

REPORT OF THE WORKING GROUP ON MINIMUM WAGE AND
PAID SICK LEAVE



April 30, 2018

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I. Background and Report Structure

To assist the Village Board and the Wilmette community in determining whether Wilmette should accept Cook County ordinances on minimum wage and paid sick leave, the Working Group was charged with identifying the information that should be considered in making these determinations.

The Working Group first met on January 8, 2018, and after seven meetings agreed on the recommendations in this report. The recommendations are organized in the following sections:

- I. Summary of the Ordinances
- II. How to Weigh Different Types of Data
- III. Questions about Minimum Wage
- IV. Questions about Paid Sick Leave
- V. List of Appendices

The Working Group recommends that Trustees and members of the Wilmette community read the Report in sequence, as interpreting the information presented for each of the questions requires familiarity with the ordinances, types of data, studies, and other information.

The appendices themselves are provided in a separate document, which can be referenced by Trustees and members of the community who are interested in the studies themselves or other information deemed relevant by the Working Group.

II. SUMMARY OF THE COOK COUNTY ORDINANCES

In collaboration with Village Staff, the Working Group approved the summary (Table below and in Appendix 1) of basic facts of each ordinance. We encourage Trustees and the public to familiarize themselves with these facts before considering the data in subsequent sections.

	<u>Minimum Wage</u>	<u>Mandatory Paid Sick Leave</u>
Definition of Employer	Any entity with any place of business in Cook County, or licensed by Cook County, with at least 4 employees one of which is a "Covered Employee" or just 1 "Domestic Worker"* *Based on Cook County's Administrative Rules	Any entity with any place of business in Cook County with 1 or more "Covered Employees"
Definition of Covered Employee	Works 2 hours in any 2 week period	Works 2 hours in any 2 week period
Jurisdiction	Employees who perform work in covered portions of Cook County, regardless of the location of their business office, may be entitled to the County Minimum Wage only for that work	Employees who perform work in covered portions of Cook County, regardless of the location of their business office, may be entitled to the County Sick Leave.
Applies to persons under 18	No	Yes
Applies to seasonal/temporary employees	No, for up to the first 90 days of employment (yes on the 91 st calendar day of employment)	No, as a practical matter, if the employer restricts accrual of sick leave benefit time for the first 180 days of employment. After the first 180 days, the employee is eligible for sick leave benefits
Applies to Independent Contractors	No* *Per the County's administrative rules whether someone is an employee or independent contractor depends on the application of a multi-factor, fact intensive legal test.	No* *Per the County's administrative rules whether someone is an employee or independent contractor depends on the application of a multi-factor, fact intensive legal test.
Gov't Exemptions	All Units of Government other than Cook County	All Units of Government including Cook County

	<u>Minimum Wage</u>	<u>Mandatory Paid Sick Leave</u>
Other Exemptions	Employees of religious organizations, certain persons with disabilities for which the employer has received authorization from the State of Illinois, individuals in a Subsidized Transitional Program, individuals in a Subsidized Temporary Youth Employment Program	None
Collective Bargaining Agreements	Does not apply to employees covered under Collective Bargaining Agreements ("CBA") entered into or on before July 1, 2017 and for those CBAs that have waived the ordinance requirements.	Does not apply to employees covered under Collective Bargaining Agreements ("CBA") entered into or on before July 1, 2017 and for those CBAs that have waived the ordinance requirements.
Current Illinois Law	Non – Tipped Employees: \$8.25/hr. Tipped Employees: Employer must pay a base wage of \$4.95/hr. and make up any shortfall in tips that would result in the employee making less than \$8.25/hr.	No paid sick leave benefits
Benefits provided by Cook County Ordinances	Non – Tipped Employees: July 1, 2017 \$10.00/hr. July 1, 2018 \$11.00/hr. July 1, 2019 \$12.00/hr. July 1, 2020 \$13.00/hr. July 1, 2021 and future: \$13/hr., annual CPI adjustment	Accrual Rate – 1 hour sick leave per 40 hours of work Max. Yearly Accrual – 40 hours per 12 month period Max. Carryover – 20 hours to next 12 month period Total Max. Accrual – 60 hours
Benefits for Tipped Employees	Same as current law, adjusted annually for inflation beginning July 1, 2018	Same as all other covered employees

III. HOW TO INTERPRET DIFFERENT TYPES OF DATA

To help interpret the volumes of data, studies and reports, some of them apparently contradictory, the Working Group developed a framework for identifying a hierarchy of information, starting with the most reliable and proceeding to what is less reliable (and, in some cases, even to be avoided). This hierarchy is summarized in the table below.

Type	Description	Value / weight to give
A	Facts and Data	While few questions will be answered fully by simple facts and data, misconceptions can be clarified and the basis for more complex analysis established. Weight to give: Very High
B1	Peer-reviewed studies and meta-studies of relevant situations	Similar ordinances to those passed by Cook County have been implemented in other regions. Where results have been studied and published in peer reviewed journals, high weight should be given to the conclusions. In particular, meta-studies (reviews of multiple studies) are most illuminating. Weight to give: High
B2	Non-peer reviewed studies (e.g., working papers, papers from advocacy groups)	While non-peer reviewed studies can provide ideas and raise important questions, their recommendations should be interpreted with caution, especially for those produced by advocacy groups. Weight to give: Moderate, depending on availability of other, peer reviewed studies
C	Surveys about current practices or opinions	If constructed carefully to ensure that results are representative and avoid speculation about future actions, surveys can provide important input for Trustees on the opinions and current practices of stakeholders. Survey questions which ask respondents to speculate about future behavior should be avoided wherever possible and such speculative responses weighed accordingly in light of the challenges associated with predicting future behavior and respondents' inherent self-interest. Weight to give: Moderate to High, for questions that are based on current opinions and beliefs with low weight and great caution about speculation regarding future purchasing decisions.
I	Information	The Working Group identified qualitative background material that may be relevant to the Village Board. Weight to give: Information only

IV. Questions On Minimum Wage

1. Who are the workers (i.e., demographics and why they work) who would be impacted by a change in minimum wage?

Why does this matter?

To understand the employees that will be impacted by the change in minimum wage. The Cook County Ordinance exempts businesses with less than four employees, employees under the age of 18, trainees during their first 90 days of employment, and certain other employees; the Cook County Ordinance also has different pay regulations for tipped employees. Understanding the employees impacted by the Ordinance will help answer Question #4 below (what are the impacts on Wilmette employees).

