23] Data from the BLS's Job Openings and Labor Turnover Survey (JOLTS) show that quit rates in "Accommodation and Food Services" are the highest among any of the very broad industries for which they publish results.

FIGURE 3

Average Store Separation, Hiring, and Turnover Rates, 2007–2009, by Store Groups Compliance Cost of MW



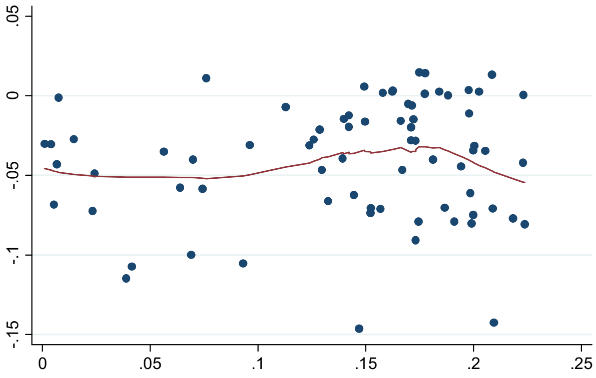
Notes: Separation and hire rates are defined in the text. The turnover rate is the sum of separations and hires divided by two times employment. "Least" affected stores include those where *GAP* is in the bottom 25 percentiles, "Middle" the 25th–75th percentiles, and "Most" percentiles 75 and over. Vertical lines mark MW increase dates.

Using the exact same setup, we also estimated hiring and turnover rate equations. None of the ln*GAP•MW* coefficients was statistically significant. The turnover coefficients had the same signs as in the separation equations for all 3 years, while the hiring coefficient signs matched the separation equations in two of the three years (2008 was the exception). Given that these estimates were largely uninformative, we do not show the results.

We also examine "long-run" (3-year) changes in store separation rates with respect to the cumulative store compliance costs. This relationship is shown in Figure 4. Although we would expect separations to decline across stores with respect to the GAP, what we see is a relatively flat relationship throughout much of the distribution, with declines evident only among those restaurants most impacted by the MW. Taking the pattern seen in the local regression curves at face value, the suggestion is that only among the lowest-paid stores—those most affected by the MW—does one see reductions in separations (and turnover more generally). Drawing such a conclusion requires that we largely ignore the large variability in separations across stores. Equivalent figures for hiring plus separations are highly similar to that seen for separations. Broadly viewed, these results appear to support Lester (1964: 204–7, 280–81),

FIGURE 4

THREE-YEAR CHANGES IN STORE-LEVEL SEPARATION RATES WITH RESPECT TO CUMULATIVE MW COMPLIANCE COSTS



NOTES: The vertical axis measures the change in store separation rates between March–May 2007 and October–December 2009. The horizontal axis measures MW compliance costs using the cumulative 3-year ln*GAP*.

who argues against monopsony interpretations and in favor of an institutional-type area of indeterminacy in wage determination and an associated "no response zone."

Turning to our individual-level analysis, although we do not observe a strong relationship between store-level turnover and compliance costs, we are able to analyze our data at the individual level despite the fact that the structure of the data limits our ability to provide a fully satisfactory duration analysis.[24] The completed length of an employment spell is unobserved; we do not observe entry for those present in the initial January 2007 pay period, and do not observe exit for those continuing beyond December 2009. We do not know the length of very short spells (less than two weeks) since we only observe such workers' hours and pay for a biweekly payroll period. Left censoring (inability to observe the point of entry for each worker) can be addressed by considering only "new hires"—workers who enter the sample *after* mid-January 2007. Descriptive evidence on employment duration confirms that turnover is high, as expected. For this subsample of workers, we find that the median "survival" in the sample is 14 weeks (seven pay periods).

[24] The "failure event" is falling off the payroll. Workers present for only one biweekly payroll are omitted.

Roughly 70 percent of all new hires beginning mid-January 2007 are not in the sample at the end of December 2009.

Keeping the censoring issues in mind, we examine how an individual worker's employment duration is affected by the compliance costs due to the MW increase. We construct individual-worker "gaps" by calculating the percent increase in individual wages needed to comply with each year's July 1 MW increase, based on observed wages 2 months earlier. Unaffected workers—those whose wages were already at or above the new required minimum—have a zero gap. Instead of collapsing the dataset to create single-spell observation for each worker, the dataset contains multiple spells per worker (i.e., the number of pay periods on the payroll) to take advantage of time-varying covariates.

The set-up for the duration analysis resembles our earlier analysis for employment and hours (Table 3). Separate models are estimated for each of the 3 years. Here, the "outcome" for each worker equals zero for all pay periods except the final one, which equals one if a worker is not on the next payroll. Survival time is censored for those who do not leave the payroll. Independent variables include the worker-level "gap" as calculated in each year, a binary variable for the 6 months after each July MW increase, and their interaction. The estimated coefficient on the interaction term tells us how the risk of a separation (due mostly to quits, but in some cases a layoff or long leave period) varies with the exogenous shock of a MW increase, conditional on being on a payroll a certain amount of time. We also control for store fixed effects and worker tenure (the baseline hazard rate).

Estimates are derived applying a discrete logistic model.[25] As expected, survival time decreases with time spent on the payroll (a higher baseline hazard rate) for each of the three periods.[26] However, we do not find a strong relationship between MW increases and separations in our data, consistent with the weak store-level evidence provided previously. We find little evidence of individual employment duration being affected by the first two MW increases (signs are mixed and insignificant). Stronger evidence is found for 2009, during which the job market substantially contracted. A worker requiring an 8 percent (0.079 log point) wage increase to reach $7.25 in July has a 4.8 percent lower probability of separation in August (a statistically significant –0.61

[25] If we assume transition events (in and out of employment) can occur at any point in time, we would have a continuous time duration process. Because of the structure of our payroll data, we observe "events" at discrete intervals (biweekly), making the discrete time duration process arguably more appropriate for the analysis.

[26] Baseline hazard rate is assumed to have logistic functional form, but results are unaffected when we assume a nonparametric baseline hazard rate and include interval-specific dummy variables.

coefficient times 0.079 = –0.048 or, equivalently, exp(–0.048) = 0.95 lower hazard rate).

In contrast to the weak relationship found here between store compliance costs and separations, clear-cut effects have been found elsewhere using comprehensive national data. For example, a recent paper by Dube, Lester, and Reich (forthcoming) examined county-level data on both employment flows and stocks. They examined contiguous county pairs across state borders using data for the restaurant industry and, separately, for teenagers. In both sets of analyses, they found that a higher MW leads to substantially lower separations and hires, but has little effect on employment levels in their preferred specification. A Canadian study by Brochu and Green (2013) provides similar evidence using household data, concluding that higher minimum wages lead to lower hiring rates and separation rates, the latter heavily influenced by lower layoff rates in the first 6 months of employment. They concluded that jobs in a higher wage regime are more stable but harder to get.[27]

The relationship between the MW and separation/turnover rates in our sample is weak. Given the downward trend in turnover evident among both high- and low-wage restaurants over this period, coupled with the strong evidence of a turnover effect seen in prior studies, it seems likely that some portion of the added costs from MW increases were offset by reduced hiring and separation costs. Two of the franchise owners estimated turnover cost at $300–$400 per employee—factoring in approximately 40 hours of non-revenue-producing training over 6 months. It is clear from the manager survey that store managers regard turnover as important. When asked to comment in an open-ended question about positive aspects of a higher MW, managers emphasized lower turnover, along with higher morale, greater worker effort, and more and better job applicants.

Operational and human resource efficiencies. We next explore operational and human resource (HR) efficiencies with data collected from manager surveys and interviews. Although these data do not readily lend themselves to formal analysis, insights are gained by peering into the "black box" of internal operations. That said, managers' statements regarding what is important or what they are likely to do need not fully align with actual responses.

Managers were asked to estimate the share of the (then upcoming) 2009 MW labor cost increase they could offset by implementing various operational efficiencies, after excluding price and employment and hours adjustments. Their responses averaged 23 percent, with no significant difference in responses among the least and most affected stores (p-value = 0.468 based on

[27] We thank a referee who pointed us to these two studies.

the 2009 GAP). Although conjectural and perhaps subject to upward bias (from overoptimism, etc.), this estimate has some reliability because the managers (1) had already worked out as part of their business plans the likely increase in their wage bill (the ratio's denominator) and (2) had two rounds of experience with operational belt-tightening from the 2007 and 2008 MW increases from which to project an estimate for 2009 (the numerator).

We next sought to identify the specific sources of cost savings. Based on input from manager interviews and several pre-tests of the survey instrument, we developed a list of twenty-three potential CoA in the following areas: human resource practices, operational efficiency and productivity, nonlabor

TABLE 6

Importance of Cost-Saving Strategies in Response to MW Increases, Manager Surveys

	Very Important	Somewhat Important	Not Very Important
A. Human resource practices and cost savings:			
Increase performance standards	39	14	4
Change work schedules	26	13	3
Hire older/more experienced workers	15	13	3
Cut weekly hours of some employees	15	14	6
Schedule more part-time employees and fewer full-time	11	4	2
Postpone or limit pay raises to your more experienced workers	11	11	3
Schedule more full-time employees and fewer part-time	5	4	2
Reduce the number of people on your payroll	4	4	3
Hire teenage workers	3	4	2
Reduce training	1	2	2
B. Operational efficiency and productivity cost savings:			
Discourage overtime work	44	4	2
Cross-train workers for multitasking	38	12	2
Get more work from each person	37	16	3
Increase morale and team spirit	35	17	5
Expand job duties of your workers	34	17	3
Tighten-up on absenteeism and discipline	29	15	6
Rearrange production operation to be more efficient	22	13	2
Less time spent on clean up	3	3	2
C. Non-labor expenses, customer services and cost savings:			
Reduce food waste in preparation/storage	44	12	2
Reduce or save water and electricity usage	38	14	6
New ways to improve customer service	36	13	3
More outreach to church, school, and community groups	20	7	2
Reduce amount of condiments and "extras" given to customers	19	10	6

Notes: This table provides distribution of answers to the following question in the Manager Survey: "Other research studies of the minimum wage have identified the following list of items as BUSINESS ADJUSTMENTS you might possibly make in order to OFFSET the payroll cost increase associated with the higher minimum wage. Which of the following are you planning to do in the next 1–3 months OR have done already in the last month (please check YES or NO)? If your answer is YES, please rate the impact of your action for cost-saving on the scale 1 to 5 (1 = least important; 5 = very important). Please circle one number from 1 to 5." The responses were collapsed as follows: "Not very important" if a respondent answered 1 or 2; "Somewhat" if 3; and "Very important" if 4 or 5. Shown are responses for those who answered YES to the "planning to do" or "have done" question.

costs, and customer service. Despite careful wording, some items inevitably have a degree of overlap.

Managers were asked whether they currently use or plan to use in the *next 3 months* each of these twenty-three cost-saving measures in order to *offset MW cost*. Those who answered yes then rated the contribution to cost savings of each item on a scale from 1 to 5, with 5 the most cost-effective. Of the total eighty-one stores, sixty-six managers responded to the survey, all of whom answered the yes/no question. Among those who answered yes, all but a small number provided ratings on importance. In the text below, we state the proportion of the managers who answered yes. Table 6 provides the distribution of answers among those who rated its importance. We collapse the five categories into three (ratings 1–2, 3, and 4–5), labeling them "not very important," "somewhat important," and "very important."

