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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Tab 1

Authorization

RECEIVED 3-8-19

FLORIDA DEPARTMENT of STATE

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

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 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. 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. On September 30th of 2027 ~~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

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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. 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. On September 30th of 2027 ~~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**

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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.—

(d) An action alleging a willful violation of s. 448.110.

(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;

-v.-

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

Defendant.
_____/

CASE NO. 2011 CA 0072

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 1st day of May 2011.

TERRY P. LEWIS
CIRCUIT JUDGE

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

[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.

[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)*		CPI-U (U.S.)**	
								Index	% Change	Index	% 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%
		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

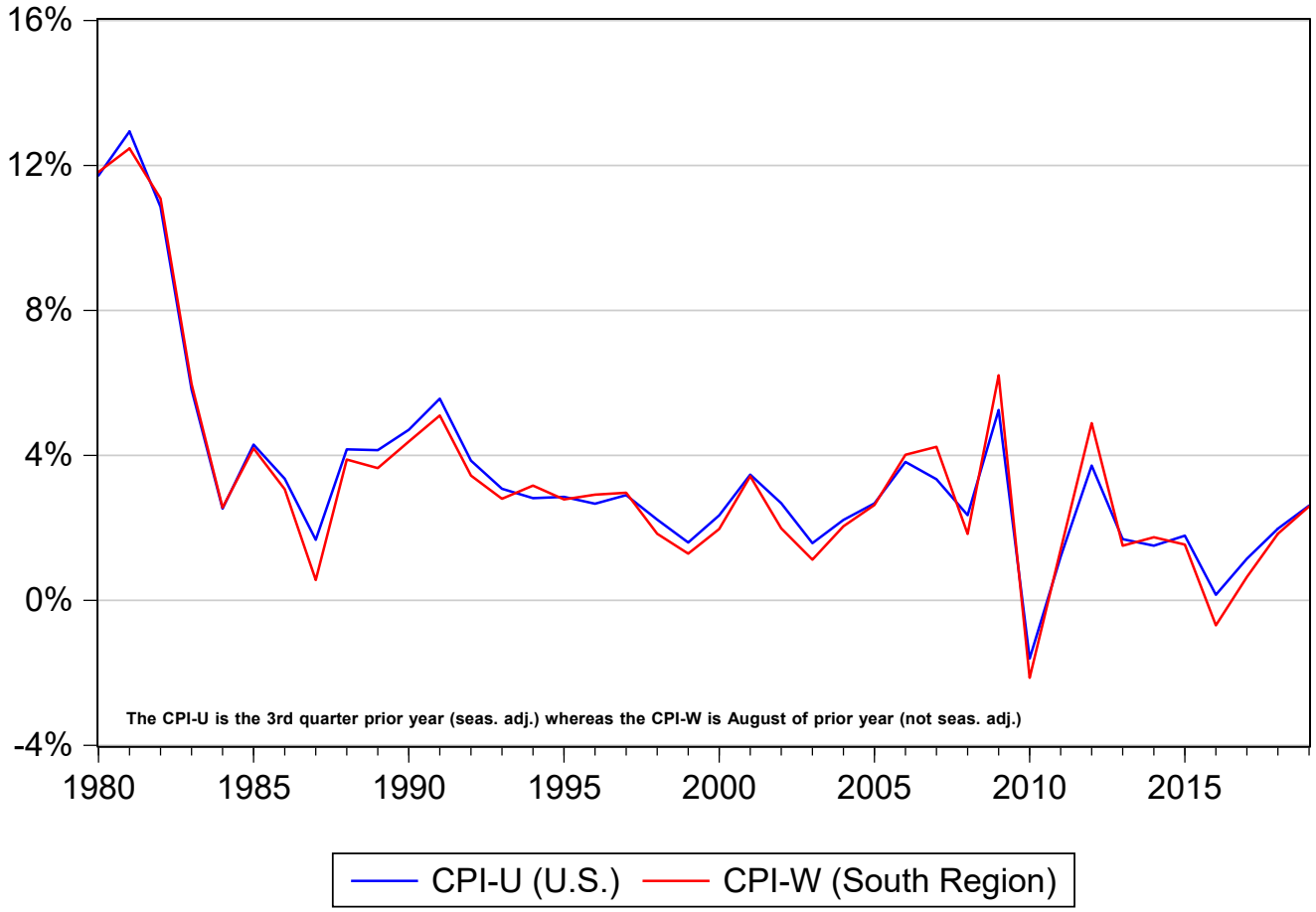
▪ Florida minimum wage held constant, even when CPI-W change over the year is negative

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

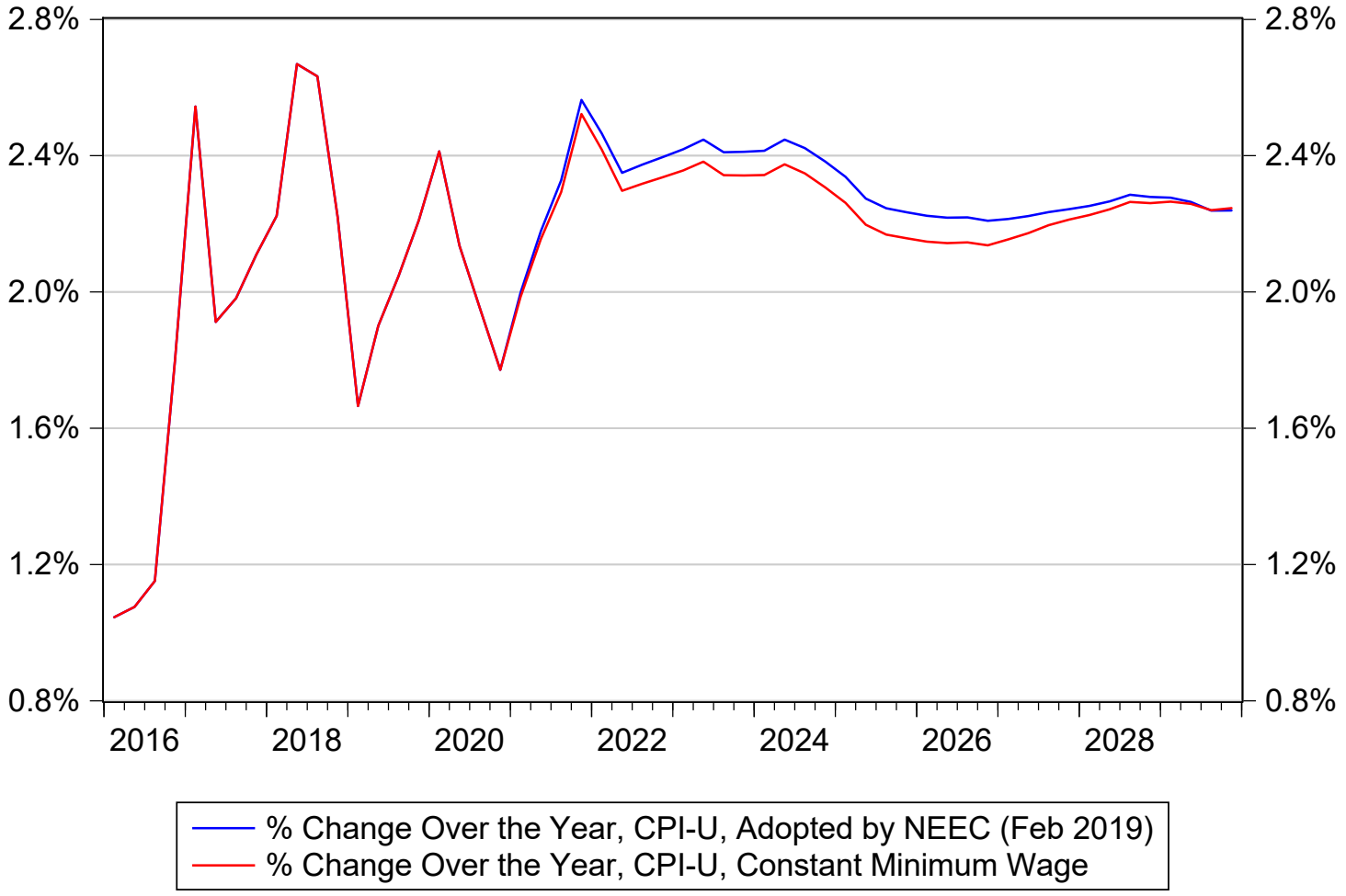
** CPI-All Urban Consumers - All Itemes (seasonally adjusted) - 3rd quarter of prior year. (February 2019 NEEC forecast) Forecast shown in blue shading; History shown in beige shading revised to match US Dept of Labor, Bureau of Labor Statistics, as of 3/27/2019.

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

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

John Schmitt

February 2013

Center for Economic and Policy Research
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Washington, D.C. 20009
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Contents

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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.

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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

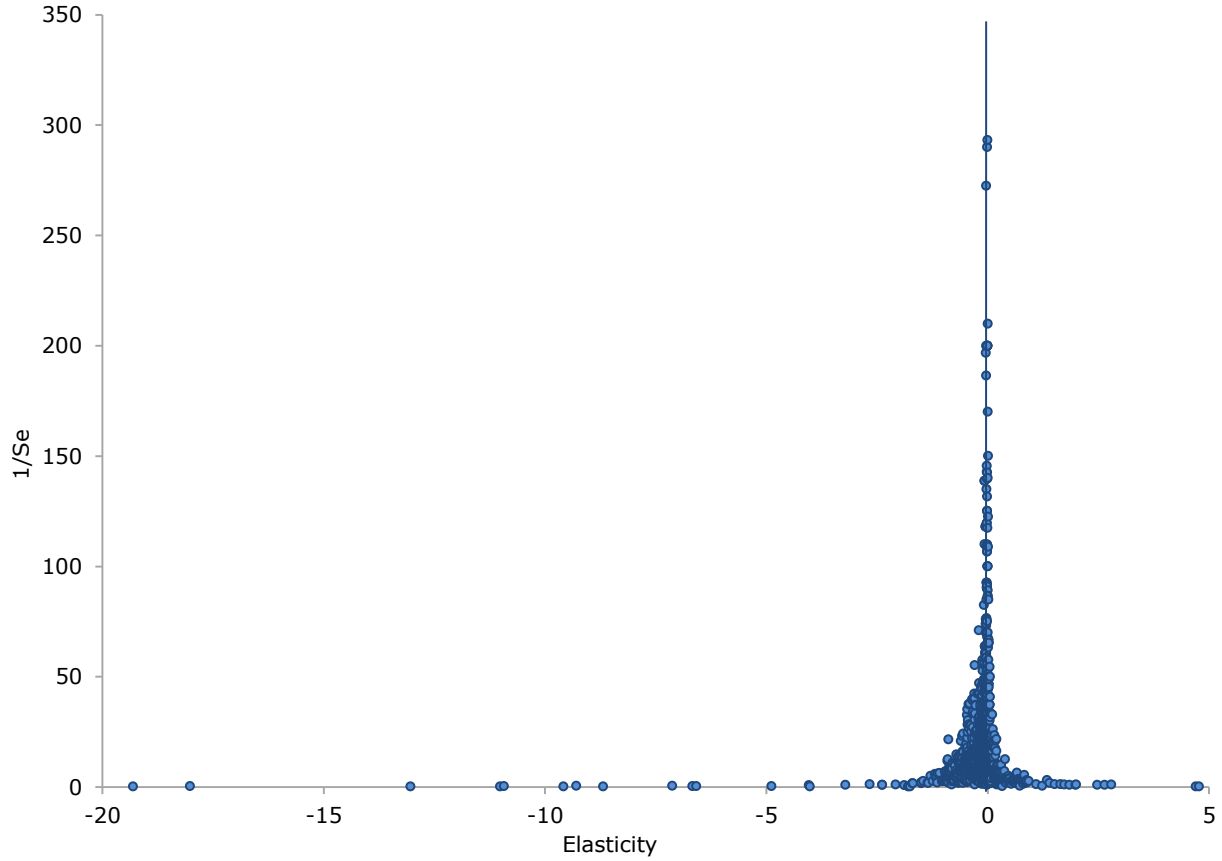
10 Neumark and Wascher (2008), p. 104.