What does the data say?

Data Type: A (Facts and Data)

Source: Illinois Department of Employment Security (IDES)

Detail in: Appendix 2

IDES defines low-wage workers as those earning \$1,250 per month or less:

- 1,347 of 6,557 workers in Wilmette are low-wage workers making less than \$1,250 per month in their primary job (about 20% of all Wilmette workers are low-wage)
- 250 (nearly 20%) low-wage workers in Wilmette live in Wilmette
- An estimated 70-80% of low-wage workers in Wilmette are working to support families, rather than in “starter” or summer jobs
- Of the low-wage workers who are older than 29, nearly 2/3 have some college education
- The vast majority of low-wage workers in Wilmette work in four sectors:
 - Retail (460 or 34%)
 - Hotel and Food Services (298 or 22%)
 - Other Services (142 or 11%)
 - Health Care and Social Assistance (133 or 10%)

Data Type: C (Surveys about current practices or opinions)

Source: Survey of Wilmette Business Owners

Details in Appendix: 3

The Working Group conducted an online and paper survey of Wilmette business owners. Of 558 licensed businesses, 220 responded (a 39% response rate).

Below are four tables that characterize the type of worker employed in Wilmette (full-time, part-time, temporary, under 18, tipped) and the pay rates for workers in Wilmette.

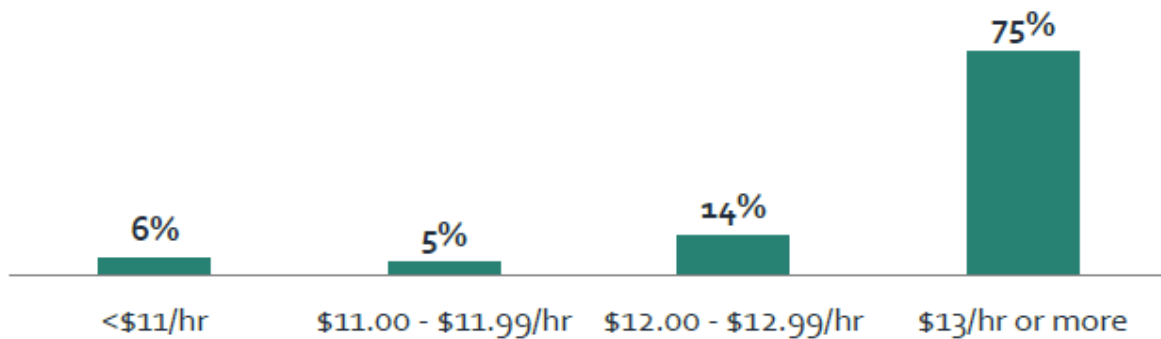
Employee Status in an Organizations



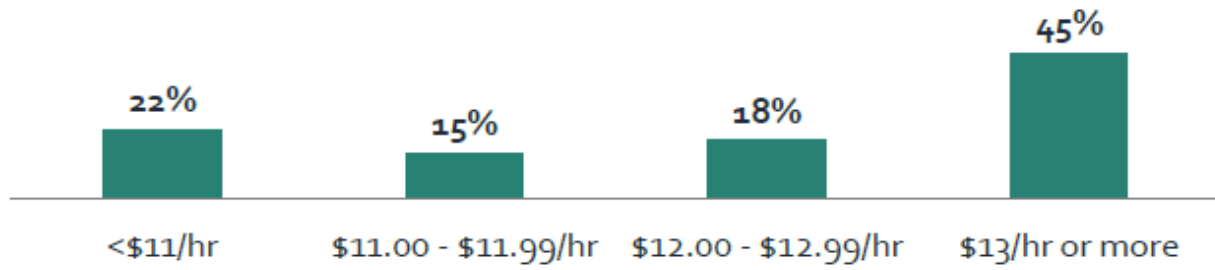
Employee Group	% of Responding Businesses with Number of Employees				
	< 4	4-10	11-25	26-40	> 40
Total Employees (n=220)	41%	29%	18%	5%	7%
Full-time Employees (n=216)	54%	25%	13%	4%	4%
Part-Time Employees (n=212)	71%	18%	8%	1%	2%
Employees Working Less than 90 Days in a Consecutive Year (n=207)	85%	10%	5%	0%	0%
Employees Under the age of 18 (n=200)	94%	1%	1%	0%	1%
Tipped Employees (n=197)	87%	7%	4%	1%	1%

n=responding businesses

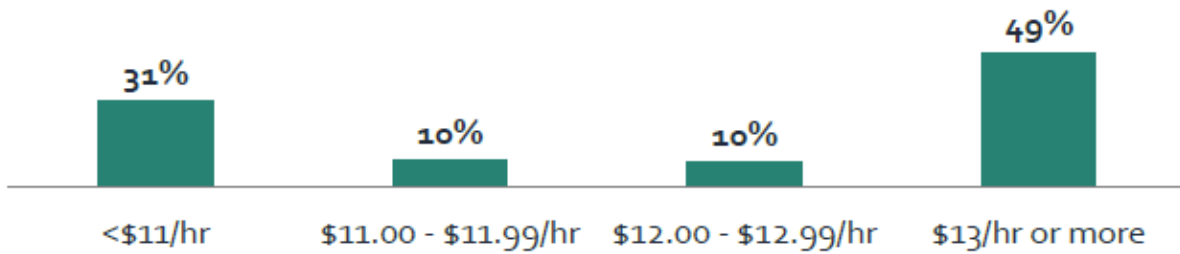
% of Full – time Employees Paid* (n=164)



% of Part – time Employees Paid* (n=132)



Lowest Starting Hourly Rate for Full-time Employees (n=186)



Question 2: What businesses might be impacted by a change in the minimum wage?

Why does this matter?

To understand the businesses that might be impacted by a change in minimum wage. The Village has approximately 560 licensed businesses and some sectors tend to have more and some fewer minimum wage workers. The Cook County Ordinance exempts businesses with fewer than 4 employees, employees under the age of 18, trainees during their first 90 days of employment, and certain other employees; the ordinance has different pay regulations for tipped employees. Understanding the businesses impacted by the Ordinance will help answer Question #5 below (what might be the impacts to Wilmette businesses).