We start with efficiencies in HR practices. We identified several such channels. Most of the sixty-six managers (90 percent) planned to increase performance standards and, among those who provide ratings on this channel's cost savings, about two thirds rank it as "very important" (Table 6, panel A). Higher performance standards include things such as requiring a better attendance and on-time record, faster and more proficient performance of job duties, and taking on additional tasks. Managers said in interviews that part of effective HR practice is to directly communicate to employees the quid pro quo of higher performance for higher wages. Another cost-saving measure is adjusting employee work schedules to more tightly match beginning and ending times with customer demand, thus gaining a fuller utilization of each employee hour. Regarding labor–labor substitution, most managers did not consider changing the part-time/full-time mix. They did express interest in hiring more experienced and older workers but fewer teenagers, and there is a positive correlation between the size of the cost shock from the 2009 MW increase and managers' reports of using this cost-saving strategy.[28] Only a few managers planned to reduce training, the reason given in interviews being that greater operational efficiency requires more training (the two are comple-

[28] We measured all the responses in Table 6 separately for restaurants least, middle, and most affected by MW compliance costs. In most cases, we could not see a clear-cut pattern between the responses and the *GAP* group. We also tested more formally for labor–labor substitution using information in the employee surveys, which identify the store but not employee. Using data on age and high school completion, coupled with their reported tenure, we matched these to stores' MW compliance costs. Simple difference-in-difference models were used, with outcome variables being age and high school completion. The independent variables included a binary variable equal to 1 if a worker was hired *after* each of the three MW increases, the store's ln*GAP* for that year, and the interaction between the two. Coefficients on the interaction terms in the two sets of regressions were positive in all years for education and for two of the three years for age, but none was significant.

ments). Consistent with evidence from the payroll data, a number of managers (40 percent) state they would delay or limit pay raises/bonuses for more experienced employees.

The manager surveys support the evidence from payroll records that reducing employment is not a principal CoA. Only 23 percent of managers planned to decrease their workforce to offset the higher cost and only 12 percent rated this strategy as "somewhat" or "very important" for cost saving. In fact, there is a significant negative correlation between the size of the 2009 MW impact on costs and managers' plans to reduce their workforce. In contrast to our findings of largely unchanged hours worked from the payroll data, a significantly greater proportion (60 percent) planned to reduce work hours. This discrepancy may represent a gap between what managers say and do; an alternative explanation is that their answers to "plan to reduce work hours" and "plan to change work schedules" substantially overlap, the latter is the dominant adjustment but has a small impact on the number of hours worked.

We probed in interviews for reasons behind the small-to-zero employment effect and several factors were cited: Speedy customer service is a "must-do" and reduced staffing threatens it, the production process features indivisibilities and fixed capital–labor ratios (e.g., one person per cash register and drive-through window) that preclude marginal labor adjustments, and team spirit and a cooperative employee attitude are the most important factors for successful operations and layoffs or cuts in hours undermine these.[29] With regard to labor input, worker effort appears to be a short-run continuous adjustment factor in the production function, with employment partially discontinuous.

We next transition to various forms of operating efficiencies. The owners keep detailed records on daily and weekly indicators of costs, sales, and payroll, and establish targets that managers are expected to meet. A rise in the MW (or other costs) moves stores past these targets and creates pressure on managers to squeeze out costs through tighter operations. With regard to the MW, the most common response was to gain greater productivity from the workforce through, say, cross-training, multi-tasking, and tighter work schedules. As indicated in Table 6, panel B, boosting morale was also a major CoA (cited by 92 percent). Interestingly, increasing performance standards and enhancing workers' morale and team spirit are the two strategies that are strongly and positively correlated with the degree to which a given store is affected by the 2009 MW increase (analysis not shown). The managers work to create interdependent utility functions among their crew to create

[29] One owner predicted to us in the first interview (before data analysis) that we would find a zero employment effect, citing reasons such as those just given.

productive synergies, team spirit, and self-enforcement of high work norms through peer monitoring. Reliance on team production makes it crucial to treat employees not as disposable "hired hands" but as valued crew members who managers strive to respect, support, and treat fairly. Managers stated that they try to avoid overt cuts in hours and headcount, an exception being use of the MW increase as an opportunity to weed out particularly low-performing employees. Another CoA in the operations and productivity area is new capital equipment. We did not include this option on the manager survey because owners make capital investment decision. Owners indicated a steady if slow process of capital-labor substitution, constrained in the short run by cash-flow constraints and the relatively simple nature of the technology of fast-food production.

We note for later discussion that there is both a neoclassical and institutional/behavioral perspective on managerial "tightening up." In the competitive and monopsony models, firms are assumed to continuously minimize cost so no slack (underutilized resources) exists. Owners and managers told us that since fast food is highly competitive they vigilantly monitor costs and after two previous MW increases felt like they had pretty well squeezed out the "fat." Yet when questioned on planned adjustments to the third MW hike they cited a number of actions to further improve efficiency.

Five observations from the interviews are apropos. The first is that the managers are overloaded with daily operation issues and work long weekly hours (often 50–55) and, hence, cannot devote the time to actively address important but longer-run or secondary operational issues. A MW hike thus acts as a catalyst or shock that forces managers to step out of the daily routine and think about where extra savings can occur. Second, a principle–agent problem is present to the extent owners cannot fully monitor salaried managers who may therefore satisfice rather than fully cost minimize. Third, managers' ability/quality varies across stores, which results in different cost curves, efficiency levels, and tightening-up responses. Fourth, as a practical matter it is difficult to distinguish such adjustments as a neoclassical-like movement along an isoquant (substituting a given stock of managerial time from one activity to another) versus an institutional/behavioral movement to a higher isoquant (generating more output from given managerial inputs by energizing effort/attention to unexploited areas of cost saving). If isoquants shift out in response to MW or other shocks, it is plausible that over time they gradually shift in as shocks (old and new) become less salient. Fifth, owners said the "quality" of the manager is the single most important determinant of unit success (given location, etc.), but managerial quality is scarce and heterogeneous. What is typically referred to as "slack" in part reflects differences in managerial quality, along with other unmeasured factors that account for productivity differences across establishments (Syverson 2011). All in all, we feel unable to disentangle these competing hypotheses.

Nonlabor costs and customer service. Two other distinct CoA are savings on nonlabor inputs and customer service improvements. Interviews indicated that utility costs, insurance, food costs, food wastage, size of drinks, and condiment supply all received attention. The survey responses (Table 6, panel C) indicate an overwhelming majority of managers plan to economize on electricity and water usage and reduce food waste (e.g., by more careful scheduling of deliveries and tighter inventory control). One area of focus was cost control at the condiment bar—a seemingly trivial example but indicative of how pressure to contain costs leads to numerous marginal adjustments.

Another CoA is aimed at increasing sales through improved service. Part of raising performance standards is more "smiling faces" at the counter and drive-in window; another element of customer service is having employees more often check the cleanliness of bathrooms, dining tables, and parking lots. Marketing strategies, such as meal discounts, new menu items, and raffles are implemented to maintain or increase volume. A number of the managers interviewed said they also planned to offset higher cost by building volume with more special events, for example birthday parties and events for local churches, youth groups, sports teams, and retirement homes.

Profit. Profit is the residual that remains following movement in all other revenue and cost channels earlier discussed. Prior evidence on the MW profit effects is scarce; several studies, however, find zero or small negative effects (Card and Krueger 1995; Draca, Machin, and Van Reenan 2011). Due to confidentiality concerns, none of the owners would share data on annual profit. We did obtain, however, data on the average profit *changes* among approximately three fourths of the stores spanning the three MW increases, based on franchise-wide and not store-specific results. The data cover a fiscal year (e.g., starting October 1) so disentangling the MW effect is difficult. For this and other reasons our evidence is best considered suggestive.

Annual profit changes at these stores averaged 6.9 percent (FY 2006), 20.0 percent (FY 2007), 5.9 percent (FY 2008), 0.5 percent (FY 2009), and –20.0 percent (FY 2010). FY 2007 includes 3 months of the 2007 MW increase, while FY 2010 begins 3 months following the 2009 increase and reflects the wage base following all three increases, plus other cost changes. We earlier showed that the bite of the MW on store labor costs increased over the three rounds; these data show that profit growth correspondingly slowed in each round from FY 2007 through FY 2010. For each MW hike, the businesses were able to keep revenue ahead of costs (profits grew each year through FY 2009) through various CoA, but their ability to do so lessened over time.

Owners indicated in interviews that if the *only* cost increases over 2007–2009 were from the MW they could have mostly to completely offset them through other CoA. Profit growth declined over this period, however, as increases in labor cost were compounded by increases in other cost areas (e.g., food and operating costs), sales growth contracted during the 2008–2010 recession, and ability to raise prices diminished. FY 2010 was, in the words of one owner, a "perfect storm" for profit (–20 percent) since the base of labor cost rose the most in 2009, commodity prices spurted upward in 2010, and local economy activity and restaurant sales remained anemic. It is impossible for us to decompose the weight due to each factor, but the owners agreed in interviews that the decline in sales volume much dominated the MW as a contributor to lower profit growth (roughly estimated as 10-to-1).[30]

The conclusion we draw from our sample and time period is that (1) the MW by itself had a small effect on profit, employment, and growth but (2) these companies are struggling with multiple sources of cost increase in a climate of significantly deteriorated sales and, hence (3) these factors together have posed growing profit and (in some cases) survival problems, with potentially negative consequences for additional business formation and employment growth in the medium-to-long run.[31]

[30] Lester (1946) found a similar result regarding employer sensitivity to sales versus wage changes. His explanation is that the marginal cost curve is horizontal-to-modestly declining until close to full capacity, causing the average total cost curve to slope downward (given declining average fixed costs). Hence, reducing output (the neoclassical scale effect) in response to a MW increase further shrinks the margin of profit and managers are led to instead search out ways to increase sales as an offsetting way to control unit cost. Thus, in Lester's institutional model the constraint on employment is the volume of sales and not the neoclassical wage equals marginal revenue product condition. Owners we interviewed guesstimated steady-to-declining unit variable costs and said they did not make hiring decisions by a wage versus marginal revenue product type calculation.

[31] The owner with the largest number of stores indicated in 2010 that for the first time in the company's history (40 + years) no new units were planned or under construction. A second owner was forced to close two stores in 2011 and in 2013 sold all the stores after 3 years of cumulative loss and lack of cash flow to pay for needed store renovations and corporate-mandated marketing programs (both non-labor cost items). Worth noting as a MW contingency effect, the former owner practiced a "commitment" human resource strategy; which, ceteris paribus, contributes to a smaller MW employment effect since maintaining employee security, trust, and morale are regarded as critical to eliciting high performance (per the previous discussion in the text). The new owner—outside the purview of our study—reportedly switched to a "control" HR strategy by much tighter monitoring and discipline (cameras throughout the work area, quick termination for small lapses) and termination of higher wage senior employees and managers—presumably also making employment/hours more wage sensitive.

Summary and Conclusion

As empirical study of the minimum wage has a near century-long history, one must question whether an additional study breaks new ground. We believe this study does so along several dimensions.

First, our dataset is innovative. It captures three rounds of MW increases, contains accurately measured establishment-level pay and employment data, supplements it with data from manager surveys on a wide range of rarely captured aspects of internal firm operations, and is rounded out with information from field-level interviews. Although shortcomings are present—for example, a small sample size of restaurants resulting in imprecise estimates, the qualitative and subjective nature of the manager data, and anecdotal reports from personal interviews—on balance we believe the analysis adds valuable insights to some old questions while addressing several new or underexplored ones.

Second, we recast analysis of the minimum wage into a broad "channels of adjustment" framework, moving beyond the conventional emphasis on employment effects. The effect of MW on the internal operation of firms has been left as a mostly unexamined black box (but see Arrowsmith et al. 2003). We believe the CoA framework usefully broadens attention to the many margins potentially affected by MW, the manner in which employers and markets adjust these margins, and the resulting incidence of costs.

Third, the employment effect of MW continues to be a major point of controversy in the literature and for public policy. Our analysis identifies the employment and hours effects based on differences across restaurants and over time in the compliance costs resulting from the three MW mandates. We find, in line with other industry-specific studies, that the measured employment and hours impacts are highly variable across establishments and in many or most cases not statistically distinguishable from zero. Although we had hoped to have a larger sample of businesses, we regard the observed variability in this sample as an informative indicator of the extent and importance of heterogeneity in economic relations.