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]

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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.

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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]

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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

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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

		1999	
Bidder	**1997**	Round 1	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.

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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]

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† 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" ($\frac{1}{2}(\partial L/\partial W)(\Delta W)^2$) 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), $\left((\eta/2)(\Delta W/W)^2\right)$, 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:

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

[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 $\left[\left(\overline{\Pi/S}\right)^{T=1}_{NMW=1} - \left(\overline{\Pi/S}\right)^{T=1}_{NMW=0}\right] - \left[\left(\overline{\Pi/S}\right)^{T=0}_{NMW=1} - \left(\overline{\Pi/S}\right)^{T=0}_{NMW=0}\right]$, and the conditional difference-in-difference, ψ_2, from the regression model

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

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.

$$\psi_2 = -\theta\psi_1. \tag{6}$$

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.

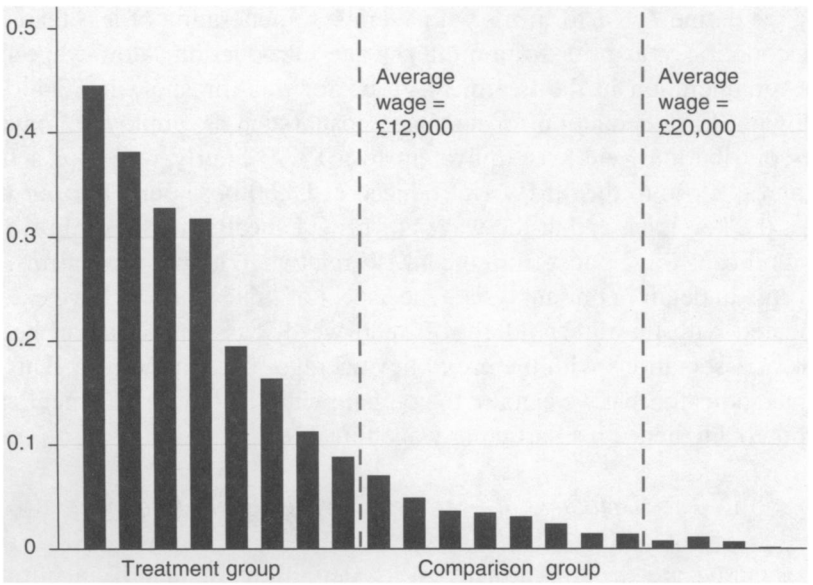

FIGURE 1. VALIDATION OF AVERAGE WAGE DATA
(*Comparison of proportion of low-wage workers and establishment average wages, WERS 1998*)

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

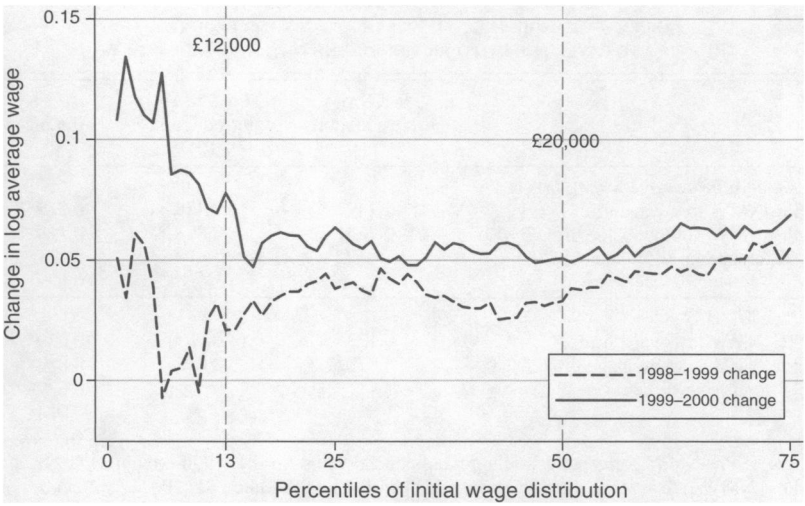

FIGURE 2. CHANGE IN LN(AVERAGE WAGE) BY PERCENTILE IN THE FINANCIAL YEAR BEFORE AND AFTER NMW INTRODUCTION

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.

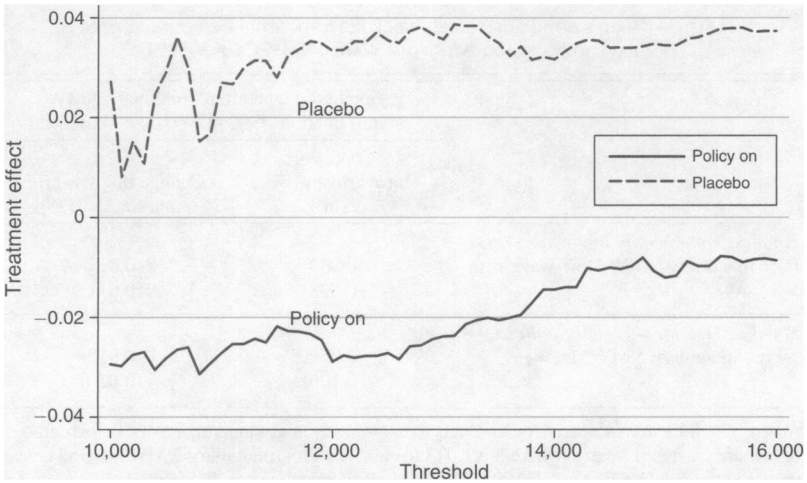

FIGURE 3. VARYING TREATMENT EFFECT COEFFICIENTS IN FAME DIFFERENCE-IN-DIFFERENCE PROFITABILITY MODELS

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†

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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

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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 $\times$ $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:

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

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 × 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

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)	Size of increase[c] Small (4)	Size of increase[c] Large (5)	Real average quarterly spending (6)	Implied marginal propensity to spend using average income[d] (7)
0	−57	67	77	−79	−55	10,938	
	(150)	(252)	(174)	(456)	(150)		
	178,075	73,569	77,790				
>0	499	−154	524	−290	530	7,640	2.1
	(412)	(174)	(369)	(775)	(414)		(2.0)
	22,474	104,506	13,027				
≥0.2	815	−232	885	−60	874	6,462	3.4
	(457)	(175)	(404)	(600)	(461)		(1.9)
	15,834	95,327	9,608				

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

	Probability of purchase (1983–2008)		Expenditure (1983–2008)		Expenditures on new cars and trucks (1992–2008)			
							Financed with loan	
Share of income from minimum wage jobs (S_i)	New cars/ trucks (1)	Used cars/ trucks (2)	New cars/ trucks (3)	Used cars/ trucks (4)	Expenditure (5)	Net outlay, not financed (6)	Down payment (7)	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.

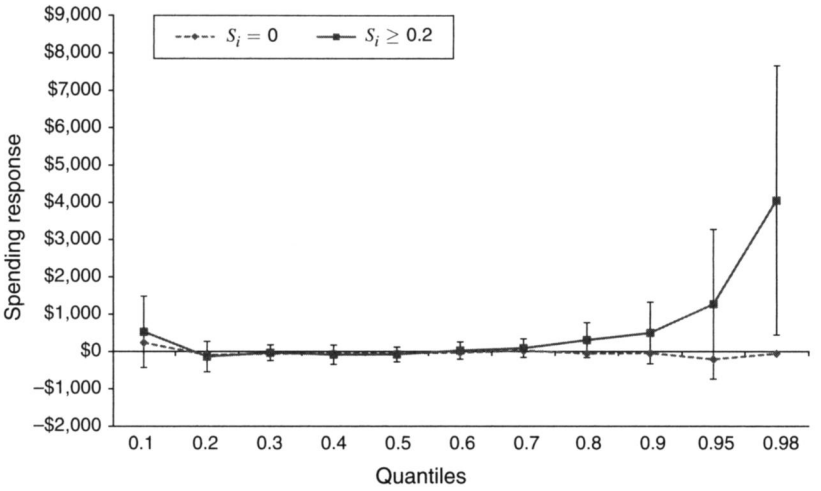

FIGURE 1. SPENDING RESPONSE TO CHANGE IN MINIMUM WAGE, CEX QUANTILE REGRESSIONS

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.

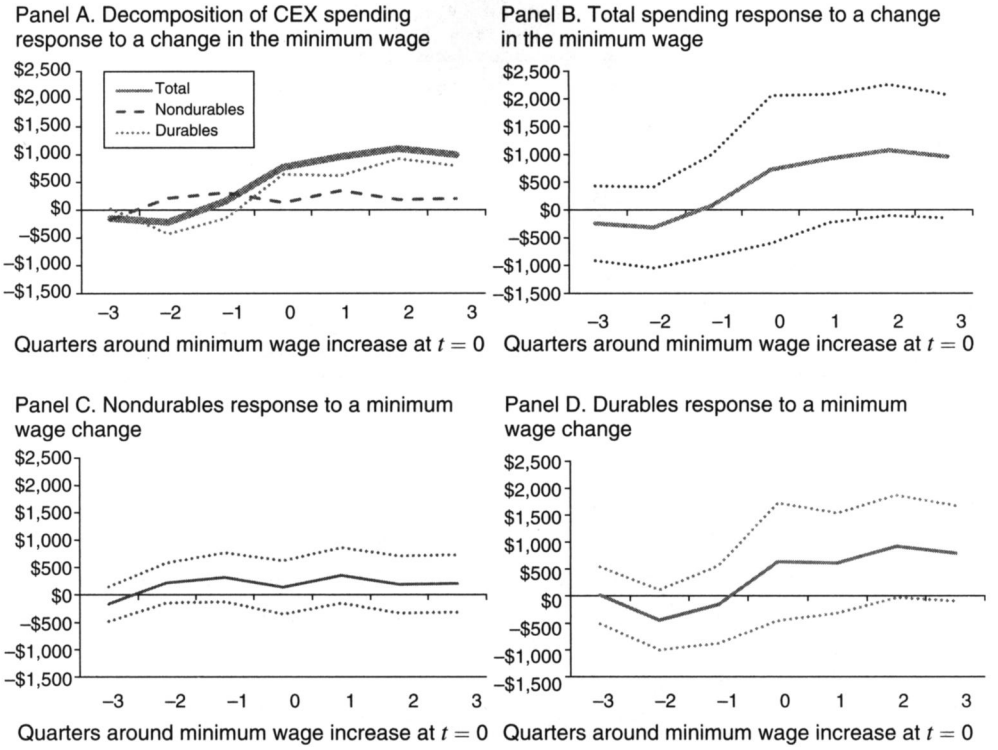

FIGURE 2

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]	Imputed from income/debt[b]	Weighted average	
	(1)	(2)	(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.