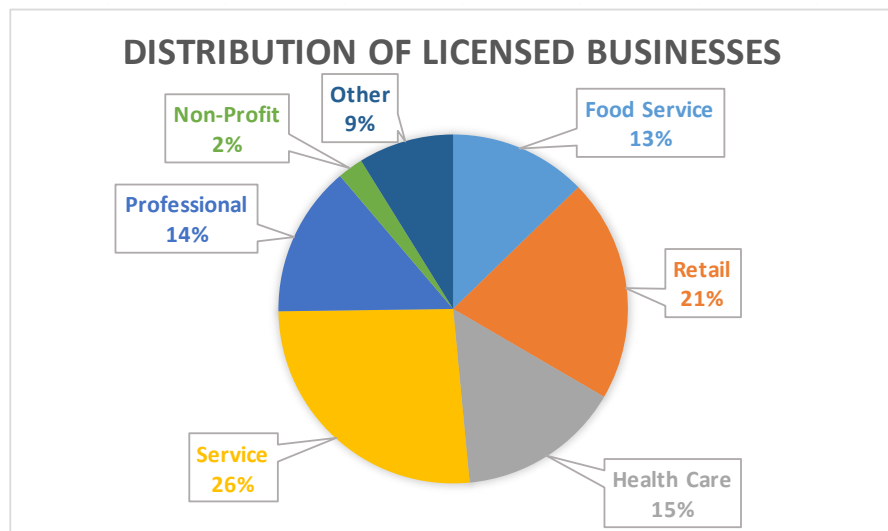
What does the data say?

Data Type: A (Facts and Data)

Source: Village of Wilmette Business License Data

The table and chart below show the number of licensed businesses in each sector in Wilmette.

Categories	Businesses
Food Service	59
Retail	96
Health Care	70
Service	122
Professional	65
Non-Profit	11
Other	41
Total	464



The total number of 464 licensed business is less than the total number of employers in Wilmette (558) because churches, schools, units of local government and home occupations are not required to obtain a local business license, though some voluntarily do, and are therefore not reflected in these numbers.

Question 3A: What is the cost of living in Wilmette and other places where Wilmette's low-wage workers live vs. other regions of the State?

Why does this matter?

The primary rationale for a minimum wage that is different in one region of a state vs. another is that the cost of living (and hence what defines a “living wage”- see page 12 for a definition of living wage) can differ by region.

What does the data say?

Data Type: A (Facts and Data)

Source: US Department of Commerce, Bureau of Economic Analysis (www.bea.gov)

The cost of living, as measured by Regional Price Parity,¹ is about 20% higher in the Chicago Region than in the rest of the State of Illinois:

Regional Price Parity for United States, Chicago MSA, and rest of IL

US	Chicago MSA²	Rest of Illinois
100	104.6	85.2

Question 3B: What constitutes a “living wage” (in terms of hourly rate equivalent) for workers living in Cook County? How do the current proposed minimum wage rates compare to the generally accepted “living wage” in Cook County?

Why does this matter?

The rationale for a higher minimum wage is that those who work full-time and depend on work to support their families should not, because of a low wage rate, be forced to live in poverty or rely on public assistance; i.e., they should earn a “living wage.”

What does the data say?

Data Type: A (Facts and Data)

Source: MIT Living Wage Calculator for Cook County. <http://livingwage.mit.edu/counties/17031>

The living wage, as estimated by the MIT Living Wage Calculator, is the hourly rate that an individual must earn to support his or her family, if that person is working full-time (2080 hours per year). All values are per adult in a family unless otherwise noted.

¹ Regional Price Parities (RPPs) measure the differences in price levels across states and metropolitan areas and are expressed as a percentage of the overall national price level. All items RPPs cover all consumption goods and services, including rents.

² A Metropolitan statistical area (MSA) is a geography having a high degree of social and economic integration. The Chicago MSA includes Cook, DeKalb, DuPage, Grundy, Kendall, Lake, McHenry, and Will Counties. Thus, workers living in Wilmette or in the surrounding communities have similar costs of living, about 20% greater than the rest of Illinois.

		1 Adult	1 Adult 1 Child	1 Adult 3 Children	2 Adults (1 Working)	2 Adults (1 Working) 3 Children
Living Wage per MIT Calculator		\$13.30	\$26.98	\$38.58	\$21.03	\$30.08
Cook County Minimum Wage	current	\$8.25	\$8.25	\$8.25	\$8.25	\$8.25
	2018	\$11	\$11	\$11	\$11	\$11
	2019	\$12	\$12	\$12	\$12	\$12
	2020	\$13	\$13	\$13	\$13	\$13

		2 Adults (both working full-time)	2 Adults (both working) 1 Child	2 Adults (both working full-time) 2 Children	2 Adults (both working full-time) 3 Children
Living Wage per MIT Calculator		\$10.51	\$14.36	\$17.01	\$19.61
Cook County Minimum Wage	Current	\$8.25	\$8.25	\$8.25	\$8.25
	2018	\$11	\$11	\$11	\$11
	2019	\$12	\$12	\$12	\$12
	2020	\$13	\$13	\$13	\$13

The MIT model is a market-based approach that draws on geographically specific expenditure data related to a family’s likely minimum food, childcare, health insurance, housing, transportation, and other basic necessities (e.g. clothing, personal care items, etc.) costs to determine the minimum employment earnings necessary to meet a family’s basic needs while also maintaining self-sufficiency.

The model accounts for only the basic needs of a family and does not allow for example pre-prepared meals or those eaten in restaurants. It does not include money for entertainment nor does it allocate leisure time for unpaid vacations or holidays. It also does not provide a financial means for planning for the future through savings and investment or for the purchase of capital assets (e.g. provisions for retirement or home purchases). The model is the minimum income standard that, if met, draws a very fine line between the financial independence of the working poor and the need to seek out public assistance or suffer consistent and severe housing and food insecurity.³

³ livingwage.mit.edu

Question 4: What might be the impact of increasing the minimum wage on employees in Wilmette?

Why does this matter?

The primary rationale for raising the minimum wage for employees is that it will increase the opportunity for work to serve as a path out of poverty and allow those who work to support the basic needs of themselves and their families. A concern with raising the minimum wage for employees is that some low wage jobs may be lost or the hours of work of some employees may be reduced, i.e., it would have a negative impact on overall employment.

What does the data say?