Fourth, our study finds evidence that the cost of MW is passed along and absorbed through a wide range of CoA. One of the more important channels is the increase in product prices, an adjustment made easier because the MW affects local competitors in a similar manner. Although standard theory predicts higher prices cause lower output and employment, it is difficult to discern such responses in our data. Other CoA include operational "tightening up," higher employee performance standards and work effort, new marketing programs to expand sales, and compression of the internal wage structure. Interview evidence suggests that in good economic times restaurants can mostly and perhaps completely maintain profit margins by utilizing these other CoA, but in economic hard times they are insufficient and a higher MW takes a bite out of profit, particularly at marginal/low-volume stores.

Fifth, this study offers a new explanation for the small and insignificant MW employment effects frequently found in the literature. Common explanations are that "failed" (non-negative) estimates are a statistical artifact from data mismeasurement or flawed estimation procedures; also, appeal is made to structural or dynamic forms of monopsony. Our study suggests an additional three-part explanation. The first—empirically important for the firms in this study but not a factor much noted in the MW literature—is that even large increases in the MW may be modest as compared to other cost increases that business owners must routinely offset or absorb, thus leading to a lower MW elasticity (per the Marshall–Hicks condition on the size of labor's cost share). The second is that a MW cost increase flows through more adjustment channels than economists have typically considered. The third is that managers see employment cuts as a relatively costly and perhaps counterproductive option, regarding them as a last resort. So viewed, a zero or very small employment effect is compatible with economic theory as long as the theory posits multiple CoA with differential costs.

For space and focus reasons we have refrained from discussion of how our empirical results bear on alternative models of labor markets. We end with brief thoughts on this matter. Since Card and Krueger (1995), debate over the minimum wage has focused primarily on the merits of a competitive versus monopsony model of labor markets. Yet going back in time, an institutional/behavioral model also contended for attention (Lester 1964; Kaufman 2010, 2012), as indicated by Card and Krueger, who dedicate their book to 1950s neo-institutional labor economist Richard Lester. All three models come in different permutations (e.g., search version of the competitive model, structural versus dynamic versions of monopsony) and are further modified and interpreted by individual users, making comparison and empirical discrimination difficult. Given these caveats, our first-line judgment is that all three models capture important elements of truth regarding the MW and adjustment effects. Since all models are abstractions and therefore capture only a slice of reality, this conclusion is not surprising.

The competitive model appears to face the most anomalies. Restaurants in our sample are not wage-takers but have some modest, wage-setting ability, which managers use in setting the entry wage and internal wage structure.[32] Further, our study does not find evidence of clear-cut employment losses—even over 3 years and a 41 percent increase in the MW. Possibly over a still longer time span, or with a larger sample of restaurants, a negative effect might appear. Discussion with owners and evidence outside our study period suggest that negative effects may manifest through reduced store openings and increased risk of store closings. Given this important qualification, the message from our study, along with other results in the literature, is that employment effects in the short-to-medium run are small, perhaps near zero in many settings, and certainly smaller than expected based solely on the competitive model. It would be surprising (at least to us) were an important reason for this result not the behavioral dimensions of wage setting and human resource management (e.g., discretionary effort, equity concerns, management heterogeneity) that the standard competitive and monopsony models largely ignore.

Our suggestive evidence of a nonlinear employment effect, with employment and hours increases in response to small wage increases and declines with respect to large increases, better matches a monopsony than competitive model. The monopsony model, however, at least in its structural version, predicts that prices should fall if the MW leads to higher employment and output, yet we find clear-cut evidence of price increases, as in the broader literature (Aaronson, French, and MacDonald 2008). With respect to prices, the competitive model performs better. The overall behavior of turnover and separation rates does not clearly match a dynamic monopsony story, the exception being restaurants "most affected" by the MW.

The institutional–behavioral model appears consistent with a number of aspects of the MW adjustment process but, at the same time, also faces challenges. Like the monopsony model(s), but for different reasons (the monopsony model emphasizes an upward-sloping supply curve, the institutional–behavioral model changes the demand curve from a well-defined line to a band), it finds support from the small-to-nil employment effect and possible nonlinear dynamic pattern (vertical movement through the band, then up the band). Price increases and the human-behavioral effects also accord with this model.

[32] An earlier study of Atlanta fast food restaurants (Young and Kaufman 1997) identified twenty-four pairs (or triplets or quadruplets) of restaurants located within two blocks of each other—often near the same street intersection. The managers were asked the wage paid for a standardized entry-level new hire (working the lunch shift Monday–Friday, high school degree, no experience). Using only observations above the minimum wage (to avoid truncation bias), high-to-low restaurant wages varied by an average 5 percent (equivalent to $.36 relative to the current $7.25 MW), suggesting even at this finely standardized level restaurant managers have some flexibility in their pay policy, per the area of indeterminacy idea. Lester (1964: 205) estimates an average indeterminacy area of 10–15 percent in cross-firm wage rates.

However, an institutional–behavioral model also predicts, at least relative to the other models, that the internal wage structure is relatively inflexible due to fairness and morale reasons, yet our evidence finds that the MW led to significant internal compression. Also, the model's hypothesis that organizational slack allows managers to absorb some of the MW cost increase faces the challenge of explaining its persistence in firms even after several rounds of tightening up.

As stated above, a variety of models/approaches appear to offer insight into the labor-market effects of minimum wages. Yet we also conclude that in hindsight Richard Lester, the strongest proponent of an institutional–behavioral explanation for a (near) zero employment effect, probably captured more of the reality of minimum wages than neoclassical price theorists have been willing to accommodate. Such a conclusion suggests that a broad CoA framework, as applied in this study, may usefully inform not only professional thinking and ongoing public policy debate on the minimum wage, but on other labor market issues as well.

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Minimum Wage Effects throughout the Wage Distribution

David Neumark
Mark Schweitzer
William Wascher

ABSTRACT

This paper provides evidence on a wide set of margins along which labor markets can adjust in response to increases in the minimum wage, including wages, hours, employment, and ultimately labor income. Not surprisingly, the evidence indicates that low-wage workers are most strongly affected, while higher-wage workers are little affected. Workers who initially earn near the minimum wage experience wage gains. Nevertheless, their hours and employment decline, and the combined effect of these changes on earned income suggests adverse consequences, on net, for low-wage workers.

I. Introduction

Labor markets can adjust along a variety of margins in response to increases in the minimum wage. For example, employers may alter the number of workers employed at an establishment, or they may adjust the average number of hours worked by each employee. In addition, firms may alter the mix of workers

David Neumark is a senior fellow at the Public Policy Institute of California, a professor of economics at Michigan State University, and a research associate of the NBER. Mark Schweitzer is an economist at the Federal Reserve Bank of Cleveland. William Wascher is assistant director in the Division of Research and Statistics, Board of Governors of the Federal Reserve System. *The authors are grateful to Scott Adams for outstanding research assistance, and to seminar participants at Michigan State, the University of Kentucky, UC-Berkeley, the Federal Reserve Bank of Cleveland, and Florida State University for helpful comments. The views expressed do not necessarily reflect the views of the Public Policy Institute of California, the Federal Reserve Board, the Federal Reserve Bank of Cleveland, or their staffs. The data used in this article can be obtained beginning October 2004 through September 2007 from Mark Schweitzer, Federal Reserve Bank of Cleveland, P.O. Box 6387, Cleveland, OH 44101.*
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employed following an increase in the minimum wage, essentially attempting to realign the marginal product of their workers with the wages they are paid. As a result of these adjustments, the effects of minimum wages may extend beyond workers whose wages are directly impacted by the higher floor. Our evidence indicates that minimum wage increases adversely affect workers initially earning near the minimum wage, but have little impact on higher-wage workers. In particular, although wages of low-wage workers rise, their hours and employment fall. The combined effect of these changes is a decline in earned income.

Past minimum wage research focuses mainly on employment effects, and fails to distinguish minimum wage effects at different parts of the wage distribution. Consequently, this past research provides insufficient information with which to evaluate the policy implications of raising the minimum wage, in particular whether such increases help low-wage workers. In contrast, this paper generates a richer description of the effects of the minimum wage on labor markets, providing evidence on a wide set of the margins along which labor market adjustments to minimum wages may occur, and how the adjustments vary at different points of the wage distribution; we provide a particularly sharp focus on minimum wage effects at the lower end of the wage distribution.

II. Existing Research

Our efforts to distinguish minimum wage effects in different parts of the wage distribution differentiates our approach from most of the existing work on minimum wages, which—in order to focus on a set of relatively low-skilled workers—typically studies employment effects for teenagers or a closely related group. However, the focus on employment effects for teenagers is arguably far removed from the most pertinent policy questions, for at least three reasons.

First, policymakers typically are most concerned with adults working near the minimum wage, because young workers are on the early part of their experience profile and hence are likely to grow out of minimum wage jobs, while adults working at minimum wage jobs are more likely to be permanent low-wage workers. In addition, teenagers are more likely to be secondary earners. Second, because many teenagers and young adults earn wages well above the minimum, estimates of disemployment effects for young workers as a whole may mask larger disemployment effects for the lowest-wage workers, and thus overstate the resulting income gains experienced by low-wage workers. Third, the emphasis on employment effects provides too narrow a picture of the effects of minimum wages on the economic well-being of low-wage workers. On the negative side, hours could fall in response to minimum wage increases, while on the positive side minimum wages may generate wage increases above the minimum. We examine the consequences of minimum wages for employment, wages, and hours, as well as the overall impact on labor income.[1] In all cases, we isolate the effects of minimum wages in different parts of the distribution of wages.

1. A more overriding policy concern is the effects of minimum wages on family incomes; see Neumark et al. (1998).

Two recent papers move beyond the estimation of employment effects for teenagers or young adults to focus more sharply on workers who are most likely to be affected by the minimum wage. Abowd et al. (1999) examine individual-level panel data for France, where the real minimum wage rose throughout their sample period (1981–89), and for the United States, where it fell (1981–87). They study minimum wage effects in two opposite but closely related ways: In France, they condition on initial employment and test for disemployment effects among workers who are "caught" by minimum wage increases, while in the United States they look at individuals who are "released" by the falling real minimum wage. For both countries, Abowd et al. report considerably larger disemployment effects of minimum wages for workers constrained by the minimum than for workers with marginally higher wages. Currie and Fallick (1996) carry out a similar analysis using NLSY data for the United States. They estimate the employment effects of the 1980 and 1981 federal minimum wage increases, defining as the treatment group workers whose wage prior to the increase was between the old and the new minimum wage, and as the control group workers earning near but above the minimum wage. Currie and Fallick find that workers bound by the minimum were about 3 percent less likely than the control group to be employed after the minimum wage increase; the estimated employment elasticity for workers bound by the minimum is about −0.4. The elasticities estimated by Abowd et al. are at least as large.

Some researchers have examined other margins of adjustment to minimum wage increases. The most extensive body of research exploring other margins of adjustment studies the extent to which minimum wage increases lead to positive "ripple" effects on the wages of workers already earning more than the new minimum. Gramlich (1976) originally broached this question, suggesting that standard substitution effects or union-related relative wage considerations might lead to increases in the wages of higher-skilled workers following an increase in the legislated minimum wage. Another possibility is that the labor supply of higher-skilled workers might increase as lower-skilled workers (in the same family) become disemployed or face lower hours as a result of minimum wage increases, leading to a decline in wages for higher-skilled workers. Gramlich presents evidence suggesting that minimum wage increases raise average wage rates by about twice what would be predicted from the direct impact of minimum wage increases on workers for whom the minimum is binding (ignoring possible employment effects). But because Gramlich relies on aggregate data, he cannot examine where in the wage distribution the wage spillovers occur. Grossman (1983) also presents evidence consistent with ripple effects from minimum wages.

More recent analyses use empirical methods that more directly reveal the impacts of minimum wages on the wage distribution. For example, DiNardo et al. (1996) present a semi-parametric analysis of how changes in national minimum wages have affected wage inequality, while Lee (1999) examines the impact of minimum wages on the wage distribution in more detail using state-level variation. Both papers find evidence suggestive of positive spillovers from minimum wages to other wages, as does more limited evidence in Spriggs (1993) and Card and Krueger (1995, Ch. 9), and evidence for Canada reported by Green and Paarsch (1998).