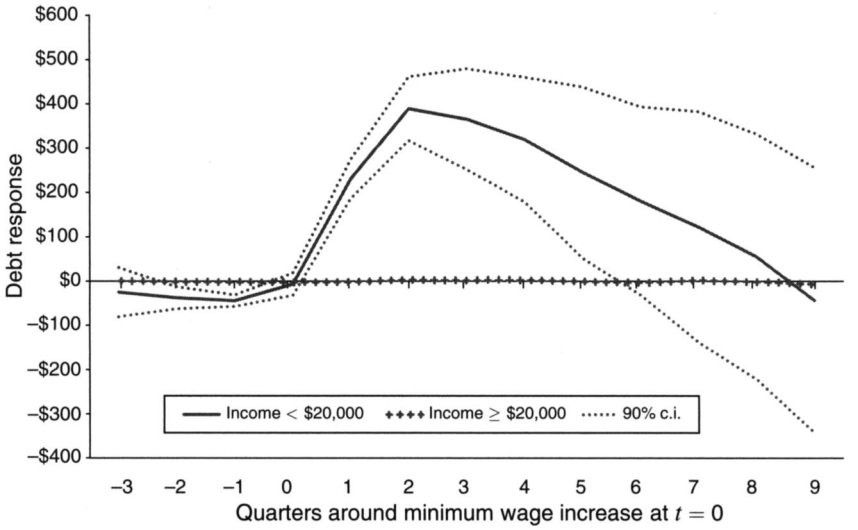

FIGURE 3. DEBT (AUTO, HOME EQUITY, AND CREDIT CARD) RESPONSE TO A CHANGE IN THE MINIMUM WAGE CREDIT CARD/CREDIT BUREAU DATA

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.

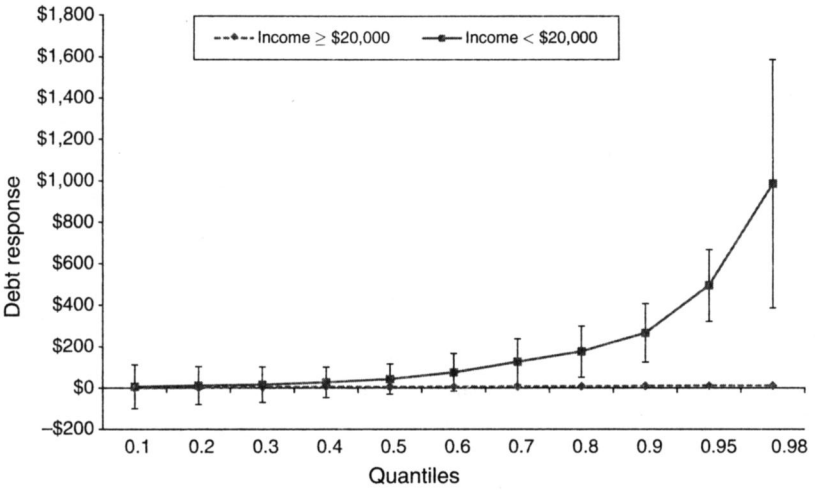

FIGURE 4. DEBT RESPONSE TO CHANGE IN MINIMUM WAGE CREDIT BUREAU QUANTILE REGRESSIONS

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.

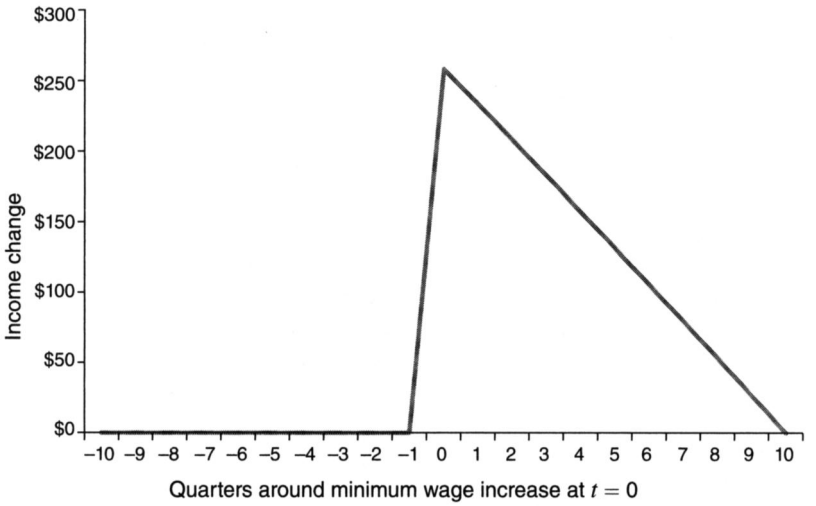

FIGURE 5. SIMULATED INCOME CHANGE AROUND A MINIMUM WAGE INCREASE

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.

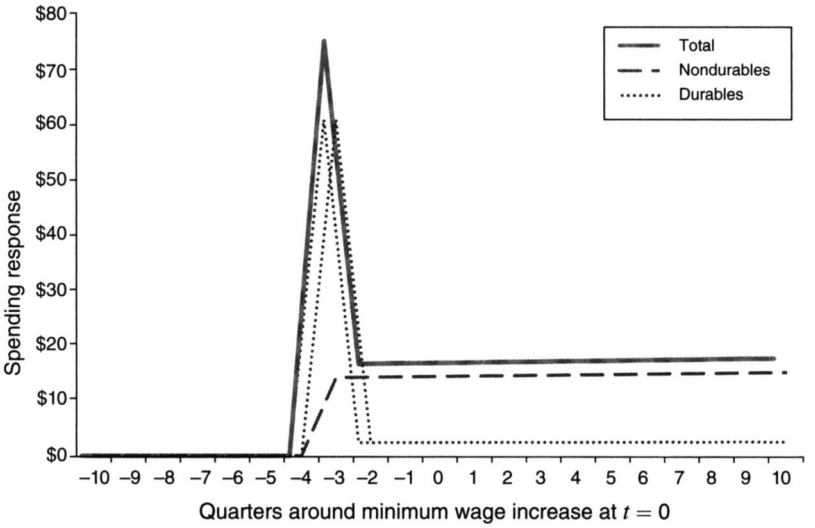

FIGURE 6. SPENDING CHANGE AROUND A MINIMUM WAGE INCREASE
SIMULATION WITHOUT BORROWING CONSTRAINTS

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:

$$\left.\frac{\partial C_0}{\partial A_0}\right|_{D_0} = (1-\theta)\left[\frac{1-\frac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}}{1-\left(\frac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}\right)^{T+1}}\right] = 0.01, \tag{10}$$

$$\left.\frac{\partial I_0}{\partial A_0}\right|_{D_0} = (\beta(1+r))^{\frac{1}{\gamma}}\left(\frac{\theta}{r+\delta}\right)\left[\frac{1-\frac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}}{1-\left(\frac{(\beta(1+r))^{\frac{1}{\gamma}}}{1+r}\right)^{T+1}}\right] = 0.04, \tag{11}$$

[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

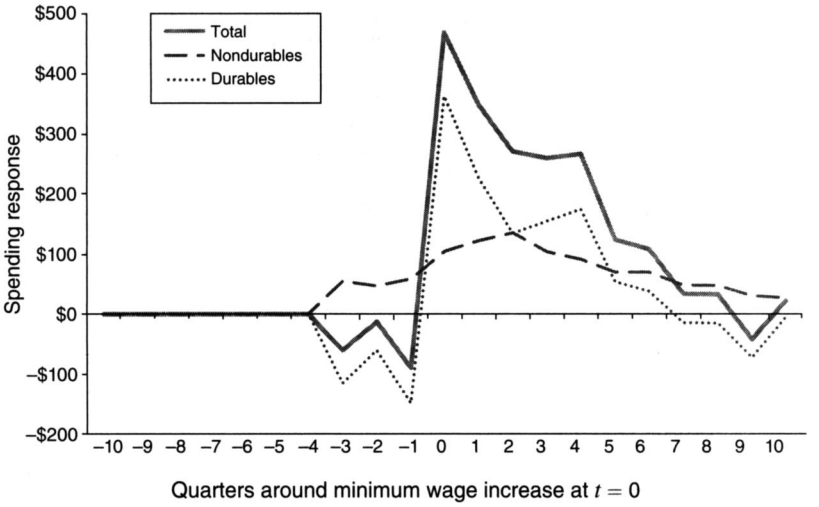

FIGURE 7. SPENDING CHANGE AROUND A MINIMUM WAGE INCREASE SIMULATION WTH BORROWING CONSTRAINTS

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 $-\Delta A_t$.

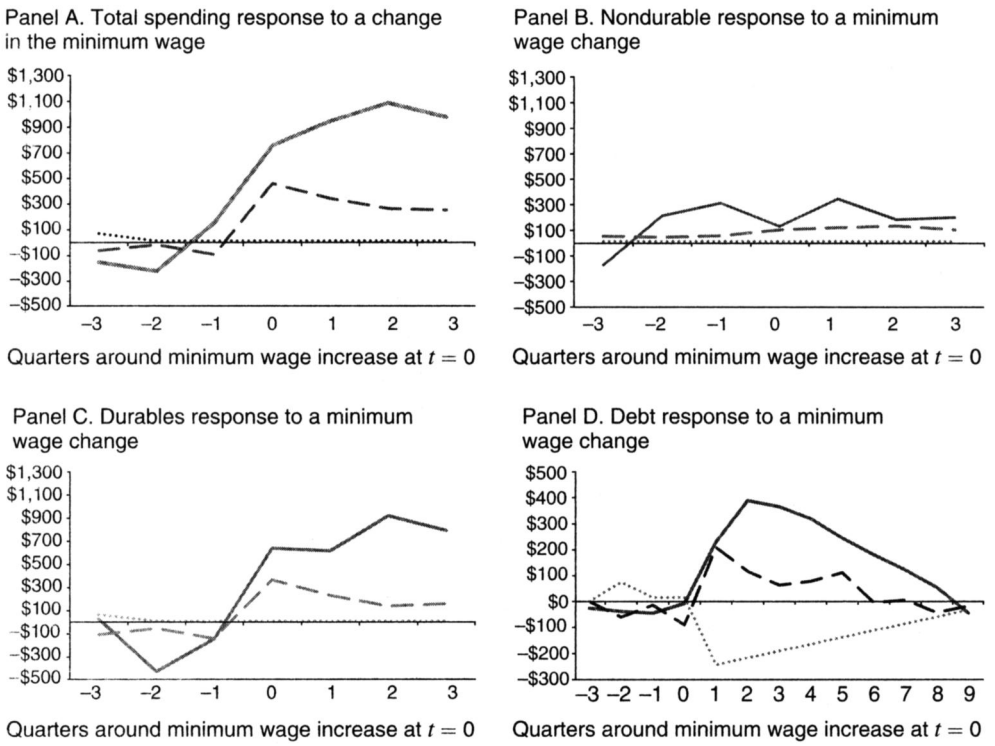

FIGURE 8

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.

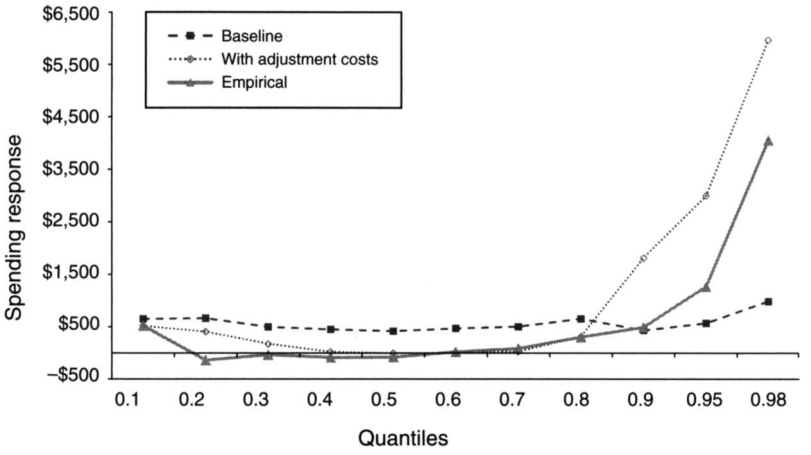

FIGURE 9. MODEL PREDICTED SPENDING RESPONSE TO A CHANGE IN MINIMUM WAGE WITH AND WITHOUT ADJUSTMENT COSTS: QUANTILE REGRESSIONS

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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CWED
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Seattle's Minimum Wage Experience 2015-16

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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.