Data Type: B1 (peer reviewed or meta studies)

Sources:

- Doucouliagos and Stanley, “Public Selection Bias in Minimum Wage Research? A Meta-Regression Analysis” (2009) – A peer-reviewed meta study – (Abstract only provided below)
- Belman, Wolfson, and Nawakitphaitoon, “Who is Affected by the Minimum Wage?” (2015) - A peer-reviewed study (Appendix 4)

The peer-reviewed studies reviewed by the Working Group suggest one of two impacts of raising the minimum wage on employment: either no impact, or negative impact for a small percentage of workers. Abstracts from the two most relevant (peer reviewed studies) are provided below. Given the technical nature of the studies, the Working Group determined it would appropriate to provide one study in full in the appendices (Belman, Wolfson, Nawakitphaitoon) and only the abstract of the Doucouliagos and Stanley study.

Doucouliagos and Stanley, “Public Selection Bias in Minimum Wage Research? A Meta-Regression Analysis” (2009)

Card and Krueger’s meta-analysis of the employment effects of minimum wages challenged existing theory. Unfortunately, their meta-analysis confused publication selection with the absence of a genuine empirical effect. We apply recently developed meta-analysis methods to 64 US minimum-wage studies and corroborate that Card and Krueger’s findings were nevertheless correct. The minimum-wage effects literature is contaminated by publication selection bias, which we estimate to be slightly larger than the average reported minimum wage effect. ***Once this publication selection is corrected, little or no evidence of a negative association between minimum wages and employment remains [emphasis added only to direct reader’s attention to conclusion].***

Belman, Wolfson, and Nawakitphaitoon, “Who is Affected by the Minimum Wage?” (2015)

Prior surveys of empirical research on the minimum wage have been organized around the question “What does the minimum wage affect?” This survey is organized around the question “Who is affected by the minimum wage?” We review the consequences of the minimum wage for teens and young workers, men and women, African Americans and Hispanics, the less educated, workers in low-wage industries, and low-wage/low-income populations. **Although there is almost universal agreement that the minimum wage boosts earnings, evidence for a negative employment effect varies between mixed and nonexistent. An important gap in the literature is the paucity of research on low-wage/low-income groups [emphasis added only to direct reader’s attention to conclusion].**

Data Type: B2 (non-peer reviewed working papers)

Sources:

- Congressional Budget Office, “The Effects of a Minimum-Wage Increase on Employment and Family Income” (2014) (Appendix 5)
- Schmitt, “Why Does the Minimum Wage Have No Discernible Effect on Employment” (2013) (Appendix 6)
- Jardim, Long, Plotnick, Inwegen, Vigdor, Wething, “Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence From Seattle” (2017) (Appendix 7)
- Reich, Allegretto, and Godoey, “Seattle’s Minimum Wage Experience 2015-16” (2017) (Appendix 8)

While not meeting the highest bar for consideration, the Working Group found four working papers particularly enlightening and worth considering. The two working papers in this section regarding the impact of the Seattle minimum wage highlight the problematic nature of utilizing non-peer reviewed working papers as opposed to peer-reviewed studies. Working papers are often amended over time and rebutted by other working papers and thus the Working Group recommends greater weight be given to the peer-reviewed studies noted on page 13 of this report.

Congressional Budget Office, “The Effects of a Minimum-Wage Increase on Employment and Family Income” (2014)

Increasing the minimum wage would have two principal effects on low-wage workers. Most of them would receive higher pay that would increase their family’s income, and some of those families would see their income rise above the federal poverty threshold. But some jobs for low-wage workers would probably be eliminated, the income of most workers who became jobless would fall substantially, and the share of low-wage workers who were employed would probably fall slightly [emphasis added only to direct reader’s attention to conclusion].

Schmitt, “Why Does the Minimum Wage Have No Discernible Effect on Employment” (2013)

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 [emphasis added only to direct reader’s attention to conclusion].

Jardim, Long, Plotnick, Inwegen, Vigdor, Wething, “Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence From Seattle” (2017)

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 \$11 per hour in 2015 and to \$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 [emphasis added only to direct reader’s attention to conclusion].***

Reich, Allegretto, and Godoey, “Seattle’s Minimum Wage Experience 2015-16” (2017)

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. [emphasis added only to direct reader's attention to conclusion].

Question 5: What might be the impact of increasing the minimum wage on businesses in Wilmette?

Why does this matter?

The most common concerns with raising the minimum wage is that businesses may be at a competitive disadvantage and higher labor costs will force businesses to reduce employment, close, move, or not open in Wilmette. At the same time, one rationale for increasing the minimum wage is that it can enhance the recruitment and retention of good employees who are important to the success of local businesses and the local economy.

The Working Group identified four sources of information relevant to this question:

- A. Studies of Cross Border Impacts
- B. Broader Studies on Business Impacts
- C. Surveys of the Wilmette Business Community, North Shore Municipal Staff, North Shore Businesses
- D. Recent Tax Law Changes on Small Businesses

A. Cross Border Impacts – what does the data say?

Data Type: A (Facts and Data)

Source: Illinois Department of Employment Security, *Where Workers Work* (2017)

In December of 2014, the City of Chicago passed an ordinance that required annual increases in the minimum wage starting July 1, 2015, from the State minimum wage of \$8.25/hour (currently the minimum wage in Wilmette) to \$10.00 in 2015, \$10.50 in 2016, and \$11.00 in 2017. During this time, the minimum wage in Suburban Cook County remained fixed, at the State minimum wage of \$8.25/hour.

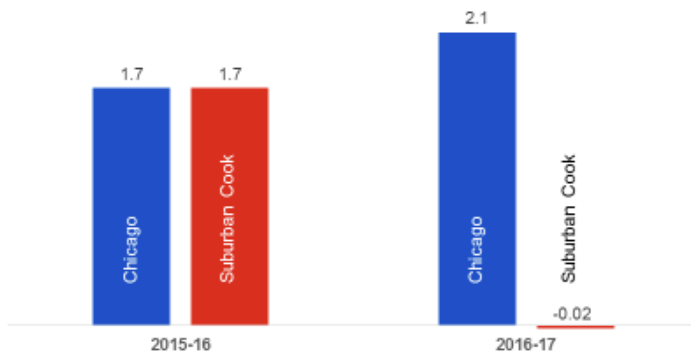
Data from the Illinois Department of Employment Security demonstrates that there was no correlation over the two years of minimum wage increase in Chicago between job growth in Chicago and in Suburban Cook County, in either the aggregate or along border municipalities and abutting Chicago neighborhoods [emphasis added only to direct reader's attention to conclusion].