The DiNardo et al. (1996) and Lee (1999) papers differ from ours in a few important ways. First, while they are concerned with trying to estimate how the distribution of wages changes as a result of minimum wage increases, we try to estimate the actual impact of minimum wages on workers at different points of the initial distribution of wages relative to the minimum. Second, we study additional outcomes (hours, employment, and labor income) to provide a more comprehensive analysis of the consequences of minimum wage increases. Finally, DiNardo et al. (1996) and Lee (1999) focus more on how minimum wages sweep up workers in the bottom tail of the wage distribution, as opposed to an analysis of the effects on the wage distribution above but near the new minimum.

Much less research looks at hours effects, and what there is focuses on the probabilities of part-time and full-time employment. Gramlich (1976) finds that minimum wages reduce full-time employment and increase part-time employment of teenagers and adult males; although an overall disemployment effect is apparent for teenagers only, the switch from full-time to part-time is consistent with hours reductions for both groups. Hungerford (2000) reports that minimum wages appear to increase the proportion of involuntary part-time workers among less-educated teenagers, and among blacks across age and education categories. In contrast, Cunningham (1981) reports evidence from an earlier period suggesting that minimum wages discourage part-time employment and boost full-time employment, as do Katz and Krueger (1992) using data from fast-food restaurants in Texas.[2] Finally, Zavodny (2000) finds that teenagers who remain employed following a minimum wage increase tend to experience an increase in hours worked, roughly offsetting the job losses incurred by other teens.

One study that comes closer to our more comprehensive approach of estimating the effects of minimum wages on wages, employment, hours, and income is Linneman (1982). He uses PSID data from 1973 to predict wages in 1974 and 1975, and on the basis of the predicted wages identifies workers who would be bound by the increases in the minimum wage in 1974 and 1975. His findings indicate hours (and to a lesser extent employment) reductions among constrained workers. However, he also estimates hours and employment effects for workers in various wage intervals above the minimum, finding that individuals just above the minimum (relative to workers further above the minimum) experience reduced employment rates but increased hours.

We have two main criticisms of Linneman's analysis. First, his estimates of the effects of the minimum wage on income are not based on actual income in 1974 and 1975, but are imputed from the estimated hours and employment effects. As a result, the estimates take no account of the effects of the minimum wage on the wages of workers earnings more than the minimum, and the imputation method takes no account of the distribution of wage and hours effects across individuals.[3] Second, his approach does not provide a credible counterfactual for the experiences of the group affected by the minimum wage increase (Card and Krueger 1995, p. 224).

We address the first set of shortcomings by looking at wage and income effects independently. We provide a credible counterfactual by using flexible estimates of

2. Neumark and Wascher (2000) and Michl (2000) touch on the issue of effects of minimum wages on employment and hours in the context of the Card and Krueger (1994) New Jersey-Pennsylvania minimum wage study.

3. In addition, Linneman does not compute standard errors for the income effects he estimates, so there is no way to determine which estimated effects are significant. Our approach readily yields standard errors of the estimated effects on income.

underlying wage, hours, employment, and income changes of the nonaffected population, and using state variation in minimum wages to obtain treatment and control groups. More generally, we provide a fuller characterization of minimum wage effects throughout the wage distribution, and update and strengthen the analysis by taking advantage of the state-level variation in minimum wages that has been fruitfully exploited in the new minimum wage research.

III. Data

Our basic approach is to estimate models for changes in wages, hours, employment, and income, using data on individuals in matched monthly CPS outgoing rotation group files for the period 1979–97. While household identifiers are available for doing the matching, individual identifiers are not. This raises two issues. First, to ensure that we match individuals correctly, we filter the data through a procedure that uses sex and age as primary characteristics for establishing a match. A match occurs when a household identifier-primary characteristics cell includes at least one pair of first-year and second-year records. For instance, a 31-year-old female in the first year and a 32-year-old female in the same household in the second year will be placed in the same cell. In the event of multiple matches we use a set of tiebreakers, including factors such as education. This tie-breaking phase checks for the correct matches in the cell at the most detailed partition of the specified variables first. If no match is found, then variables are systematically dropped to arrive at a match. Additional steps are conducted to match others that may have been missed in the first step. For example, since the CPS is not necessarily conducted on the same calendar day in subsequent years, a 31-year-old female and a 33-year-old female may be the same person, but they would have been excluded in our first step. There are two sets of months that cannot be matched to observations 12 months ahead because of changes in the sample in response to decennial Censuses of Population: July 1984–September 1985 and June 1994–August 1995.

Second, about 20 percent of the individuals in the outgoing rotation groups who can potentially be matched across years are not successfully matched, most likely because of a change in residence. When we weight the observations, we adjust the sampling weight to account for the possibility that certain individuals have a lower probability of being in the survey in consecutive years and thus are less likely to be included in our matched sample. This adjustment is based on logit model estimates of the probability of a match as a function of demographic characteristics, with the adjusted weight an estimate of the inverse of the probability of being in our matched sample of families. Of course this procedure does not correct for nonrandom matching that, conditional on these observables, is correlated with changes in outcomes and therefore possibly also with minimum wage changes. We conjecture that, if anything, families most adversely affected by minimum wage increases tend to move away from areas where minimum wages have increased and toward areas where they have not. If so, the bias from nonrandom matching will tend to understate any adverse consequences of minimum wage increases.

The outcomes we study are wages, hours, employment, and labor income. We attempt to construct straight-time wages (excluding tips, commissions, and overtime) by using a reported hourly wage—as opposed to usual weekly earnings/usual weekly hours—whenever the former is reported. The weekly measure explicitly includes tips, commissions, and overtime, while the hourly wage measure is less likely to include these. Beginning with the redesigned CPS in 1994, the hourly wage measure was changed so as to explicitly exclude tips, commissions, and overtime, but we assume that the year effects will pick up the influence of this change on average wage changes. For hours, we use usual weekly hours (which is coded as missing beginning in 1994 if the respondent indicates variable hours), and for labor income we use the product of the wage and hours. The employment variable is an indicator equal to one for workers employed during the survey week.

IV. Empirical Framework

We illustrate our strategy by focusing on the estimation of the effects of minimum wages on the wage distribution, although the discussion generalizes to the other dependent variables we consider. To motivate the research design, we first discuss the relatively simpler issue of estimating contemporaneous effects, which illustrates our strategy of allowing the effects of minimum wages to differ across the wage distribution while controlling in a flexible fashion for other sources of changes in wages. In particular, we estimate the contemporaneous effects from the specification:

$$(1)\quad \frac{w^2_{isym} - w^1_{isym}}{w^1_{isym}} = \alpha + \sum_j \beta_j \frac{MW^2_{sym} - MW^1_{sym}}{MW^1_{sym}} \cdot R(w^1_{isym}, MW^1_{sym})^j$$
$$+ \sum_j \gamma_j R(w^1_{isym}, MW^1_{sym})^j$$
$$+ \sum_j \phi_j R(w^1_{isym}, MW^1_{sym})_j \cdot \frac{w^1_{isym}}{MW^1_{sym}}$$
$$+ X^1_{isym}\,\delta + M_{im}\lambda + S_{is}\cdot Y_{iy}\pi + \varepsilon_{isym}.$$

In this specification, the superscripts 1 and 2 denote the Year 1 or Year 2 observation in the matched CPS data; the subscript i denotes the individual, and the subscripts s, y, and m refer to state, calendar year (as opposed to the 1 and 2 superscripts, which indicate year in sample), and month. MW is the higher of the state or federal minimum wage, and w denotes the individual's wage. The controls in X are sex, race, ethnicity, education, and a quartic in potential experience (all defined as of Year 1 for each individual), which permit average wage changes to differ across workers distinguished by these characteristics. S denotes state dummy variables and Y year dummy variables. We include the full set of state-year interactions, which subsume standard state and year effects and capture nonindependence between observations from the same state and year, including that associated with the effects of omitted variables that vary at the state-year level. Important omitted variables might include state-specific business cycle effects on the labor market, often captured in a more restrictive fashion by including the state-level unemployment rate. Because the data set covers all months of the basic CPS, we also include calendar month dummy variables (M) to control for seasonality—summer or holiday employment, for example—that might be spuriously correlated with minimum wage changes. Finally, ε is a random error term assumed to have zero expectation conditional on the regressors, and to be independent across state, year, and month cells conditional on the regressors; given that the regressors on which we condition include month dummy variables and state-year interactions, we have already controlled for many sources of nonindependence of observations across space and time. In the estimation of the regression equations, we compute standard errors that are robust to heteroskedasticity.

R_j denotes a set of dummy variables that describe the level of the Year 1 wage relative to the Year 1 minimum wage; these are spelled out fully in Table 1. The R_js control for differences in wage changes at different points of the wage distribution for reasons unrelated to changes in minimum wages, including measurement error in wages that can lead to a negative correlation between measured Year 1 and Year 2 wages.[4] In addition, we include interactions of the R_js with the ratio of the individual's wage to the minimum wage; with the interactions, we have a spline specification without restricting the lines to join at the knot points (a dummy/spline specification). These additional interactive terms allow wage changes to differ within the cells defined by the R_js, hence allowing a more flexible specification of underlying wage changes.

The parameters of direct interest are the β_js, which capture the effects of an increase in the minimum wage for each region in the wage distribution defined by the dummy variables R_j, relative to the baseline changes for the unaffected workers captured by the control variables described in the previous paragraph. The β_j coefficients are still identified with the full set of state-year dummy interactions included, because the minimum wage change multiplying $R(\cdot,\cdot)$ may vary from month to month within the year. In principle, we could include month-state interactions, in which case we could only identify the effects of minimum wages at different points of the wage distribution relative to some omitted category, with the natural choice being the highest-wage workers. However, it is inappropriate to assume a priori that higher-wage workers are not affected by minimum wage increases; supply shifts or relative demand shifts could affect these workers indirectly. Nonetheless, in most of the results reported below, the effects of minimum wages in the upper cell of the wage distribution (6–8 times the minimum) are relatively small, and thus the estimates

4. While not reported in the tables, the estimated coefficients on the dummy variables, their interactions with the relative minimum wage variables, and the other controls can be combined to estimate how the dependent variables are changing in each cell of the distribution of wages relative to minimum wages. For the two measures directly tied to wages—wages themselves, and earnings—the estimates indicate substantial regression to the mean. That is, the estimated wage and earnings growth is very strong in the bottom part of the distribution, and weak or negative at the top. (This is far different from results showing, for example, slower wage growth in the bottom decile of the wage distribution than in the middle or at the top, since such results are based on sample means or percentiles, rather than individual data that are potentially subject to severe regression to the mean. So while one individual with a wage in the bottom decile who subsequently earns higher wages is replaced by another whose wages have most likely fallen, that is not the case with individual-level data.)

for low-wage workers essentially mirror what we obtained when outcomes were defined relative to this cell.[5]

Our specification of minimum wage effects is highly flexible in that it uses a set of dummy variables that divide up the initial wage distribution into fairly narrow regions—especially near the minimum wage. But this flexible specification imposes strong demands on the data. An alternative, therefore, is to impose some smoothness on the estimates—in particular, on how the minimum wage effect varies across the wage distribution—by using a high-order polynomial in the wage relative to the minimum wage. We therefore consider estimates using an alternative specification:

$$(1') \quad \frac{w^2_{isym} - w^1_{isym}}{w^1_{isym}} = \alpha + \sum_{j=1}^{J} \beta_j \frac{MW^2_{sym} - MW^1_{sym}}{MW^1_{sym}} \cdot \left(\frac{w^1_{isym}}{MW^1_{sym}}\right)^j + \sum_{j=1}^{J} \gamma_j \left(\frac{w^1_{isym}}{MW^1_{sym}}\right)^j + X^1_{isym}\delta + M_{im}\lambda + S_{is} \cdot Y_{iy}\pi + \varepsilon_{isym}.$$

We experimented with polynomials of different orders and settled on seven as the order required to capture the variation in estimated minimum wage effects. The qualitative results were not sensitive to increasing or decreasing this order somewhat. Although the remaining discussion in this section is couched in terms of Specification 1, it carries over to Specification 1′ as well.