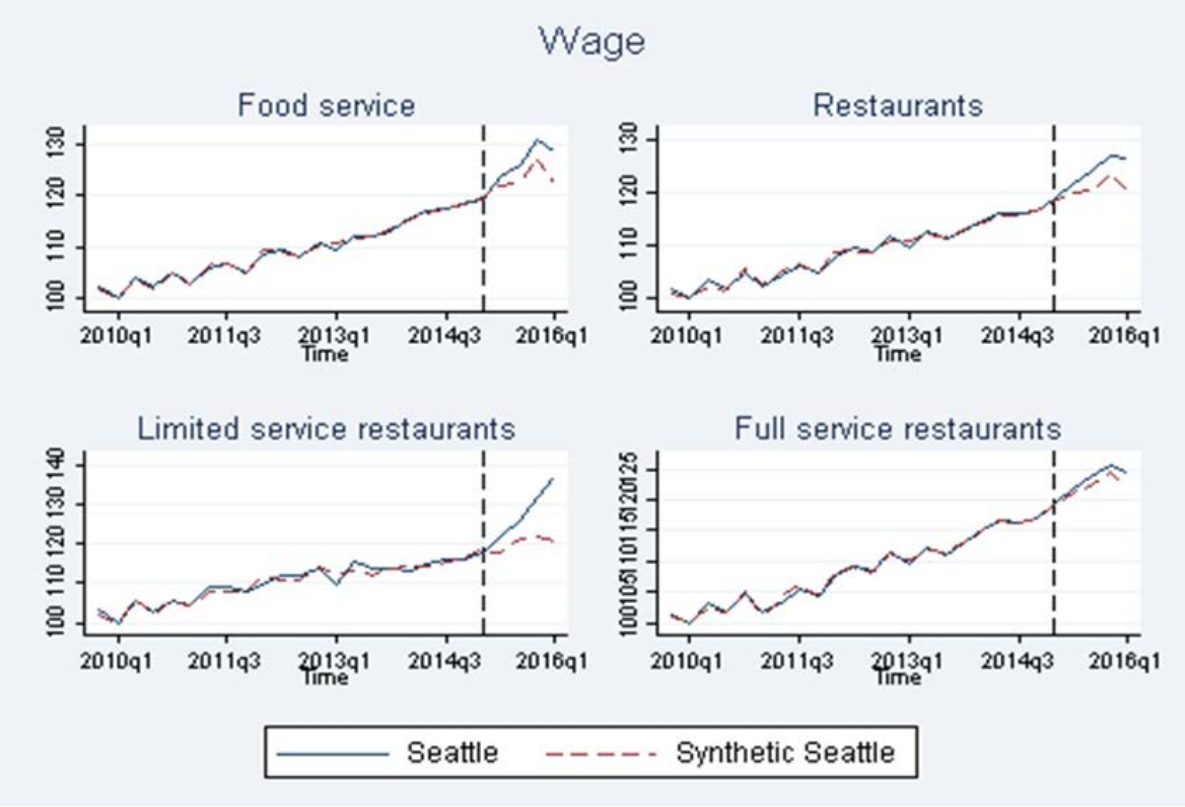
As the figure shows, wages in Synthetic Seattle track wages in Seattle remarkably well, and over the entire pre-treatment period.[7] This finding indicates that our application of the synthetic control method strongly passes the parallel trends requirement. These results thereby satisfy the first of 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

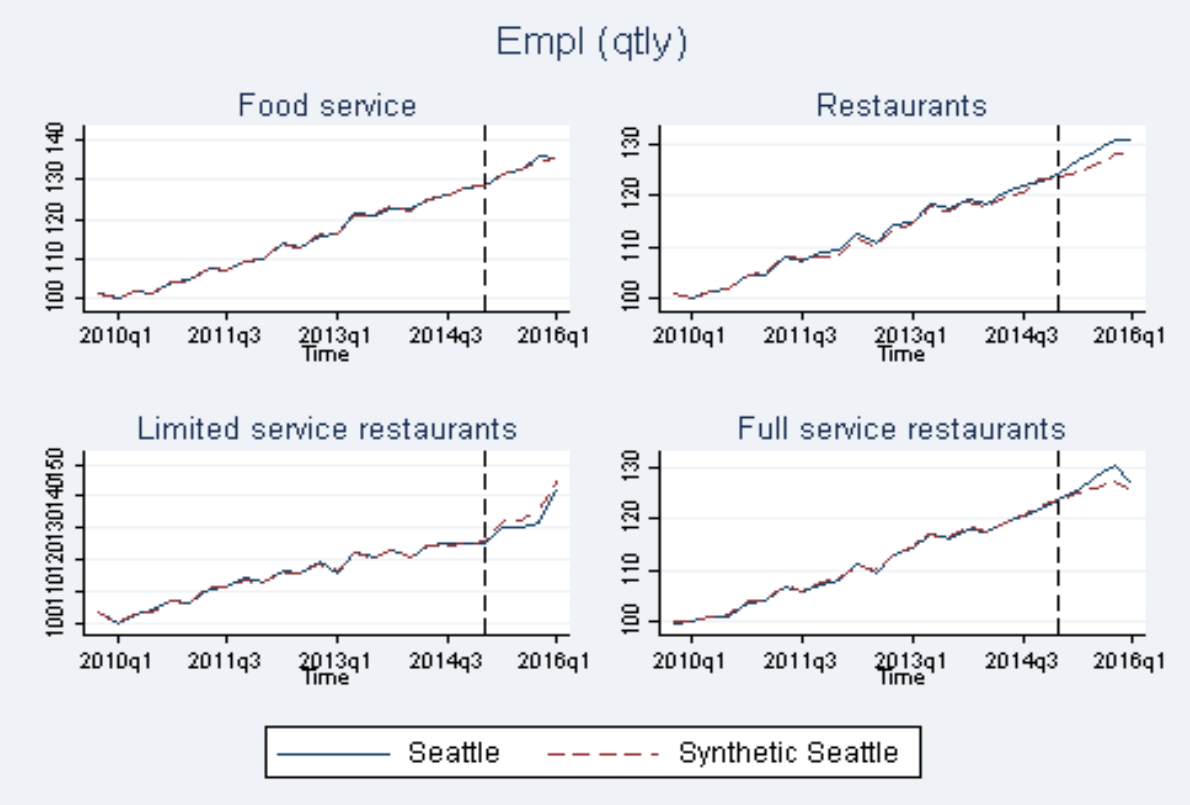


Notes: City-level QCEW data for Seattle. County-level QCEW data for the donors that make up Synthetic Seattle. See Appendix B for a list of donors. The vertical dashed line refers to April 1, 2015, the implementation date of the first phase. The second increase occurred on January 1, 2016.

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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Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence from Seattle
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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.

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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]

[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:

$$\Delta Y_{rt} = Y_{rt} / Y_{r,t-4} - 1 \quad (1)$$

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:

$$\Delta Y_{rt} = \alpha_r + \psi_t + \sum_{q=1}^{9} \beta_q T_{rt} + \varepsilon_{rt}, \quad (2)$$

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:

$$\Delta Y_{rt} = \sum_{k=1}^{K} \lambda_{rk}\mu_{tk} + \sum_{q=1}^{9} \beta_q T_{rt} + \varepsilon_{rt}, \quad (3)$$

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:

$$\min_{w_r} \sum_{t=-33}^{0} \left(\Delta Y_{r=1,t} - \sum_{r=2}^{R} w_r \Delta Y_{rt}\right)^2, \quad (4)$$

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:

$$\beta_q^{Synth} = \Delta Y_{r=1,q} - \sum_{r=2}^{R} \widehat{w}_r\, \Delta Y_{rq}\ . \quad (5)$$

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

[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

Quarter	Quarters After Passage/ Enforcement	Number of Jobs: Hourly wage rates: Under $13	Number of Jobs: Hourly wage rates: Under $19	Number of Jobs: Hourly wage rates: All	Total Hours (thousands): Hourly wage rates: Under $13	Total Hours (thousands): Hourly wage rates: Under $19	Total Hours (thousands): Hourly wage rates: All	Average Wage: Hourly wage rates: Under $13	Average Wage: Hourly wage rates: Under $19	Average Wage: Hourly wage rates: All	Total Payroll ($mlns.): Hourly wage rates: Under $13	Total Payroll ($mlns.): Hourly wage rates: Under $19	Total Payroll ($mlns.): Hourly wage rates: 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		Difference-in-Differences between Seattle and: 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 $p \leq 0.01$, 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		Jobs	
		SC	IFE	SC	IFE
2014.3	1	0.008 (0.018)	0.004 (0.013)	0.004 (0.017)	-0.006 (0.015)
2014.4	2	0.003 (0.018)	-0.001 (0.013)	-0.010 (0.021)	-0.023 (0.015)
2015.1	3	-0.023 (0.018)	-0.018 (0.013)	0.000 (0.023)	-0.013 (0.015)
2015.2	4/1	-0.013 (0.019)	-0.014 (0.014)	-0.014 (0.019)	-0.032** (0.015)
2015.3	5/2	-0.034 (0.025)	-0.022 (0.020)	-0.019 (0.021)	-0.035* (0.021)
2015.4	6/3	-0.021 (0.033)	-0.009 (0.019)	-0.045 (0.029)	-0.048*** (0.020)
2016.1	7/4	-0.106*** (0.031)	-0.090*** (0.024)	-0.051* (0.028)	-0.053*** (0.021)
2016.2	8/5	-0.087*** (0.031)	-0.079*** (0.027)	-0.052* (0.028)	-0.083*** (0.020)
2016.3	9/6	-0.102*** (0.042)	-0.100*** (0.034)	-0.063* (0.036)	-0.106*** (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 $p \leq 0.01$, 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

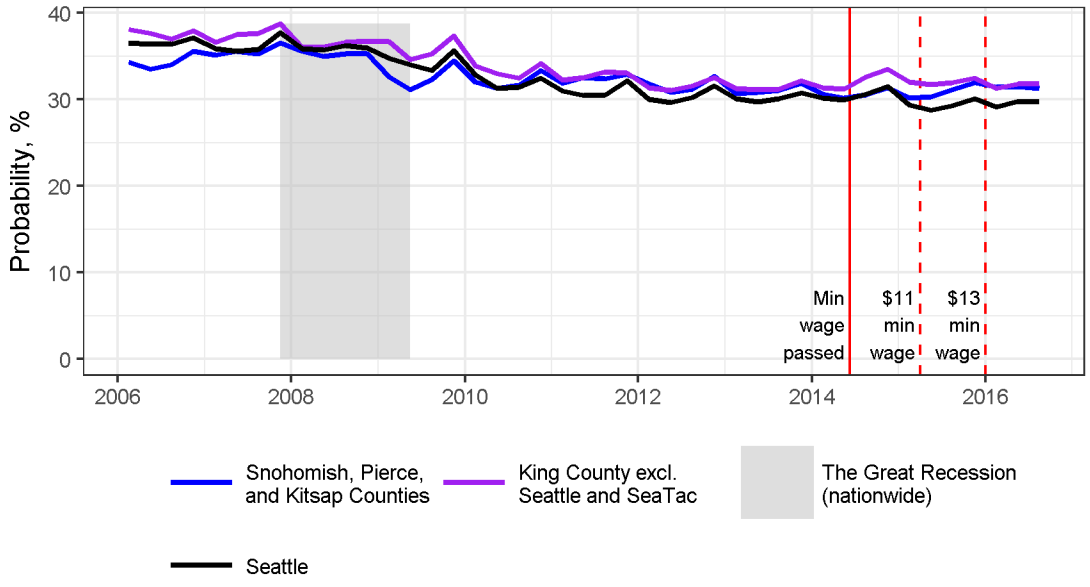
		All industries	Restaurant Industry (NAICS 722)			
	Quarter since Passage/	Wages under $19	Wages under $19		All wage levels	
Quarter	Enforcement	Hours	Hours	Jobs	Jobs	Hours
2014.3	1	0.008	-0.008	0.039	0.038	-0.008
		(0.018)	(0.030)	(0.030)	(0.029)	(0.029)
2014.4	2	0.003	-0.008	-0.006	0.035	0.009
		(0.018)	(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.