In the aggregate (first chart on the following page) private sector job growth from 2015-16 in Chicago and Suburban Cook County was roughly equal (1.7% in both the City and Suburban Cook County) in the first year of implementing Chicago's minimum wage increase. After the second increase in Chicago (2016-17) job growth in the City was much greater (2.1%) than in Suburban Cook County, where minimum wage remained fixed at \$8.25/hour and employment growth was essentially flat.

Along borders (second chart below) there was likewise no correlation between minimum wage increases and job growth. Each data point in Chart 2 represents growth in a border municipality (y-axis) and abutting Chicago neighborhood (x-axis). The solid line represents equal growth in both the border municipality and abutting Chicago neighborhood. Data points above the line represent faster job growth in border municipalities and data points below the line represent faster job growth in Chicago border neighborhoods. As evidenced by the broad scattering of data points above and below the line, there is no statistical correlation between increases in minimum wage and job growth along abutting Chicago / municipal borders.

Private Sector Job Growth in the City of Chicago and Suburban Cook County

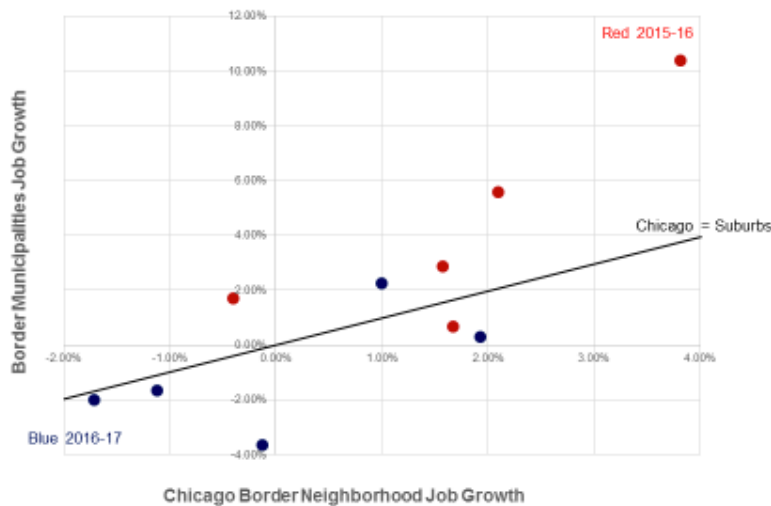
% Change



Source: IL Department of Employment Security, Where Workers Work, December 2017

3

Private Sector Job Growth in Chicago Neighborhoods and Corresponding Border Municipalities



Source: Illinois Department of Employment Security, Where Workers Work report
 Note: Employment as of March in indicated year; Each dot represents one geography (North, Northwest, West, Southwest, and South)

Data Type: B2 (non-peer reviewed working papers)

Sources:

- Dube, Lester, and Reich, “Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties” (2010) (Abstract only provided below)
- Neumark, Salas, and Wascher, “More on recent evidence on the effects of minimum wages in the United States” (2014) (Abstract only provided below)

In addition to the data from IDES, the Working Group reviewed economic studies related to this question and identified two relevant studies. While not peer-reviewed, these studies provide some insight into potential effects, concluding that there is either no effect or a limited adverse effect involving job loss for very low skilled workers, especially teens. Note: The Cook County Minimum Wage law exempts employees under the age of 18.

Dube, Lester, and Reich, “Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties” (2010)

We use policy discontinuities at state borders to identify the effects of minimum wages on earnings and employment in restaurants and other low-wage sectors. Our approach generalizes the case study between 1990 and 2006. We compare all contiguous county-pairs in the United States that straddle a state border and find no adverse employment effects. ***We show that traditional approaches that do not account for local economic conditions tends to produce spurious negative effects due to spatial heterogeneities in employment trends that are unrelated to minimum wage policies. Our findings are robust to allowing for long-term effects of minimum wage changes [emphasis added only to direct reader’s attention to conclusion].***

Neumark, Salas, and Wascher, “More on recent evidence on the effects of minimum wages in the United States” (2014)

A central issue in estimating the employment effects of minimum wages is the appropriate comparison group for states (or other regions) that adopt or increase the minimum wage. In recent research, Dube et al. (Rev Econ Stat 92:945-964, 2010) and Allegretto et al. (Ind Relat 50:205-240, 2011) argue that past U.S. research is flawed because it does not restrict comparison areas to those that are geographically proximate and fails to control for changes in low-skill labor markets that are correlated with minimum wage increases. They argue that using “local controls” establishes that higher minimum wages do not reduce employment of less-skilled workers. In Neumark et al. (Ind Labor Relat Rev 67:608-648, 2014), we present evidence that their methods fail to isolate more reliable identifying information and lead to incorrect conclusions. Moreover, for subsets of treatment groups where the identifying variation they use is supported by the data, the evidence is consistent with past findings of disemployment effects. Allegretto SA,

Dube A, Reich M, Zipperer B (2013a) Credible research designs for minimum wage studies. IZA Discussion Paper No. 7638, Bonn, Germany have challenged our conclusions, continuing the debate regarding some key issues regarding choosing comparison groups for estimating minimum wage effects. We explain these issues and evaluate the evidence. ***In general, we find little basis for their analyses and conclusions and argue that the best evidence still points to job loss from minimum wages for very low-skilled workers – in particular, for teens [emphasis added only to direct reader’s attention to conclusion].***

B. Broader Studies on Business Impacts – what does the data say?

Data Type: B2 (non-peer reviewed working papers)

Sources:

- Schmitt, “Why Does the Minimum Wage Have No Discernible Effect on Employment” (2013) (Appendix 6)
- Luca and Luca, “Survival of the Fittest: The Impact of the Minimum Wage on Firm Exist” (2017) (Appendix 9)

The Working Group reviewed economic studies related to this question and identified two relevant studies. While not peer-reviewed, these studies provide some insight into potential effects on businesses. A note from the Working Group on the Luca and Luca study: literature regarding the impacts of the minimum wage is almost entirely focused on the impacts on employment; this is the only study the Working Group found that addresses the specific impact on business entities.