Previous research indicates that a significant portion of the total minimum wage effect on employment occurs with a one-year lag (Neumark and Wascher 1992; Baker et al. 1999). Some have argued that high turnover for low-wage workers implies that adjustments to minimum wages will occur quickly (for example, Brown et al. 1982). But because changes in technology and management needed to replace low-skilled workers may take time, the existence of lagged adjustment effects is an empirical question. In addition, our earlier research on minimum wage effects on family incomes (Neumark et al. 1998) indicated that minimum wage increases had beneficial effects on low-income families contemporaneously, but adverse effects after one year. This pattern is consistent with upward wage adjustments occurring quickly, and employment and hours adjustments occurring with a lag. As this paper also looks at incomes, a similar specification seems appropriate.

A complication arises, however, in estimating lagged effects with our data: We cannot define a comparable set of R_js in the year prior to Year 1 (call it Year 0) because the matched CPSs only include wage data for Year 1 and Year 2.[6] To get around the absence of the earlier data, we instead define the R_js that we use to identify the lagged effects based on the Year 1 wage (relative to the Year 1 minimum), and estimate the equation

5. These results are available upon request.

6. In principle the missing Year 0 data could be avoided by using a data set covering 24 months or more, such as some of the SIPP or NLS panels. However, the rich characterization of minimum wage effects that we are attempting to provide in this paper requires very large samples, making the CPS the only feasible source of data.

$$(2)\quad \frac{w^2_{isym} - w^1_{isym}}{w^1_{isym}} = \alpha + \sum_j \beta_j \frac{MW^2_{sym} - MW^1_{sym}}{MW^1_{sym}} \cdot R(w^1_{isym}, MW^1_{sym})_j$$
$$+ \sum_j \beta^L_j \frac{MW^1_{sym} - MW^0_{sym}}{MW^0_{sym}} \cdot R(w^1_{isym}, MW^1_{sym})_j$$
$$+ \sum_j \gamma_j R(w^1_{isym}, MW^1_{sym})_j + \sum_j \phi_j R(w^1_{isym}, MW^1_{sym})_j \cdot \frac{w^1_{isym}}{MW^1_{sym}}$$
$$+ X^1_{isym}\delta + M_{im}\lambda + S_{is} \cdot Y_{iy}\pi + \varepsilon_{isym}.$$

In this specification, the lagged effects associated with a minimum wage increase from Year 0 to Year 1 are defined conditional on where a worker's Year 1 wage was in the wage distribution relative to the minimum in Year 1. (The superscript 0 represents the year prior to Year 1.) This specification of the lagged effect has the same interpretation as the usual lagged effect if the individual's wage history does not matter. That is, it reflects the usual lagged effect if, conditional on w^1 relative to MW^1, the Year 1 to Year 2 effect of the minimum wage does not depend on the path of wage rates up to w^1 (for example, whether an individual's wage was at w^1 all along or instead was swept up to w^1 by the initial minimum wage increase). To see why this assumption is implicit in the specification, consider the most general form of the regression we would use instead of Equation 2 if we actually had three years of data:

$$(3)\quad \frac{w^2_{isym} - w^1_{isym}}{w^1_{isym}} = \alpha + \sum_j \beta_j \frac{MW^2_{sym} - MW^1_{sym}}{MW^1_{sym}} \cdot R(w^1_{isym}, MW^1_{sym}, w^0_{isym}, MW^0_{sym})_j$$
$$+ \sum_j \beta^L_j \frac{MW^1_{sym} - MW^0_{sym}}{MW^0_{sym}} \cdot R(w^1_{isym}, MW^1_{sym}, w^0_{isym}, MW^0_{sym})_j$$
$$+ \sum_j \gamma_j R(w^1_{isym}, MW^1_{sym}, w^0_{isym}, MW^0_{sym})_j$$
$$+ \sum_j \phi_j R(w^1_{isym}, MW^1_{sym}, w^0_{isym}, MW^0_{sym})_j \cdot f\left(\frac{w^1_{isym}}{MW^1_{sym}}, \frac{w^0_{isym}}{MW^0_{sym}}\right)$$
$$+ X^1_{isym}\delta + M_{im}\lambda + S_{is} \cdot Y_{iy}\pi + \varepsilon_{isym}.$$

There are two differences relative to Equation 2. First, the R function is more general in that it allows the minimum wage and baseline effects to depend on wages and minimum wages in both Year 0 and Year 1. For example, R could be a set of dummy variables defined over a grid of values of the wage relative to the minimum wage in each of the two years. Second, and related, in the term involving the coefficients ϕ_j, the baseline effects depend on pairs of values of the wage relative to the minimum wage in each of the two years, rather than on just the Year 1 relative values. The problem, of course, is that we do not have data for three years.

However, if w^0 and its value relative to MW^0 do not affect either baseline wage growth (as captured in the coefficients γ_j and ϕ_j) or the magnitude of the minimum wage effect, once w^1 and MW^1 are known, then Equation 3 reduces to Equation 2.[7]

7. We also assume that the function f applied to the ratio of w^1 to MW^1 is linear.

This is potentially a strong assumption, as it implies, for example, that anticipated wage growth should be unrelated to past wage growth for two workers with the same current wage facing the same minimum wage. In fact, though, our specification allows this restriction to be relaxed in a number of ways, by allowing wage growth to vary with a large set of control variables. Furthermore, because it includes lagged effects, we view this specification as likely to better capture the effects of minimum wages than a specification with exclusively contemporaneous effects. Nonetheless, as the results described below show, the substantive conclusions depend importantly on the inclusion of the lagged effects, and thus in future research it will be worthwhile to consider alternative methods or data sources that probe the sensitivity of the results to the treatment of lagged effects.[8]

To this point, we have described how we estimate contemporaneous and lagged effects of minimum wages conditional on a worker's wage relative to the minimum wage. However, combining the estimated contemporaneous and lagged effects to obtain estimates of the total effects of minimum wages on wages (or hours, employment, or income) is more complicated than in the usual case. In particular, in computing the sum of the contemporaneous and lagged effects we have to keep track of the contemporaneous effect, and measure the lagged effect from the point in the wage distribution that prevails after one year (either because of minimum wage changes or baseline wage changes). That is, because workers experience wage growth whether or not minimum wages increase, and many low-wage workers are on steep regions of experience or tenure profiles, a worker whose wage is near the minimum wage in Year 0 may have a wage significantly above the minimum in Year 1. As a result, the typical minimum-wage worker may move several steps up through the wage categories defined by our R_j variables, and thus measurement of the effects of minimum wages on low-wage workers needs to be conditioned on expected changes in wages for other reasons. In addition, first-year minimum wage effects may move a worker to a different part of the wage distribution, in which case the estimated lagged effect needs to be applied to the region of the wage distribution in which the worker is likely to be found one year after the initial increase.[9]

To estimate the total effects, we consider a set of hypothetical workers based on average characteristics and responses to minimum wage increases in each cell defined by the R_js. The case we consider is a one-time, c percent increase in the minimum wage. We first use our estimates of Equation 2 to predict the contemporaneous wage change for the representative worker in each cell defined by the R_js, using

$$(4)\quad E\left(\left[\frac{w^2 - w^1}{w^1}\right]_j \middle| \frac{MW^2 - MW^1}{MW^1} = c, \frac{MW^1 - MW^0}{MW^0} = 0, \overline{X_j}, \overline{S \cdot Y_j}, \overline{M_j}, \overline{w_j^1}\right)$$

$$= \hat{\alpha} + \hat{\beta}_j \cdot c + \hat{\gamma}_j + \hat{\phi}_j \frac{\overline{w^1{}_j}}{MW^1} + \overline{X_j}\hat{\delta} + \overline{M_j}\hat{\lambda} + \overline{S \cdot Y_j}\hat{\pi} \quad j = 1,\ldots,J,$$

8. An alternative is to try to predict the value of the Year 0 wage (or equivalently to predict the indicators R). But such predictions would be extremely noisy, given that wage regressions do not have great predictive power, and our ability to predict whose wages are in the extremes of the distribution might be particularly weak.

9. Note that the problem of estimating total effects is not related to our need to estimate lagged effects with only two years of data; the same issue would arise with three years of data. The problem arises because an individual's position in the wage distribution can change over time.

where the "hats" on the Greek letters indicate estimates, and the means of X, $S \cdot Y$, M, and w^1 are defined for individuals in cell j. Based on these predictions, and the average value of w^1 in each cell, we can obtain a predicted value of w^2 (denoted w^{2p}) for the representative worker in each cell, which will have been affected both by the minimum wage increase and the control variables.

The next step is to predict the lagged effects that occur one year later. To make this prediction we shift the superscript on the predicted wage from w^{2p} to w^{1p}, use these predicted values to place workers in new predicted cells defined by the R_js (based on MW^1 and w^{1p}), and predict the lagged effect at that point (updating potential experience by one year as well). The equation used for this prediction is

$$(5)\quad E\left(\left[\frac{w^2 - w^{1p}}{w^{1p}}\right]_j \middle| \frac{MW^2 - MW^1}{MW^1} = 0, \frac{MW^1 - MW^0}{MW^0} = c, \overline{X_j}, \overline{S \cdot Y_j}, \overline{M_j}, \overline{w_j^{1p}}\right)$$
$$= \hat{\alpha} + \hat{\beta}_j^L \cdot c + \hat{\gamma}_j + \hat{\phi}_j \frac{\overline{w_1^{1p}}}{MW_1} + \overline{X_j}\hat{\delta} + \overline{M_j}\hat{\lambda} + \overline{S \cdot Y_j}\hat{\pi} \quad j = 1,\ldots,J.$$

The sum of the expressions in Equations 4 and 5 is the implied two-year effect of minimum wage increases on wages for workers in states with minimum wage increases. Note that the lagged effects are based on the average response within a wage category of a set of workers whose wages responded to a minimum wage increase one year prior. Similar expressions with c set to 0 provide estimates of the counterfactual, that is, changes in the wage distribution that would have occurred without minimum wage increases. Changes in the wage distribution for these workers are predicted based on the other control variables. Note that we cannot simply subtract the terms involving these control variables off of Equations 4 and 5 and report the estimated βs multiplied by the minimum wage increase, because these control variables influence w^{1p} in Equation 5.

Although the procedures in this section have been described with the change in the wage as the dependent variable, identical procedures can be used to estimate the effects of minimum wage increases on hours and income. And, with respect to the analysis of employment, the only difference is that we condition on Year 1 employment and look at whether the individual becomes nonemployed in Year 2, so that the dependent variable is an indicator of a change in employment status. We restrict attention to those initially employed because we do not have an initial wage for those initially nonemployed. As a result, care should be taken in interpreting the results as measuring overall employment effects of minimum wages, as labor market entry could either rise or fall in response to minimum wage increases, although we would expect the change to be in the same direction as for those initially employed.

In these cases, we first estimate the contemporaneous effect using an equation corresponding to Equation 4 for the relevant dependent variable. We then predict the Year 1 wage as described above, and use an equation corresponding to Equation 5, again for the relevant dependent variable, to estimate the correct lagged effect. Combining these predicted changes for wages, hours, employment, and income yields a detailed characterization of the effects of minimum wages at different points of the wage distribution.

Finally, hypothesis testing for our estimated total effects is more complicated than in a simple regression analysis. The potential for cross-year and cross-equation correlations of errors, together with the rather complicated way we combine predictions from different equations, makes conventional standard errors difficult to calculate. We therefore instead use bootstrap-based standard errors. Hypothesis testing is done using the realized empirical distributions of the bootstrap estimates, in each case based on 500 repetitions.[10] One could argue that our large sample calls for significance levels for hypothesis testing considerably smaller than the conventional 5 or 10 percent levels. However, our effective sample size is overstated by the number of observations on individuals, because we identify minimum wage effects from state-year variation. In addition, we are estimating effects for narrow subgroups, introducing many parameters to allow for a flexible specification and numerous dimensions of heterogeneity. Given that we exploit the large sample size to put strenuous demands on the data, it is not accurate to think of the sample size getting very large ceteris paribus, relative to other research using conventional significance levels.