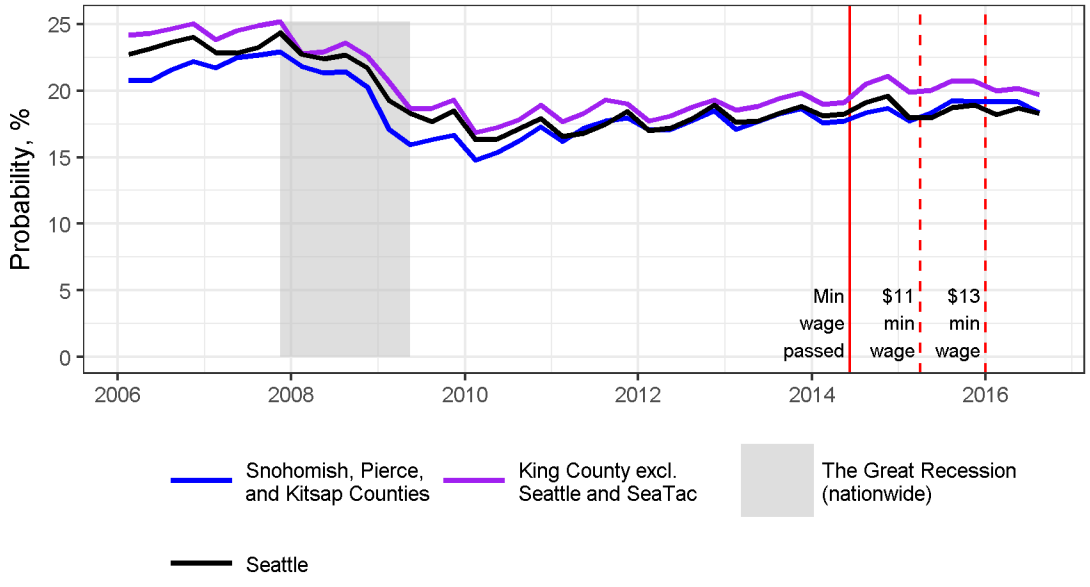
***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

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

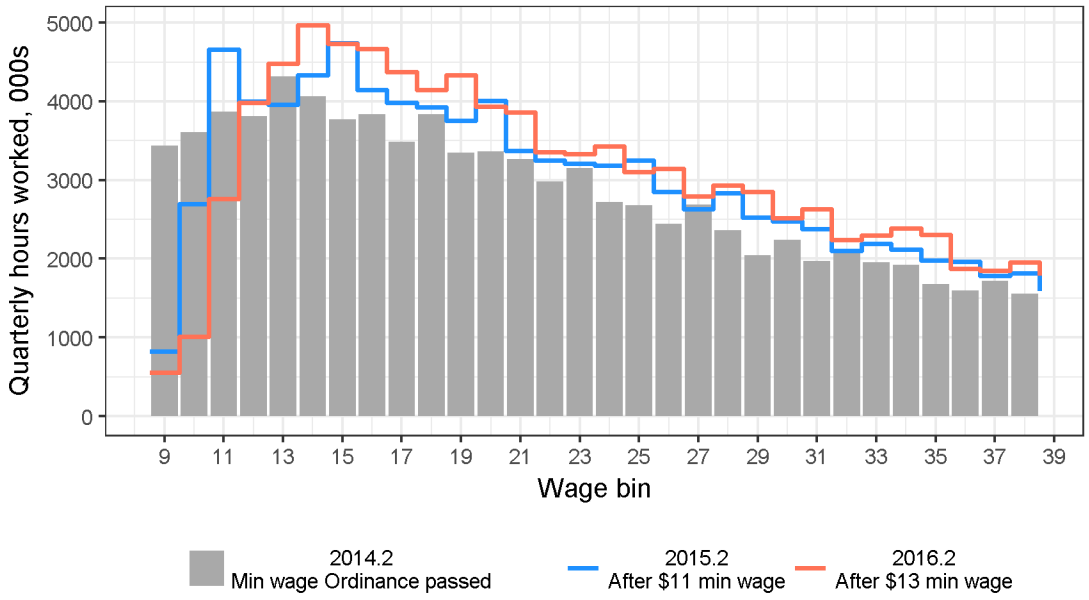


Pane B. *P*(non-locatable job in t | locatable and paid under $19/hour in t-4) by initial location



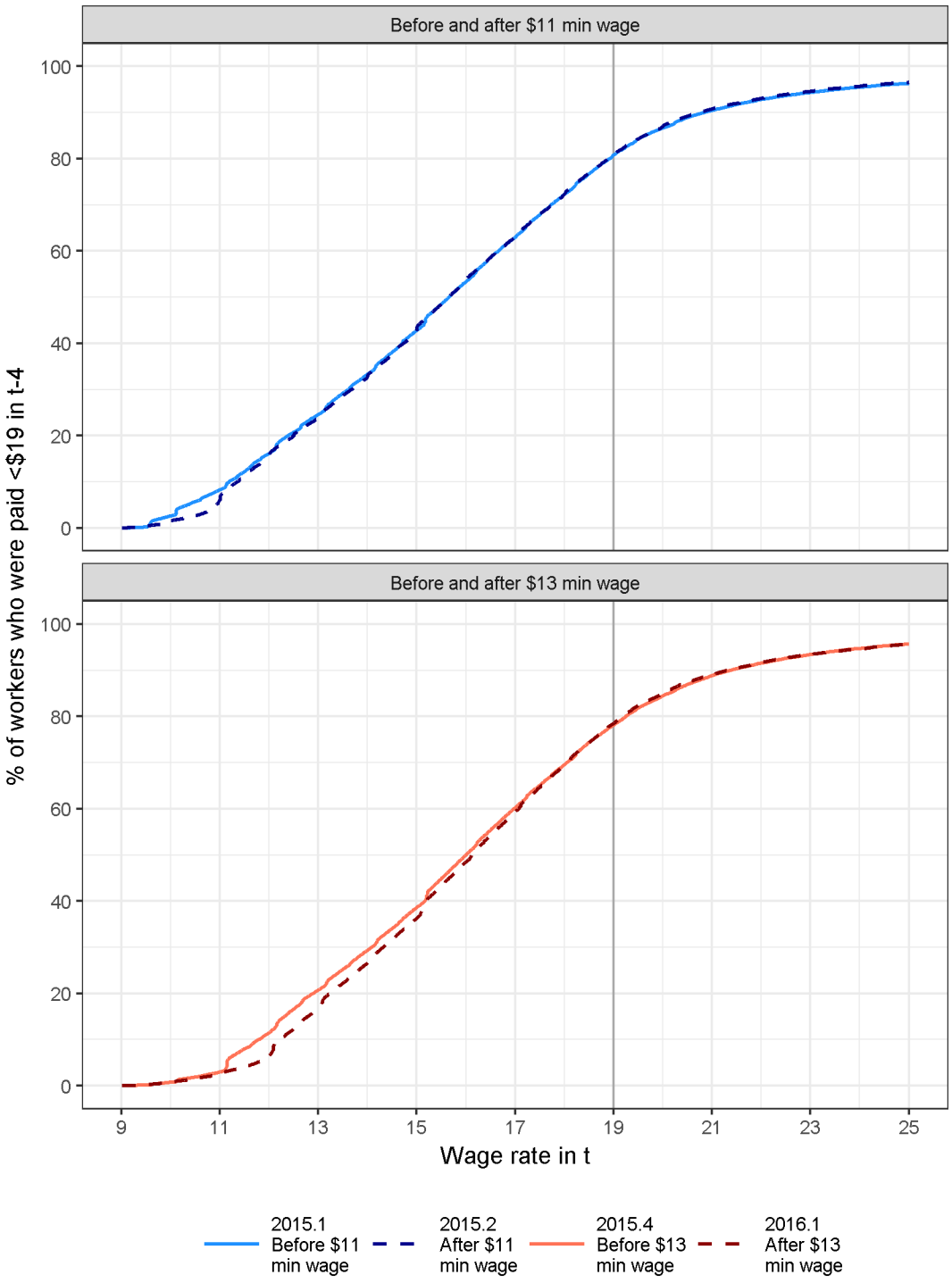
Notes: Non-locatable jobs are defined as those in a non-locatable business anywhere in Washington State. Hourly wages are inflation-adjusted to the 2nd quarter of 2015 using CPI-W.

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



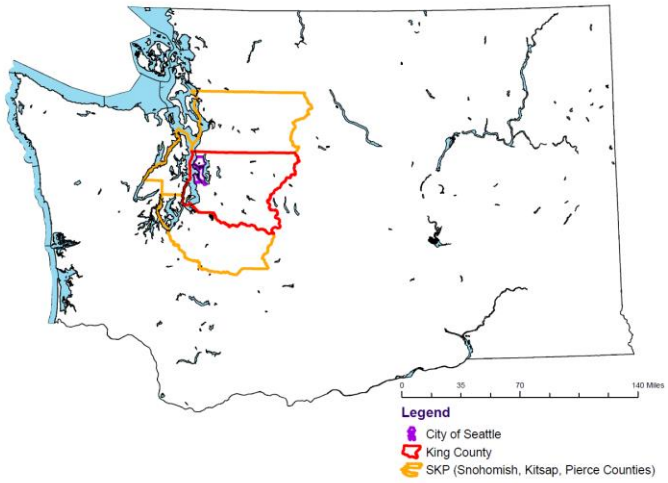
Notes: Workers who were employed in Seattle by locatable establishments in periods t and t-4, and paid less than $19 in t-4.