Schmitt, “Why Does the Minimum Wage Have No Discernible Effect on Employment” (2013)

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 [emphasis added only to direct reader's attention to conclusion].

Lucas and Luca, "Survival of the Fittest: The Impact of the Minimum Wage on Firm Exit" (2017)

We study the impact of the minimum wage on firm exit in the restaurant industry, exploiting recent changes in the minimum wage at the city level. ***The evidence suggests that higher minimum wages increase overall exit rates for restaurants. However, lower quality restaurants, which are already closer to the margin of exit, are disproportionately impacted by increases to the minimum wage [emphasis added only to direct reader's attention to conclusion].*** Our point estimates suggest that a one dollar increase in the minimum wage leads to a 14 percent increase in the likelihood of exit for a 3.5-star restaurant (which is the median rating), but has no discernible impact for a 5-star restaurant (on a 1 to 5 star scale).

C. Surveys of the Wilmette Business Community, North Shore Municipal Staff, and North Shore Businesses – what does the data say?

Data Type: C (Surveys about current practices or opinions)
Source: Survey of the Wilmette Business Community
Details in Appendix 3

The Working Group conducted an online and paper survey of Wilmette business owners. Of 558 licensed businesses (including the 94 businesses that have voluntarily sought licenses), there were 220 respondents (a 39% response rate). Full results are included in Appendix 3.

The survey found the following:

- 54% of Wilmette business owners responding to the survey oppose the minimum wage increase while 44% support the increase (2% did not answer the question)
- Of the three sectors with the most low wage workers:
 - 70% of restaurant respondents oppose the minimum wage increase
 - 70% of retail respondents oppose the minimum wage increase
 - 53% of service respondents oppose the minimum wage increase

Data Type: C (Surveys about current practices or opinions)
Source: Survey of the Business Owners in North Shore Communities
Details in Appendix 10

Note: The following information may not be representative of the North Shore business community due to the low number of respondents.

The Minimum Wage & Paid Sick Leave Working Group asked Village staff to interview businesses to help understand the effects of the Cook County Ordinances. Questions for the interviews were developed by the Working Group. Village staff reached out to business owners of surrounding communities that did not opt out of the Cook County Ordinances or who did not have an option because they are not Home Rule municipalities. These communities include Glencoe, Winnetka, Skokie and Evanston. Below is a summary of the responses to those interviews.

As instructed, all interviews were conducted over the phone anonymously. An attempt was made to obtain an equal proportion of surveys from business categories in other communities as exist in Wilmette. For example, if 21% of the businesses in Wilmette are retailers, 21% of the survey responses would be from retailers. This proved to be difficult, as most businesses did not wish to participate in the survey.

Of the 70 businesses contacted only eight businesses agreed to be interviewed. Four were retailers, two were restaurants and two were professional businesses. Many questions were not answered because the business did not wish to answer the question or were not sure how to answer the question.

Minimum Wage Question Results

- Three retailers had less than 4 employees and were not subject to the ordinance
- One retailer already paid above minimum wage
- Two restaurants said they were impacted and increased prices, but did not want to quantify
- Two professional offices already paid above minimum wage

Sick Leave Question Results

- One business started providing paid sick leave
- Two businesses were unaware of the paid sick leave ordinance
- Four businesses were already providing paid sick leave
- One business did not respond to the question

Three businesses track sick time manually, three use computer software and one outsources this function. The one business who outsources tracking is a professional office with 22

employees. This business provided paid time off and outsourced payroll prior to the Cook County ordinance. Thus, we are unable to quantify the additional costs, if any, to administer paid sick leave.

When asked if the minimum wage and paid sick leave ordinances would be a factor in opening or relocating their business, three of the eight businesses provided a response. Two businesses said it would not be a factor and one business said it would account for 5-10% of their decision.

Data Type: C (Surveys about current practices or opinions)

Source: Survey of North Shore Municipalities

Details in Appendix: 10

Note: The following information may not be representative of North Shore municipalities due to the low number of respondents.

The Minimum Wage & Paid Sick Leave Working Group asked Village staff to interview municipal staff in other North Shore communities to help understand the effects of the Cook County Ordinances. Questions for the interviews were developed by the Working Group. Village staff reached out to municipal officials of surrounding communities that have not opted out of the Cook County Ordinances or that did not have an option because they are not Home Rule municipalities. These communities include Glencoe, Winnetka, Skokie and Evanston. Each of these contacts were made via phone calls placed with economic development offices or municipal administration.

None of the communities agreed to respond to the survey questions. While Glencoe did not formally respond to the questions, they did say too little time has passed to gain much insight into the impact of the ordinances. In addition, no businesses had contacted Glencoe to express concern or support about the ordinances since they became effective in July of 2017.

Skokie and Winnetka stated they did not wish to participate in the surveys after checking with municipal administration (Winnetka letter to the Village of Wilmette can be found at the end of Appendix 10). In those cases, they felt the issue was still very sensitive in their business community. They did not want to initiate/re-open debate by participating in the survey. Evanston did not respond to our inquiries. None of the four communities wished to assist the Village in finding potential businesses to survey.

D. Impact of Tax Law Changes on Small Businesses– what does the information say?

Data Type: I (Information)

Source: Memorandum from Wilmette resident Gina Kennedy

Details in Appendix: 11

One concern expressed with raising the minimum wage is that businesses may have to raise prices to compensate for the resulting increase in labor costs. Recently, major changes were made in federal income tax laws, effective January 1 of this year, that, among other things, reduces the income tax burden on businesses generally. Proponents for these changes have asserted that this reduction in the federal tax burden would afford businesses with the capital needed to increase employee compensation and hire additional workers. However, the actual impact of these tax law changes are, of course, unknowable at present.

Village resident Gina Kennedy, an attorney who specializes in federal tax issues affecting corporate transactions, submitted a memorandum summarizing the recent tax law changes applicable to business entities. It should be noted that the tax benefits described in this section are not as significant for businesses that are marginally profitable or operating at a loss. Ms. Kennedy summarizes the changes as follows:

The changes in federal tax law effectuated by the Act are among the most wide-ranging in recent decades. Overall, they will result in a significant reduction in the federal tax obligations (and, in some cases, a corresponding reduction in the state tax obligations) of businesses and their owners. While these benefits are not evenly distributed across businesses -- larger businesses clearly stand to gain more than smaller ones, in general, and certain classes of businesses are favored over others -- most businesses and their owners will realize a substantial increase in their after-tax income.