V. Results

Table 1 reports descriptive statistics for the full sample, and the subsamples defined by the R_js that break up the distribution of initial wages into cells that—especially near the minimum—are quite disaggregated. To account for rounding and slight reporting errors in wages, note that workers with wages between ten cents less and ten cents more than the minimum are defined as minimum wage workers. The figures in this table are largely as expected. With the exception of workers initially paid below the minimum and workers in the highest wage category, average hours worked per week increase monotonically with the initial wage, from the high 20s—suggestive of a fairly high proportion of part-time workers—to more than 40. Combining hours and wages, weekly labor income (defined in nominal terms) rises monotonically. Teenagers are heavily overrepresented in the lowest wage categories; although they make up only 6 percent of the sample, they comprise 36 percent of minimum wage workers and 29 percent of workers earning above the minimum but below 1.1 times the minimum. Similarly, women, blacks, and Hispanics are overrepresented among low-wage workers.

We next discuss, in turn, minimum wage effects on wages, hours, employment, and labor income. Although the regression estimates do not provide a description of the total (contemporaneous and lagged) effects of minimum wages, we begin with these estimates before moving on to more readily interpretable graphs that report these total effects for representative workers in each region of the wage distribution. We focus first on the dummy/spline specification with dummy variables for each cell of the distribution of wages relative to minimum wages, and the associated interactions. Following a detailed discussion of these estimates, we present results from the more restrictive polynomial specification.

10. Although the errors in the equations for the different dependent variables are potentially correlated, the controls are the same. The wage and hours equations are estimated conditional on employment in both years, and the employment and earnings equations are estimated conditional on employment in Year 1. Thus, for each pair of equations we have seemingly unrelated regressions, for which OLS is efficient.

Table 1
Means, Overall and by Position in the Wage Distribution

Year 1 variables	Proportion (1)	Hours, Employed Year 2 (2)	Weekly Year 1 Labor Income (3)	Age 16–19 (4)	Age 20+ (5)	Women (6)	Men (7)	Black (8)	Hispanic (9)	Nonblack/ non-Hispanic (10)
Full sample	1.00	38.8	377.3	0.06	0.94	0.47	0.53	0.11	0.06	0.83
By Year 1 wage										
$w < MW - \$.10$	0.026	32.0	86.4	0.24	0.76	0.64	0.36	0.14	0.07	0.79
$MW - \$.10 \le w \le MW + \$.10$	0.046	28.0	95.8	0.36	0.64	0.62	0.38	0.17	0.10	0.74
$MW + \$.10 < w \le 1.1{\cdot}MW$	0.031	30.9	114.3	0.29	0.71	0.63	0.37	0.14	0.09	0.77
$1.1 < w/MW \le 1.2$	0.052	33.0	136.9	0.19	0.81	0.61	0.39	0.15	0.10	0.76
$1.2 < w/MW \le 1.3$	0.032	35.8	159.7	0.13	0.87	0.63	0.37	0.14	0.09	0.78
$1.3 < w/MW \le 1.5$	0.084	36.6	184.6	0.09	0.91	0.59	0.41	0.14	0.09	0.78
$1.5 < w/MW \le 2$	0.168	38.9	249.4	0.03	0.97	0.56	0.44	0.13	0.08	0.80
$2 < w/MW \le 3$	0.265	40.5	365.5	0.01	0.99	0.45	0.55	0.10	0.06	0.84
$3 < w/MW \le 4$	0.148	41.4	531.6	0.002	0.998	0.34	0.66	0.08	0.05	0.87
$4 < w/MW \le 5$	0.078	41.9	695.6	0.001	0.999	0.28	0.72	0.07	0.04	0.89
$5 < w/MW \le 6$	0.043	42.1	862.8	0.001	0.999	0.24	0.76	0.06	0.03	0.91
$6 < w/MW \le 8$	0.029	41.8	1,082.6	0.001	0.999	0.20	0.80	0.05	0.03	0.93

Notes: The sample is restricted to wage and salary employees working for a wage in Year 1; this sample includes 847,175 observations. The sample conditioning on Year 2 employment, in Column 2, includes 749,510 observations. Observations with wages less than 50 percent of the minimum wage in Year 1 are excluded, as are observations with Year 1 wages more than eight times the minimum (fewer than 1 percent of the observations), and observations with increases of 1,000 percent or greater in wages or hours (fewer than 1 percent of the observations). All estimates are weighted to adjust for sampling weights and match probabilities (based on demographic characteristics). Proportions black, Hispanic, and nonblack/non-Hispanic do not always add to one, because of rounding.

A. Wages

Columns 1 and 1′ of Table 2 report the estimates of β and β^L, respectively, in Equation 2, using the percent change in wages as the dependent variable. The contemporaneous effects (the βs) are straightforward to interpret, as they measure the percentage change in the wage resulting from a 1 percent increase in the minimum wage. The estimates reveal pronounced, statistically significant positive effects near the minimum. In particular, for workers at or just above the minimum wage, the elasticity of wages with respect to the minimum is about 0.8. The elasticity falls to about 0.25 to 0.4 for workers between 1.1 and 1.5 times the minimum. For workers below the minimum, the estimated elasticity actually exceeds one.[11] Higher into the initial wage distribution, the estimated elasticities become quite small, although some are significant.

The estimates of the lagged effects (the β^Ls) also reveal some interesting patterns. In particular, especially near the minimum, the estimated coefficients are strongly negative. This negative lagged effect implies that some of the wage gains associated with minimum wage increases are "given back" in the following year. These give-backs have not been noted in previous work on the effects of minimum wages on the wage distribution. However, it is perhaps not surprising that employers take advantage of inflation in subsequent years to realign wages, partly undoing the effects of legislated nominal wage increases for low-wage workers.

In the upper left-hand panel of Figure 1, we display graphically the estimated effects on the wage distribution based on the calculation described in the previous section.[12] In this figure (and subsequent ones) we report the effect of a one-time 10 percent increase in the minimum wage. The figure displays the differential between the percentage change in the wage experienced by workers in states with the 10 percent minimum wage increase, and workers in states without an increase. The light bars simply replicate the estimated contemporaneous effects that were reported in Column 1 of Table 2. Of more interest are the estimated total effects, the gray bars, which incorporate lagged effects of minimum wages. As suggested by the negative estimates of the β^Ls in Column 1′ of Table 1, the effects of minimum wages on the wage distribution are tempered considerably when lagged effects are incorporated. Near the minimum wage, the elasticity of the wage with respect to the minimum falls to about 0.4. The estimated elasticities then decline for the cells slightly higher in the wage distribution, and become negative, albeit small, for cells more than twice the minimum. The graph also displays information on the p-values associated with each estimated effect (for the null hypothesis of no effect, versus the two-sided alternative). As indicated by the presence of three or four asterisks, most of the elasticities greater than 0.1 or so (in absolute value) are statistically significant at the 5 percent level or better.

11. We suspect that estimates for the part of the wage distribution below the minimum are less reliable for a couple of reasons, including regression to the mean in wage data erroneously reported as below the minimum, and transitions between uncovered or tipped jobs and covered jobs. The latter scenario is likely to have a positive influence on the estimate of β for this cell, because the jump in the wage upon moving to a covered job will be higher the more the minimum has increased. Finally, we conjecture that minimum wage increases are followed by upward (perhaps temporary) ratcheting of minimum wage compliance, as employers and workers become better informed about prevailing minimum wages.

12. Note that Table 2 reports conventional regression standard errors, while Figure 1 and subsequent ones report bootstrapped standard errors. As might be expected, for the contemporaneous effects these standard errors are almost identical.

Table 2
Effects of Minimum Wages on Wages, Hours, Employment, and Income

	Wages		Hours, Conditional on Year 2 Employment		Employment		Weekly Labor Income	
	Current	Lagged	Current	Lagged	Current	Lagged	Current	Lagged
Percent change in minimum wage × dummy variables for	(1)	(1′)	(2)	(2′)	(3)	(3′)	(4)	(4′)
$w < MW - \$.10$	1.39	−0.76	−0.30	0.23	0.034	−0.014	1.00	−0.75
	(0.25)	(0.23)	(0.13)	(0.13)	(0.069)	(0.066)	(0.37)	(0.33)
$MW - \$.10 \leq w \leq MW + \$.10$	0.79	−0.60	−0.09	−0.52	−0.115	−0.065	0.19	−1.47
	(0.10)	(0.09)	(0.11)	(0.10)	(0.062)	(0.060)	(0.20)	(0.19)
$MW + \$.10 < w \leq 1.1 \cdot MW$	0.78	−0.29	−0.05	−0.23	−0.145	0.100	0.38	−0.49
	(0.12)	(0.11)	(0.10)	(0.09)	(0.069)	(0.063)	(0.21)	(0.18)
$1.1 < w/MW \leq 1.2$	0.41	−0.42	0.06	−0.20	−0.074	−0.003	0.37	−0.58
	(0.08)	(0.08)	(0.07)	(0.07)	(0.048)	(0.049)	(0.16)	(0.14)
$1.2 < w/MW \leq 1.3$	0.36	−0.27	0.16	−0.11	−0.169	0.067	0.26	−0.29
	(0.10)	(0.10)	(0.07)	(0.07)	(0.059)	(0.052)	(0.16)	(0.16)
$1.3 < w/MW \leq 1.5$	0.26	−0.27	0.11	−0.17	−0.030	0.014	0.29	−0.45
	(0.06)	(0.06)	(0.05)	(0.04)	(0.036)	(0.038)	(0.10)	(0.10)
$1.5 < w/MW \leq 2$	0.16	−0.12	0.04	−0.01	−0.002	−0.004	0.15	−0.16
	(0.04)	(0.05)	(0.03)	(0.03)	(0.025)	(0.024)	(0.06)	(0.06)
$2 < w/MW \leq 3$	0.06	−0.14	−0.00	0.005	0.038	0.012	0.03	−0.07
	(0.03)	(0.03)	(0.02)	(0.02)	(0.020)	(0.021)	(0.05)	(0.04)
$3 < w/MW \leq 4$	0.00	−0.18	−0.04	0.09	0.000	−0.040	−0.06	−0.12
	(0.03)	(0.03)	(0.02)	(0.02)	(0.023)	(0.023)	(0.05)	(0.04)

Table 2 (*continued*)

	Wages		Hours, Conditional on Year 2 Employment		Employment		Weekly Labor Income	
	Current	Lagged	Current	Lagged	Current	Lagged	Current	Lagged
$4 < w/MW \leq 5$	0.03	−0.12	−0.01	0.11	0.027	0.047	0.01	−0.04
	(0.04)	(0.04)	(0.02)	(0.03)	(0.025)	(0.027)	(0.05)	(0.05)
$5 < w/MW \leq 6$	0.08	−0.23	−0.05	0.11	0.018	−0.010	0.10	−0.14
	(0.04)	(0.04)	(0.03)	(0.03)	(0.032)	(0.036)	(0.05)	(0.06)
$6 < w/MW \leq 8$	0.09	−0.21	−0.09	0.09	0.011	0.045	0.02	−0.13
	(0.04)	(0.05)	(0.04)	(0.04)	(0.032)	(0.037)	(0.06)	(0.06)
Adjusted R^2	0.16		0.04		0.31		0.07	
N	749,510		749,510		847,175		847,175	

Notes: Dependent variable is the percent change from Year 1 to Year 2 in wages (Columns 1–1′), hours conditional on employment (Columns 2–2′), and labor income (Columns 4–4′). Dependent variable is a dummy for employment in Year 2, in Columns 3–3′. In Columns 3 and 3′ coefficient estimates are multiplied by 100, so reported effects are for a 100 percent increase in the minimum wage; in the other columns the coefficient estimates are not multiplied, so reported effects are for a 1 percent increase in the minimum wage. In all columns the sample is restricted to individuals working for a wage in Year 1; in Columns 1–2′ the sample is also restricted to individuals working for a wage in Year 2. The percent change variables are defined from Year 1 to Year 2, and the dummy variables indicated in the left-hand column are based on the Year 1 wage relative to the Year 1 minimum. Each specification also includes the full set of dummy variables for the wage relative to the minimum wage that are used to define the interactions reported in the table, interactions of these with the wage relative to the minimum wage, as well as a quartic in potential experience, and dummy variables for female, black, Hispanic, high school dropout, some college, college graduate, and postgraduate education, and calendar month (to control for seasonal effects); all are defined as of Year 1. In addition, the specifications include fixed effects for each state-year combination; all coefficients are identified because there are within-year minimum wage increases. All estimates are OLS with robust standard errors. As explained in the text, lagged effects are not equivalent to two-year effects; the latter are displayed in the accompanying figures.