Figure 4: Geography of Seattle and King, Snohomish, Kitsap, and Pierce Counties

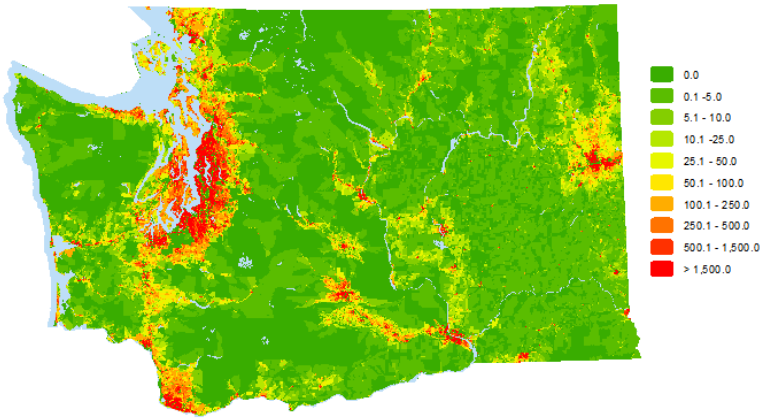
Panel A: Seattle's Water Boundaries

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

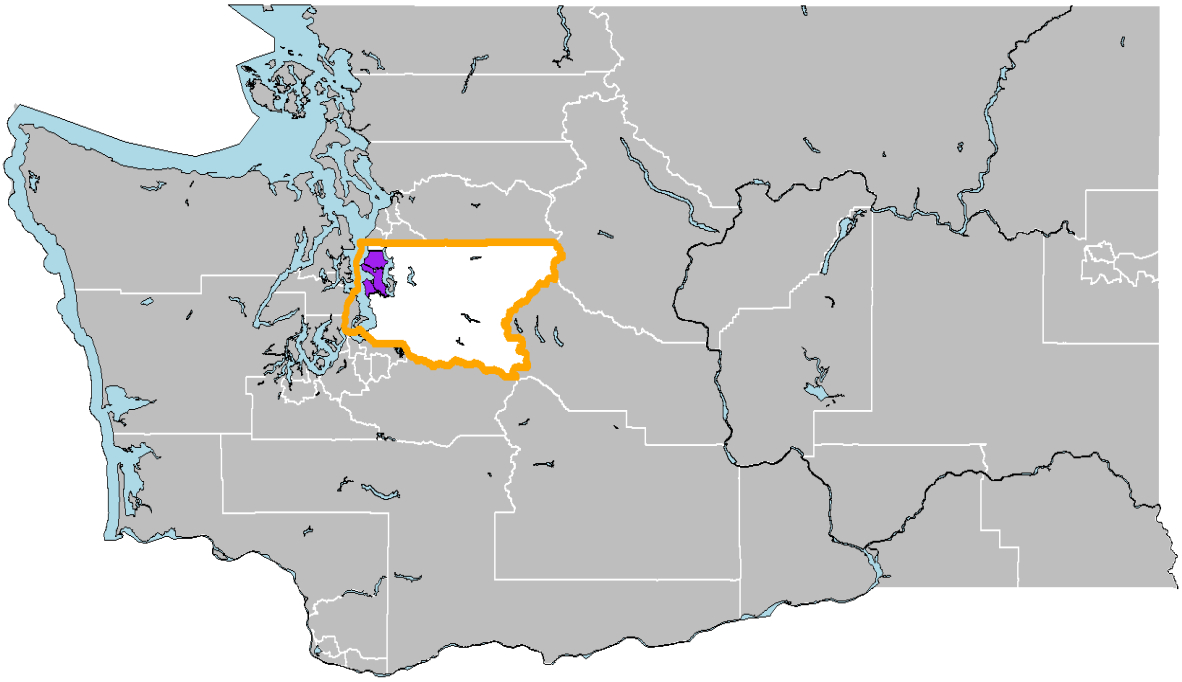
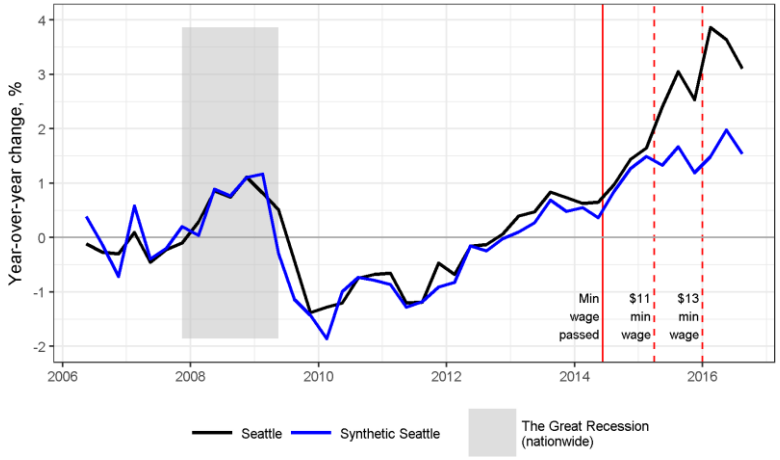
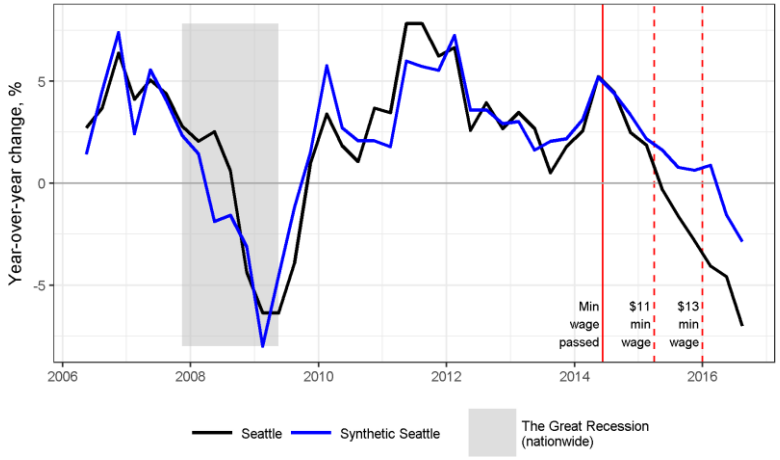


Figure 6: Employment, Wages, and Payroll in Seattle Compared to Synthetic Seattle in Jobs Paying Less than $19 Per Hour

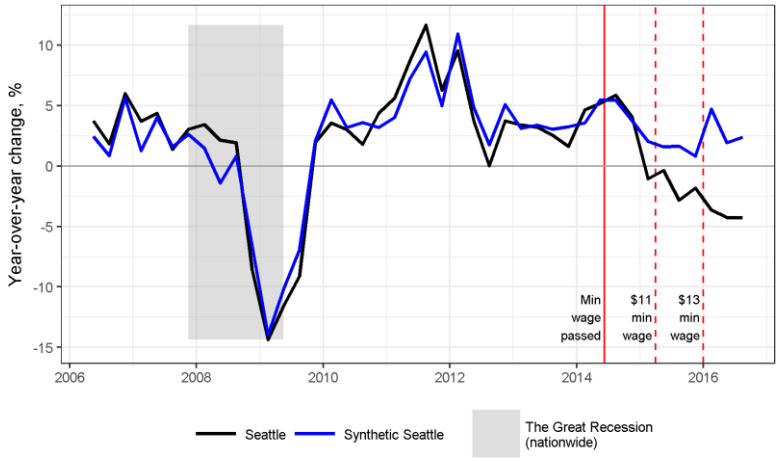
Panel A: Average Wage



Panel C: Number of Jobs



Panel B: Hours Worked



Panel D: Payroll

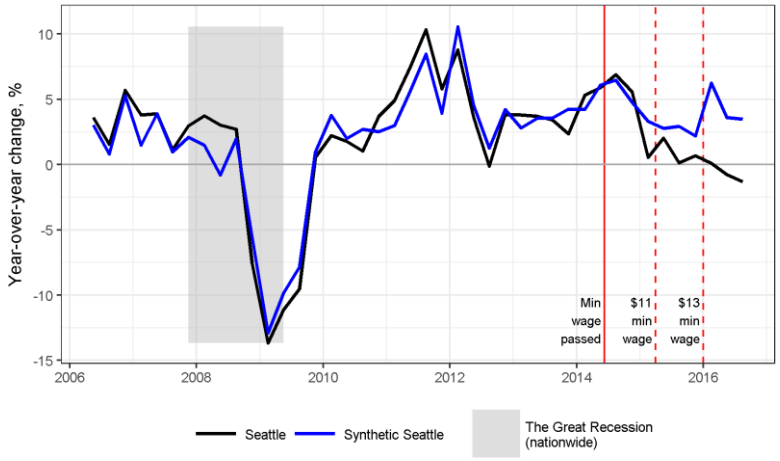
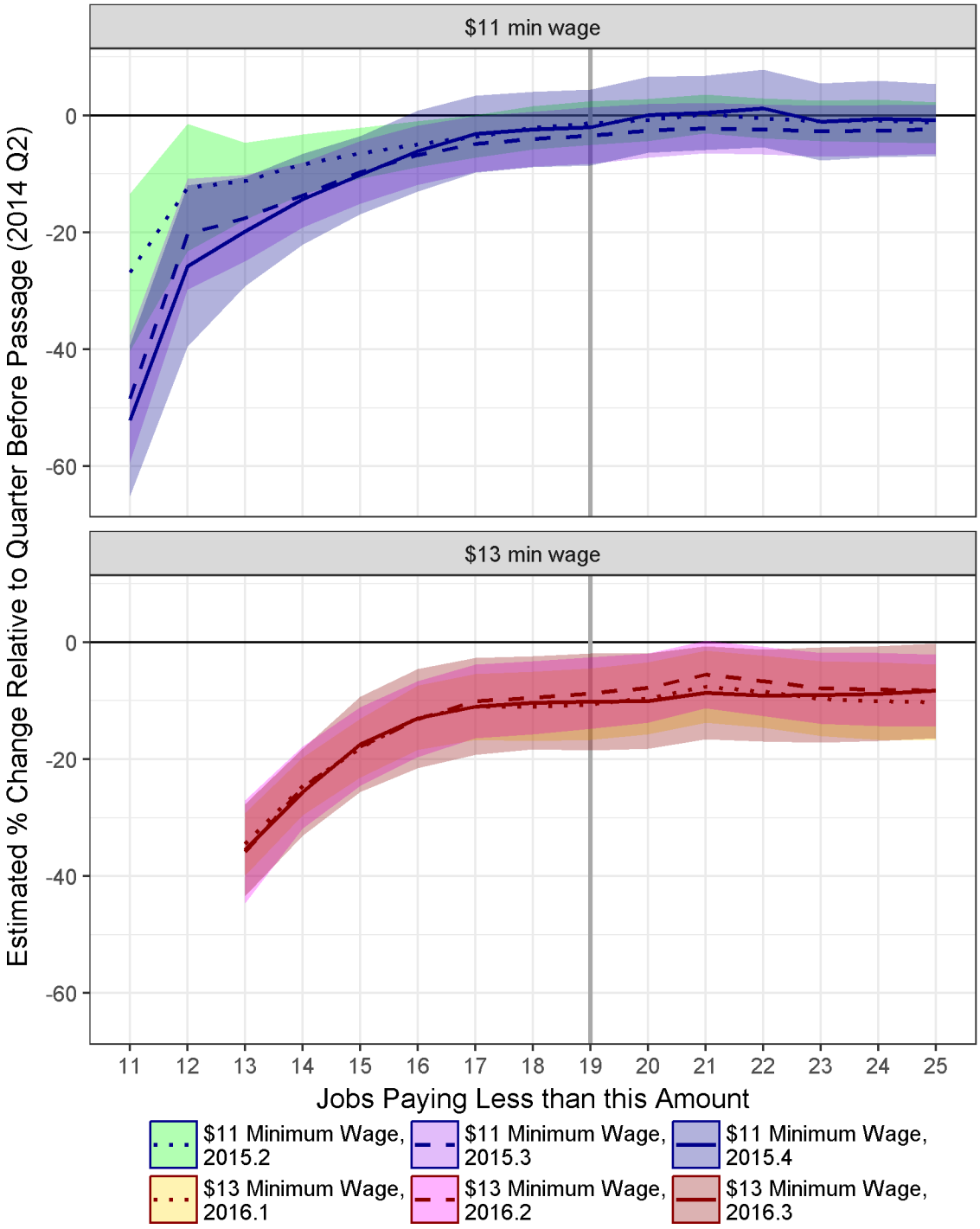
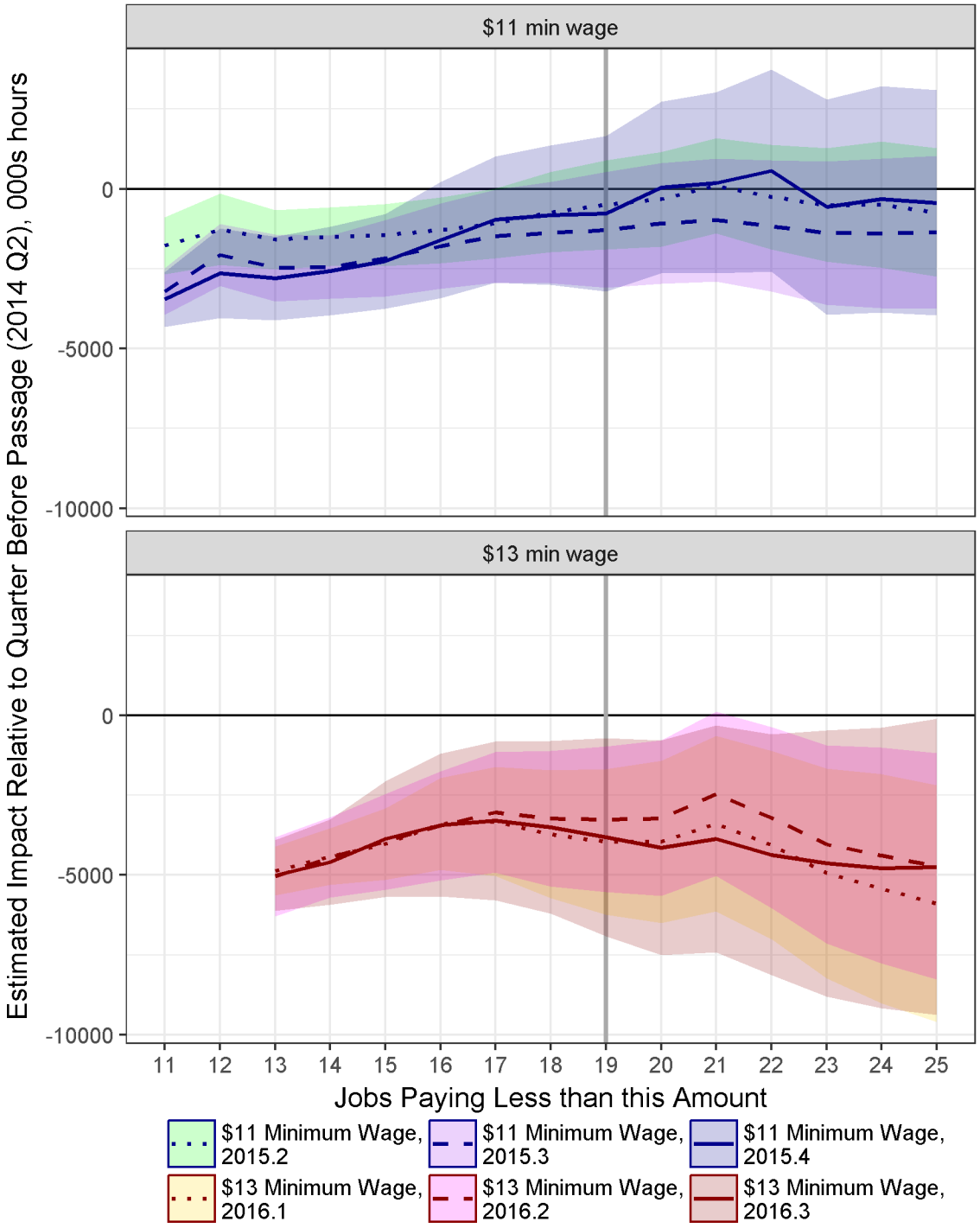