Question 6: What might be the impact of increasing the minimum wage on Wilmette as a community?

Why does this matter?

There are a number of possible impacts to the community of raising or not raising the minimum wage. These may include, but are not limited to, the following: 1) Raising the minimum wage might lead to increased prices for goods and services and uncertainty whether customers will continue to patronize businesses; 2) Not raising the minimum wage might diminish the perception of Wilmette as a thriving community that values the welfare of its workers; 3) Raising the minimum wage might diminish the perception of Wilmette as an economically attractive place to do business and might present an obstacle to the Village's future economic development efforts and ability to recruit new businesses; 4) Raising the minimum wage would be consistent with the outcome of the 2014 statewide advisory referendum in Wilmette, although the minimum wage rate presented in that referendum (\$10 per hour effective January 1, 2015 on a statewide basis) is different from the minimum wage rates contained in the County's minimum wage ordinance.

The Working Group reviewed this question in three separate parts:

- A. Survey of Wilmette Residents
- B. Survey of North Shore Commercial Brokers
- C. Feedback from the Wilmette Faith Community

A. Survey of Wilmette Residents – what does the data say?

Data Type: C (Surveys about current practices or opinions)

Source: Fallon Research & Communications, Inc.

Details in Appendix: 12

The Working Group commissioned a phone survey of 300⁴ Wilmette residents conducted by Fallon Research & Communications, Inc.. This survey research data was gathered through telephone interviews that specially-trained interviewers conducted with 303 randomly-selected adult residents of the Village of Wilmette, Illinois, who had valid residential, VOIP or cellular telephone numbers. The interviews were performed during the period of April 11, 2018 through April 13, 2018. The overall estimated margin of sampling error is +/- 5.62%, based on a confidence level of 95%, although it varies for each individual question. This means that if this survey were repeated, 95 times out of 100 the results would be within plus or minus 5.62% of those provided herein. Some adjustments were made to weight the results toward demographic and geographic characteristics of the village's adult population, in order to

⁴ Although only 300 interviews were targeted by the Village, 303 were actually completed because simultaneous interviews were conducted.

account for under- and over-sampling that normally occurs as a result of the random selection process, and to ensure that all major sub-groups are represented in proportion to their actual percentages. Like all polls, this opinion survey research is subject to other possible sources of error, such as unintentional bias in the wording of questions, data-entry error and nonresponse bias.

While there were a total of 12 questions, the questions directly related to Minimum Wage are as follows:

Q. 4. The Cook County ordinance requires local businesses to increase the minimum wage for most employees except those in businesses with less than 4 employees, teens under age 18, trainees during their first 90 days, and independent contractors. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?

66.4% Should follow
26.7 Should not
6.9 Unsure/no answer

Q. 8. Does knowing that the Village has chosen not to follow the Cook County ordinances, so businesses in Wilmette are not required to increase the minimum wage or offer paid sick leave to employees, make your opinion of the Village of Wilmette more favorable, less favorable or does it not affect your views?

15.2% More favorable
44.4 Less favorable
36.5 No effect
3.9 Unsure/no answer

Q. 9. Since research shows that restaurant prices tend to increase following a raise in the minimum wage, would you be more or less likely to patronize Wilmette restaurants if the minimum wage is increased or does it have no effect on your decision?

10.3% More likely
12.3 Less likely
74.5 No effect
2.9 Unsure/no answer

Appendix 12 includes the following data and information: 1) General survey results; 2) Cross-tabulated survey results; 3) Report on the investigation of a resident survey incident.

B. Survey of North Shore Commercial Brokers – what does the data say?

Data Type: C (Surveys about current practices or opinions)
Source: Phone Interviews Conducted by Village of Wilmette Staff
Details in Appendix: 10

The Minimum Wage & Paid Sick Leave Working Group asked Village staff to interview brokers to help understand the effects of the Cook County Ordinances. Questions for the interviews were developed by the Working Group. Three commercial real estate brokers were interviewed. Each of them has listings throughout the North Shore with a good understanding of local transactions. None of the brokers had heard the issue raised when discussing locations with clients. They stated the most important factors tenants consider when selecting a location are:

- Price of rent
- Physical location
- Access to parking

The three brokers did not think the existence of the Cook County ordinances would attract or discourage a tenant from locating in a particular community.

C. Feedback from the Wilmette Faith Community – what does the data say?

Data Type: I (Information)
Source: Letters from Members of the Wilmette Clergy
Details in Appendix: 13

At the suggestion of a Village resident, the Working Group solicited feedback from Wilmette Clergy. A total of three Wilmette clergy members submitted written responses, all of whom supported an increase in the minimum wage. A copy of the clergy responses can be found in Appendix 13.

V. Questions On Paid Sick Leave

1. Who are the workers (i.e., demographics and why they work) who would be impacted by paid sick leave?

Why does this matter?

Unlike the minimum wage Ordinance, all employees (not including independent contractors) are eligible for paid sick leave benefits under the County Ordinance. Understanding the employees impacted by the Ordinance will help answer Question #6 below (what might be the impacts on employees in Wilmette).

What does the data say?

Data Type: A (Facts and Data)

Source: Illinois Department of Employment Security

Detail in: Appendix 14

IDES provides the following data for all workers in Wilmette:

- There are a total of 7,257 jobs in Wilmette
- 3,536 (48.7%) of workers in Wilmette are between the ages of 30 and 54
- Workers in Wilmette are generally educated, nearly 3/4 have some college, an associates, bachelors, or graduate degree
- 65% of workers in Wilmette work in five sectors:
 - Educational services (1,363 or 19%)⁵
 - Retail (1,275 or 18%)
 - Hotel and food services (779 or 11%)
 - Health care and social assistance (634 or 9%)
 - Other services (569 or 8%)

Data Type: C (Surveys about current practices or opinions)