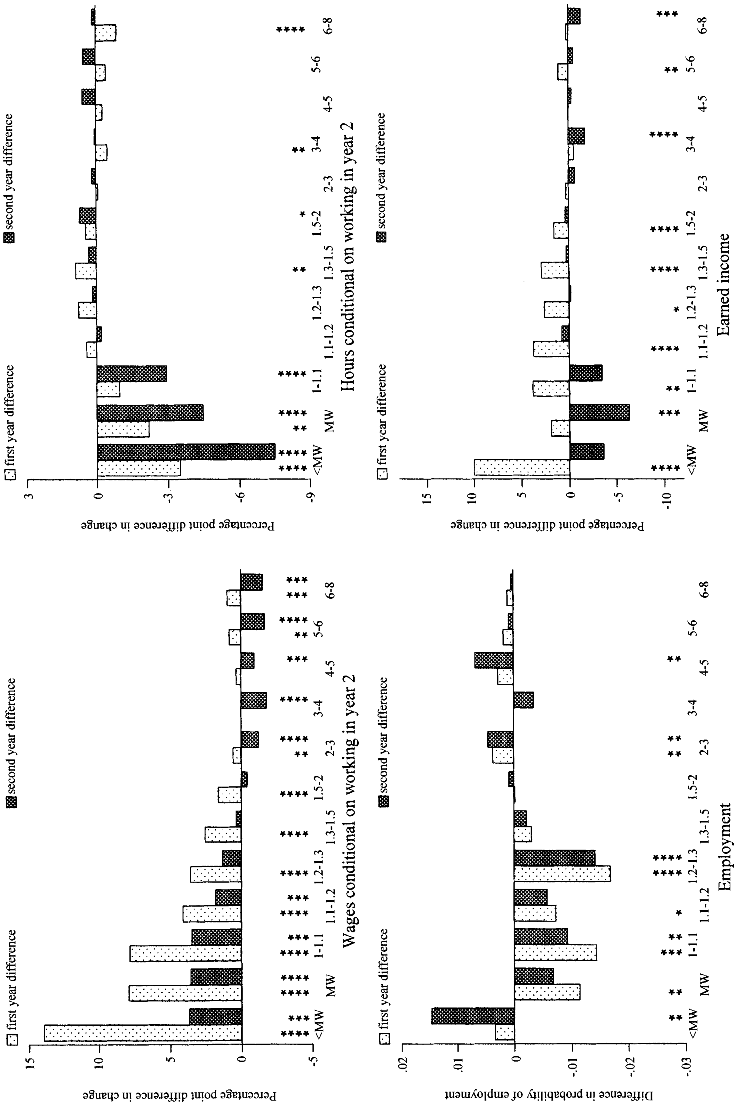

Figure 1

Effects of 10 Percent Minimum Wage Increase, Full Sample, Dummy/Spline Specification

Note: Significance levels for two-sided tests are indicated as follows: 1 percent (4 asterisks); 5 percent (3 asterisks); 10 percent (2 asterisks); 15 percent (1 asterisk).

Taken as a whole, these results indicate that minimum wage increases raise the wages of the lowest-paid workers. However, the evidence of wage declines for workers initially earning higher wages suggests either that outward labor supply shifts for higher-wage workers outweigh increases in labor demand from substitution effects, or that the scale effects resulting from higher overall labor costs outweigh the substitution effects. In addition, the results in the figure indicate that wage losses at the upper end of the wage distribution are smaller in percentage terms than gains at the lower end, but larger in absolute terms. With more workers represented in some of the higher-wage cells, the findings may suggest that minimum wages are a rather inefficient tax and transfer scheme.

B. Hours

We next turn to effects on hours (conditional on remaining employed) for workers in different regions of the wage distribution. The regression estimates are reported in Columns 2 and 2′ of Table 2. The estimates reveal little evidence of contemporaneous hours reductions for workers paid at or slightly above the minimum wage, although there is a significant estimated decline for workers initially earning below the minimum. For workers earning between 1.2 and 1.5 times the minimum, there is evidence of moderate but statistically significant increases in hours. The lagged effects are more striking, with significant hours reductions for individuals at or above the minimum wage, up to about 1.5 times the minimum.

The full set of contemporaneous and total effects is displayed in the upper right-hand panel of Figure 1. For individuals below the minimum, the estimated total effect on hours is more negative than the contemporaneous effect alone. The more negative total effect occurs because the wage gains experienced by these workers (see the upper left-hand panel of the figure) put them into higher cells in the wage distribution, where, as reported in Table 2, there are lagged hours reductions. More importantly, the figure reveals hours reductions for workers initially paid at or just above the minimum wage, with elasticities near −0.3; the estimates for both cells are strongly significant. In contrast, there are no significant total effects for higher-wage workers, although the point estimates are nearly all positive. Coupled with reductions in wages for higher-wage workers, such hours increases suggest that a higher minimum wage leads them to increase labor supply—perhaps in response to reductions in hours for low-wage family members.

C. Employment

With respect to employment, the contemporaneous estimates in Column 3 of Table 2 reveal disemployment effects for individuals at the minimum and just above the minimum (up to 1.3 times the minimum); these estimates are statistically significant at the 5 or 10 percent level. The estimated elasticities range from −0.12 to −0.17 (with the exception of the cell for workers with wages 1.1 to 1.2 times the minimum), close to the so-called consensus range of estimated disemployment effects for teenagers (Brown et al. 1982; Fuchs et al. 1998). Past research has focused on teenagers because they are viewed as low skill. Using low wages to identify low-skill workers instead seems to yield similar results.

However, as suggested by the lagged estimates in Column 3′, and as displayed in the lower left-hand panel of Figure 1, the disemployment effects are partially offset in the second year, with the total effect becoming smaller and statistically weaker except for workers with initial wages between 1.2 and 1.3 times the minimum. The pattern of stronger employment effects initially, but stronger hours effects later, is consistent with employers first laying off part-time workers, reducing fixed costs of labor, and then later adjusting hours downward.

D. Income

Finally, we turn to earned income. Based on the results reported above, a higher minimum wage could have either a positive or negative effect on the earned income of low-wage workers. Low-wage workers experience wage gains as a result of minimum wage increases, but also experience hours and employment declines. And, as noted previously, we cannot simply use the elasticities for hours, employment, and wages to predict income effects, since we do not know the joint distributions of changes in these variables. Columns 4 and 4′ of Table 2 report the regression estimates. The contemporaneous effects are positive (and significant for most cells) for workers initially earning up to twice the minimum wage. In contrast, the lagged effects are uniformly negative and quite strong, especially up to about twice the minimum.

The lower right-hand panel of Figure 1 reports the one-year and total effects. The contemporaneous effects might be interpreted as suggesting that minimum wages increase the earnings of low-wage workers; the elasticities are in the 0.2 to 0.4 range for workers initially earning up to twice the minimum and are statistically significant. However, adding in the lagged effects reverses this conclusion. As shown by the dark bars, the total effects indicate that workers initially below the minimum, at the minimum, and up to 1.1 times the minimum experience income declines. The estimated effect for minimum wage workers is on the order of a 6 percent decline and is statistically significant at the 5 percent level. The source of the reversal is clear from the other panels of the figure. Although disemployment effects are tempered, hours reductions after one year are much sharper, and the wage gains considerably weaker.

Overall, the analysis indicates that very low-wage workers are not helped and are more likely hurt by minimum wage increases. Although minimum wages bump up wages of these workers, hours reductions, in particular, interact with changes in wages in such a way that earned income declines.

In Figure 2, we show estimates based on the polynomial specification of variation in minimum wage effects throughout the wage distribution; this specification is more restrictive but imposes some smoothness on how these effects change across the distribution. For comparison purposes, we report estimated effects of minimum wages for the same cells of the wage distribution (based on the mean for each cell) as were used in the dummy variable specification.

The first feature of the results to notice is the greater smoothness, as the effects typically change monotonically up to about 1.5–2 times the minimum wage. For wages, hours, and earned income, the results are qualitatively similar to those in Figure 1. There is evidence of positive wage effects one year after a minimum wage

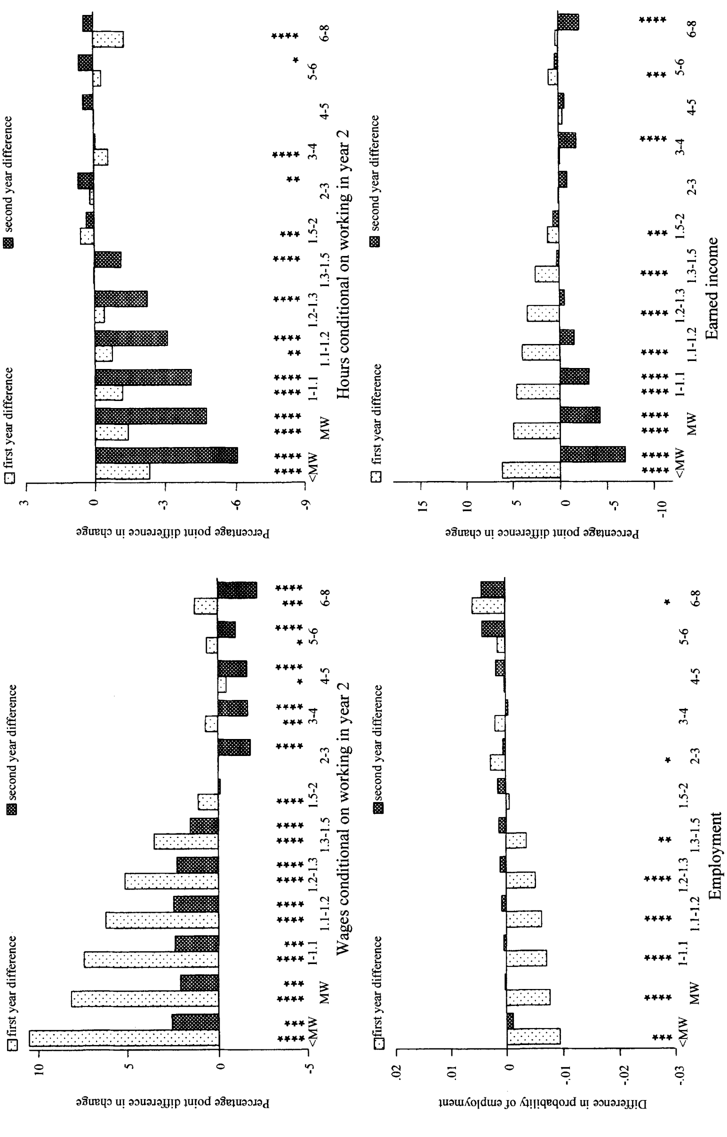

Figure 2

Effects of 10 Percent Minimum Wage Increase, Full Sample, Polynomial Specification

Note: Significance levels for two-sided tests are indicated as follows: 1 percent (4 asterisks); 5 percent (3 asterisks); 10 percent (2 asterisks); 15 percent (1 asterisk).

increase for the lowest-wage workers, although they are somewhat smaller than in Figure 1. Also, while these positive impacts extend out a bit further into the wage distribution, the cross-over point from positive to negative effects is still at about 2–3 times the minimum. Correspondingly, the negative hours effects also extend a bit higher up into the wage distribution, with negative effects one year out evident up to 1.5 times the minimum. Similarly, the negative estimated earnings effects are the same rough magnitude as in Figure 1 for the lower-wage workers, but the negative effects extend a bit higher into the wage distribution.