Figure 7: Sensitivity of the Estimated Effects on Percentage Change in Hours Worked Using Different Thresholds



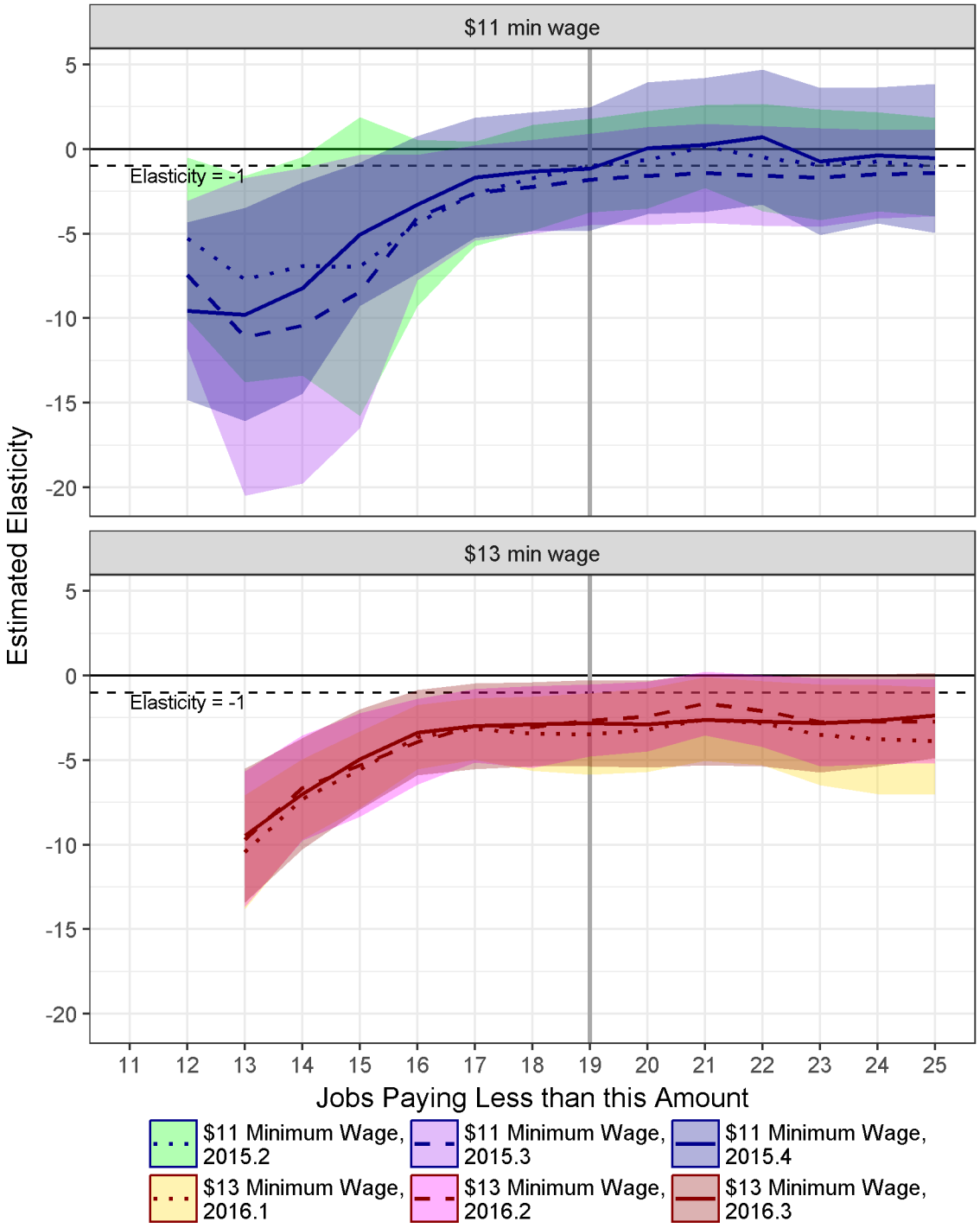
Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

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.

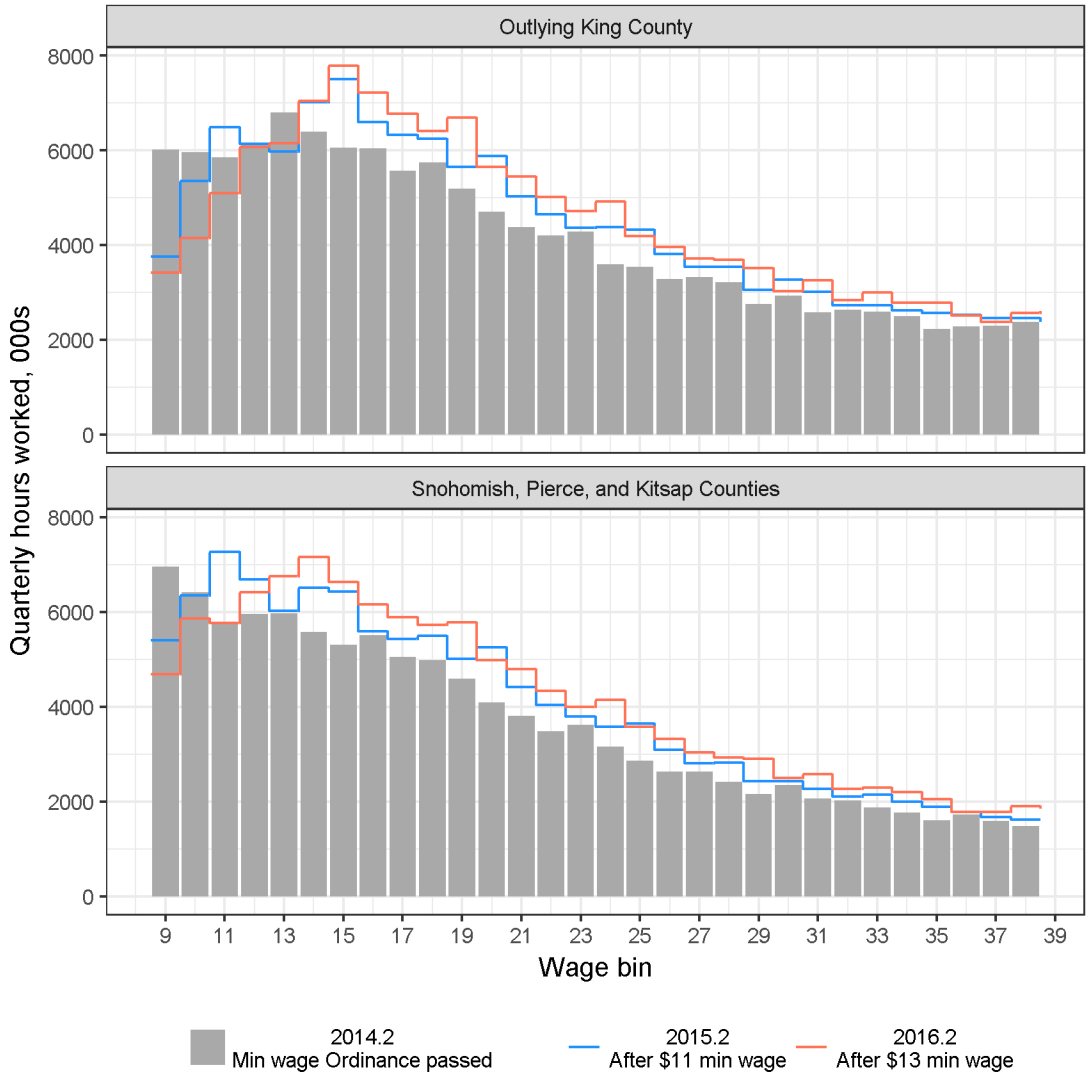
Appendix Table 2: Number of Jobs in Seattle's Locatable Establishments, by Wage Level

		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

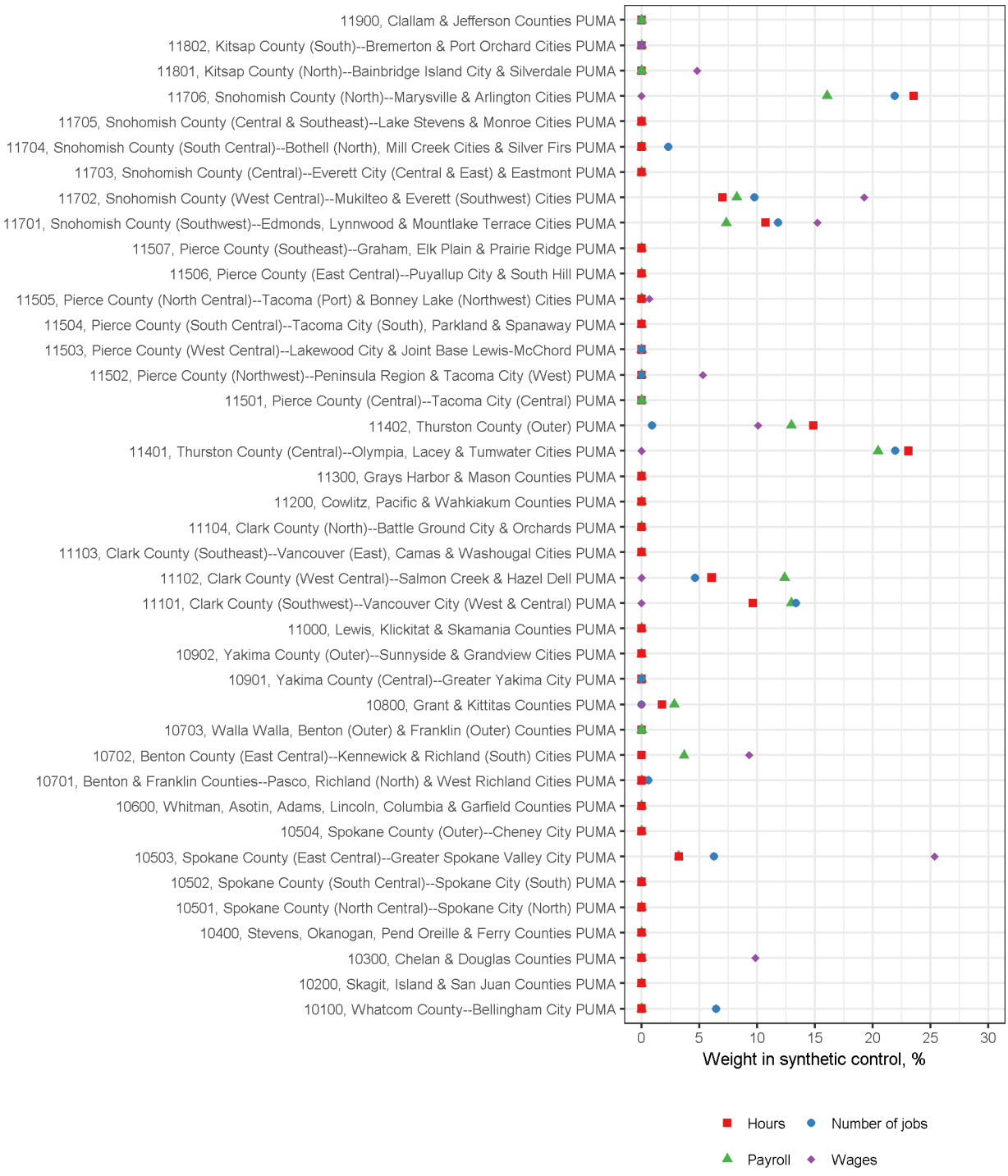
Level of Government	Industry and Outcome	Years	Method	Elasticity
State	Restaurant Employment All Jobs	1990-2010	Interactive FE	-0.04
			Common Correlated Effects-Pooled Estimator	-0.01
			Common Correlated Effects-Mean Group Estimator	-0.01
State	Restaurant Employment All Jobs	2000-2011	DnD (State and 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.



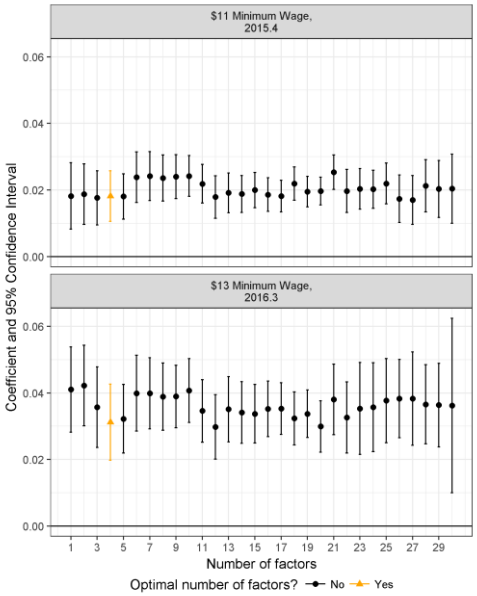
Notes: Authors calculations based on UI records from State of WA using the sample of jobs in locatable employers. Wage rates and earnings are expressed in constant prices of 2015 Q2.

Appendix Figure 2: Weights Chosen by Synthetic Control Estimator, by Outcome.

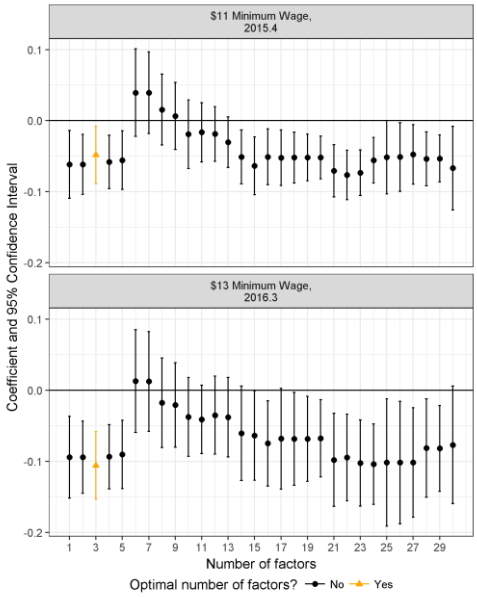


Appendix Figure 3: Sensitivity of the Interactive Fixed Effects Estimates to the Number of Factors Used

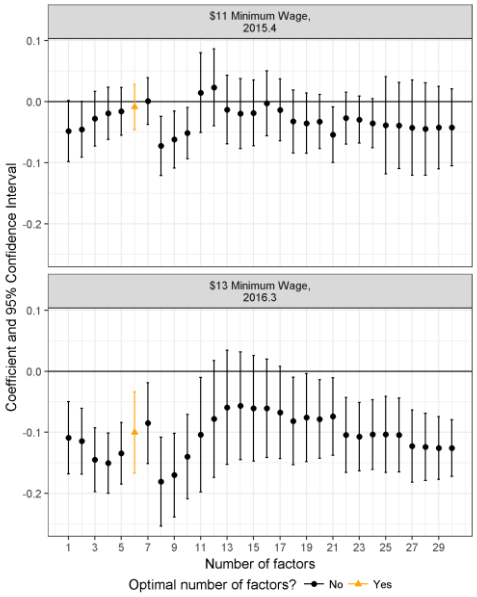
Panel A: Average Wage, Jobs paying <$19 per hour



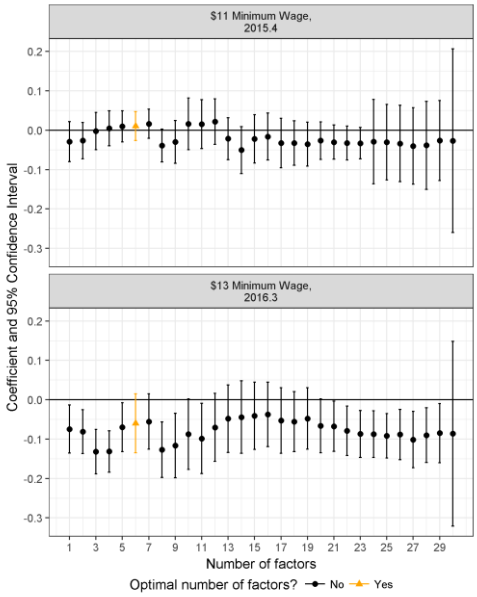
Panel C: Number of Jobs, Jobs paying <$19 per hour



Panel B: Hours Worked, Jobs paying <$19 per hour

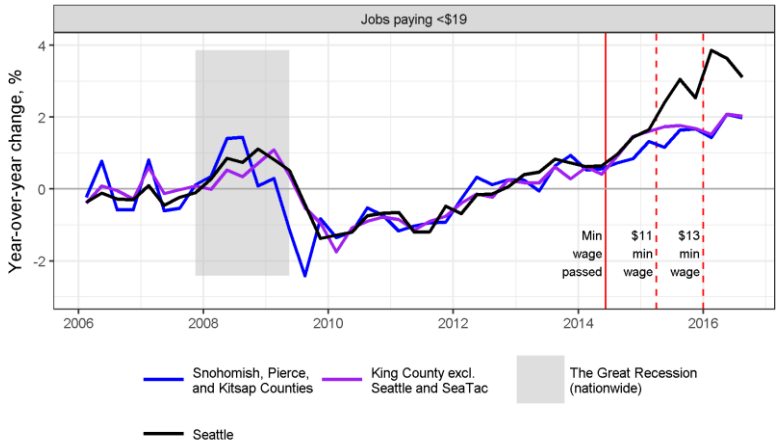


Panel D: Payroll, Jobs paying <$19 per hour

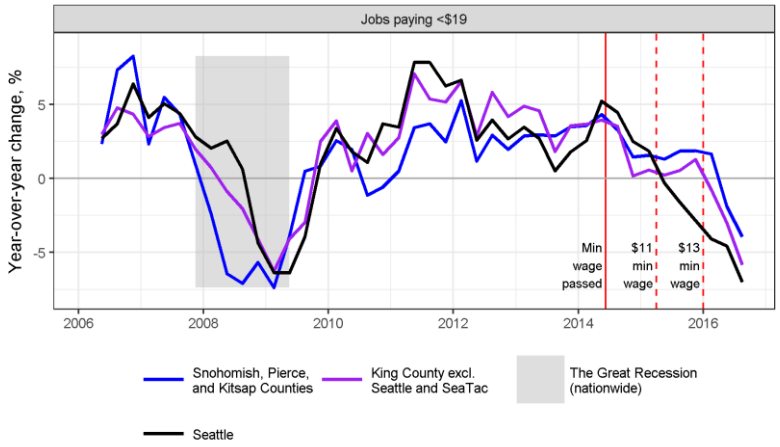


Appendix Figure 4: Employment, Wages, and Payroll in Seattle Compared to Outlying King County and Snohomish, Kitsap, and Pierce Counties

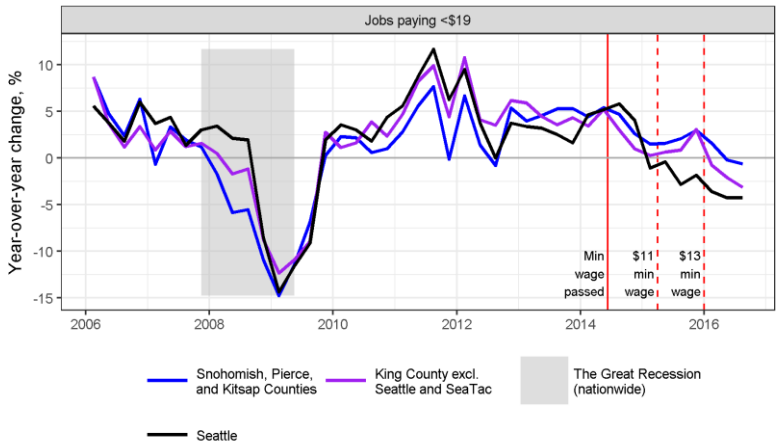
Panel A: Average Wage



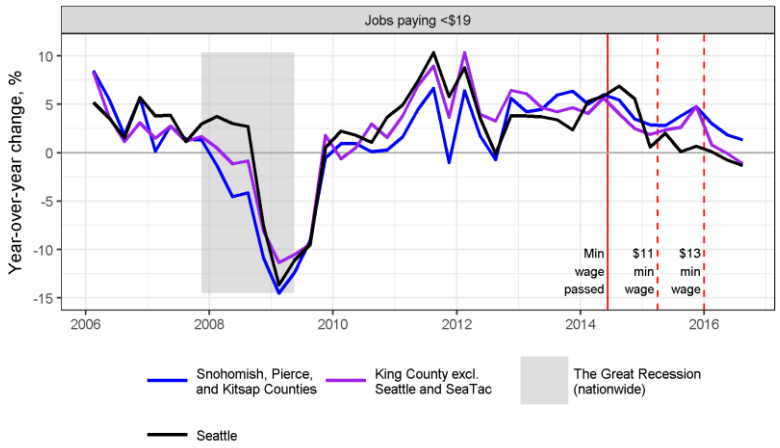
Panel C: Number of Jobs



Panel B: Hours Worked



Panel D: Payroll



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.