The only substantive difference is that the polynomial specification provides no evidence of disemployment effects one year after a minimum wage increase. Inspection of Figure 1 suggests that this may owe to the more irregular pattern of employment effects in the unrestricted model. But regardless, the evidence on employment effects is rather weak using either specification.

E. Adults Only

One of the motivations we cited for looking at minimum wage effects conditional on workers' positions in the initial wage distribution was to see whether there is evidence of labor demand reductions for low-skilled workers. Our focus on low-wage workers differs from past work studying specific age groups (for example, teens or young adults) that include many—but not exclusively—low-skilled workers. To assess the extent to which our results identify effects for low-skilled adults, Figure 3 reports estimates using a sample restricted to individuals aged 20 and older.[13] Overall, these estimates are quite similar to those in Figure 2, with evidence of significant positive wage effects in the lower range of the wage distribution, but significant negative hours and income effects for the lowest-wage workers. Thus, the negative consequences of minimum wages are not restricted to teenagers, but appear more generally for low-wage workers. We also looked at separate results for men and women, see Tables 4a and 4b. The findings were similar for the two groups, with a slight hint that the consequences of minimum wages are worse for women.

VI. Conclusions

In this paper, we present evidence on wage, hours, employment, and labor income adjustments that occur in response to minimum wage increases. Our main contribution is to estimate these adjustments in a consistent framework that provides a relatively complete description of the effects of minimum wages at many different points of the wage distribution.

The evidence indicates that low-wage workers are most strongly affected, while higher-wage workers are little affected. Workers who initially earn near the minimum wage experience wage gains. But their hours and employment decline, and the com-

13. Because most of the evidence is quite similar using the polynomial specification, and because the restrictions imposed by that specification are even more useful when we take smaller cuts of the sample, these disaggregated results were estimated using the polynomial specification. Results were qualitatively similar using the unrestricted specification—paralleling the full-sample results.

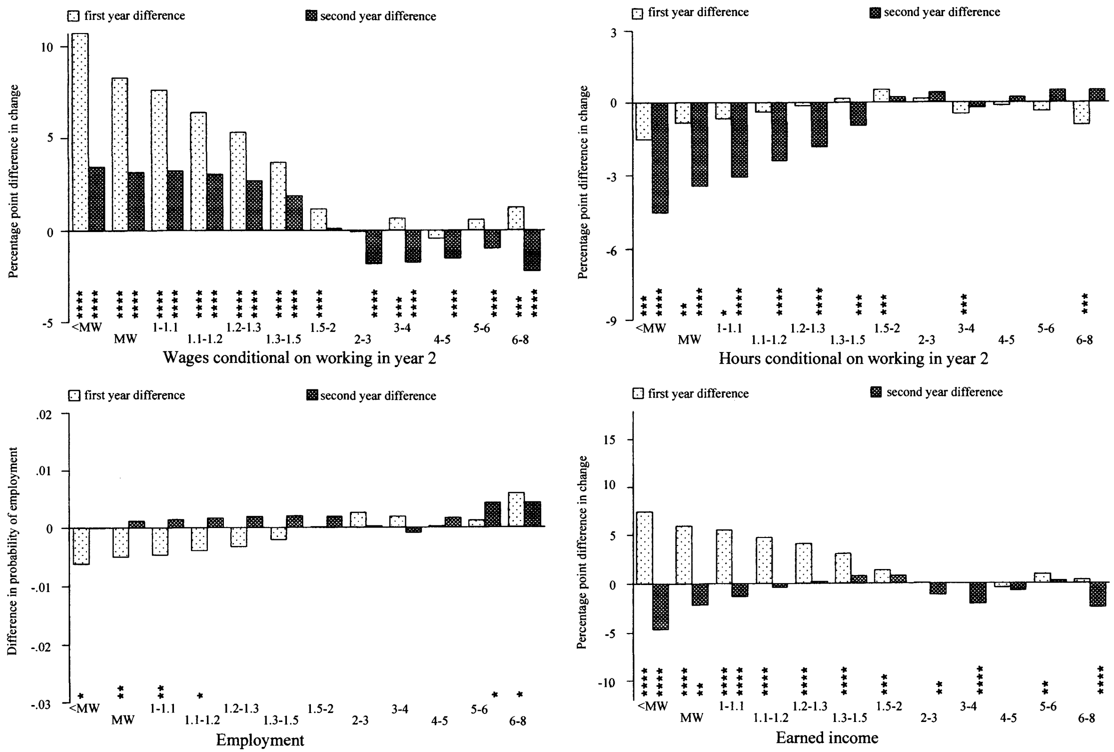

Figure 3

Effects of 10 Percent Minimum Wage Increase, Adults Only, Polynomial Specification

Note: Significance levels for two-sided tests are indicated as follows: 1 percent (4 asterisks); 5 percent (3 asterisks); 10 percent (2 asterisks); 15 percent (1 asterisk).

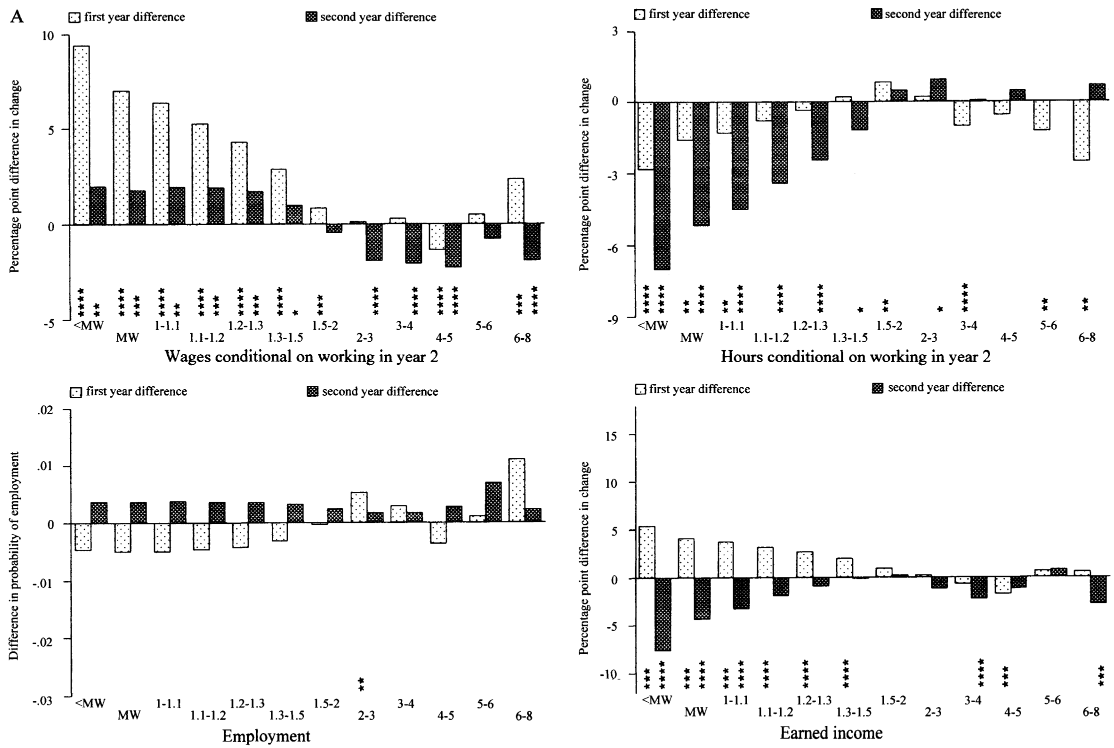

Figure 4A

Effects of 10 Percent Minimum Wage Increase, Women, Polynomial Specification

Note: Significance levels for two-sided tests are indicated as follows: 1 percent (4 asterisks); 5 percent (3 asterisks); 10 percent (2 asterisks); 15 percent (1 asterisk).

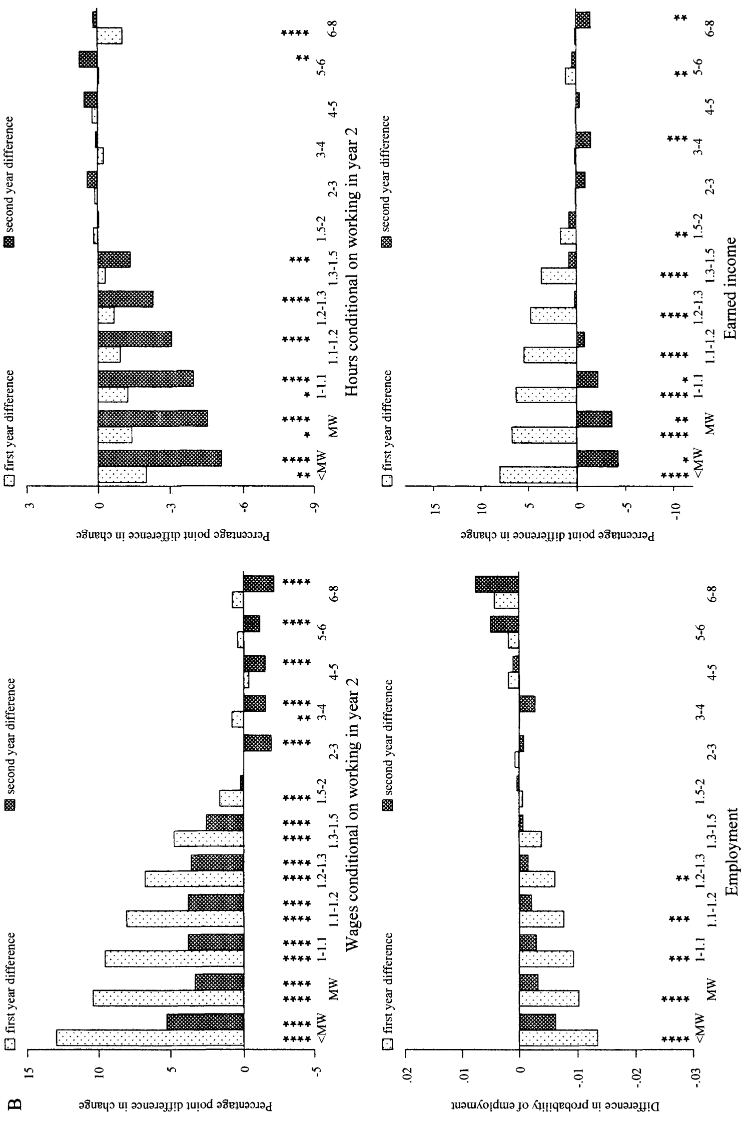

Figure 4B

Effects of 10 Percent Minimum Wage Increase, Men, Polynomial Specification

Note: Significance levels for two-sided tests are indicated as follows: 1 percent (4 asterisks); 5 percent (3 asterisks); 10 percent (2 asterisks); 15 percent (1 asterisk).

bined effect of these changes on earned income suggests net adverse consequences for low-wage workers. The inclusion of lagged minimum wage effects is critical in arriving at the conclusion that low-wage workers are adversely affected, as we find that contemporaneous effects overstate the wage gains and understate the hours and income losses experienced by low-wage workers when minimum wages rise.

The results in this paper are complementary to previous work on the impact of the minimum wage on family incomes (Neumark et al. 1998; Neumark and Wascher, 2002). In particular, our finding that the earned incomes of low-wage workers decline in response to a minimum wage hike is consistent with reduced-form evidence indicating that minimum wages may increase the proportion of families that are poor or near-poor. In this paper we provide some insight into the transmission mechanism of minimum wage effects. A legislated increase in the minimum wage does lead to an immediate boost in the pay of low-wage workers. However, the ultimate sizes of the wage increases induced by the higher minimum typically are considerably smaller than the minimum wage hike itself, and there are hours reductions among employed workers that, coupled with small disemployment effects, generate net losses in earned income. Thus, the findings in this paper indicate that the full range of labor market effects associated with raising the minimum wage most likely reduce the well-being of low-wage workers.

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