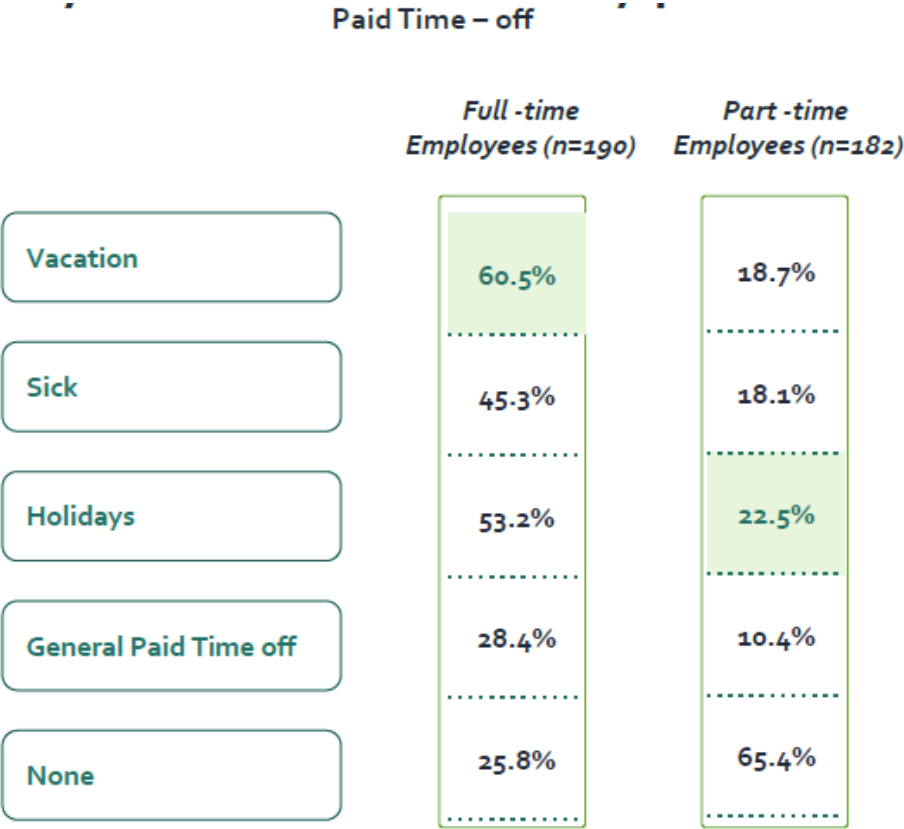
Source: Survey of Wilmette Business Owners

Details in Appendix: 3

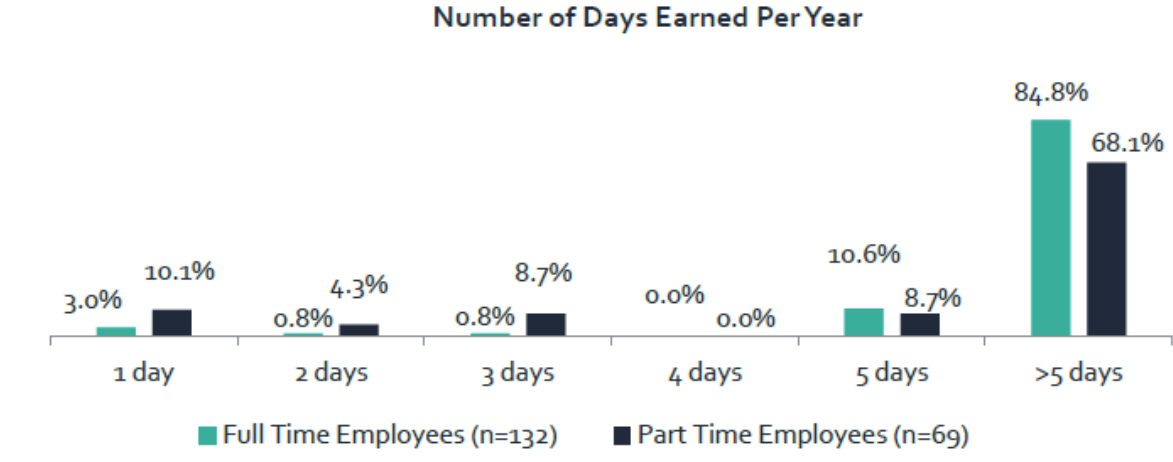
The Working Group conducted an online and paper survey of Wilmette business owners. Of 558 licensed businesses, 220 responded (a 39% response rate). The survey results are included in Appendix 3 and on the following page are two tables that summarize the paid time off provided to workers in Wilmette.

⁵ Many educational service workers in Wilmette are exempt from the Ordinance as they work for a unit of local government.

The first chart demonstrates that 75% of responding businesses provide some form of paid time off to full-time employees and 35% of responding businesses provide some form of paid time off to part-time employees.



The second chart demonstrates that 95% of the responding businesses that offer paid time off to full-time employees provide five or more days off per year and 77% of responding businesses provide part-time employees with five or more days off.



2. What might be the cost of providing paid sick leave to employees?

Why does this matter?

Employers have voiced concern about the cost of paying employees when they stay home sick.

Data Type: A (Facts and Data)

Source: Cost Model Developed by the Civic Consulting Alliance

Detail in: Appendix 15

A cost model developed by the Civic Consulting Alliance, which was utilized in the City of Chicago's 2016 Working Families Task Force report, found that paid sick leave would cost employers 0.7 – 1.5% of payroll. The relevant excerpt from the Working Families Task Force report can be found below:

The Task Force asked for a cost analysis to be done to determine the recommendation's potential cost implications for businesses. The model was built based on the model used in the Mayor's Minimum Wage Working Group. It relied upon the best publicly available sources of data, including:

Bureau of Labor Statistics data on wages in the Chicago metropolitan statistical area, Bureau of Labor Statistics data on the projected inflation in wages and healthcare, as well as Congressional Budget Office estimates of the Consumer Price Index Estimates of benefit costs to employers from the Bureau of Labor Statistics and major payroll and accounting firms.

Using this information, the cost model provided an estimate of the potential costs. Because of its underlying assumptions, the numbers it produced were not meant to be taken as the certain outcome of any policy, but instead as a tool for the Task Force to use when evaluating its proposals. Using the Task Force recommendations discussed above (in particular, a 40 hour cap with one hour accrued for every 40 hours worked and a 20 hour cap on time rolling over from year to year), the model projected that for a full-time, non-tipped worker making the median hourly wage in the Chicago area in 2016, the proposals would cost employers:

0.8-1.5% of base wages

0.7-1.3% of compensation costs under current law for large companies

0.7-1.5% of compensation costs under current law for small companies

The ranges are based on different assumptions about usage, ranging from 40% (low) to 80% (high). For reference, national estimates on usage range from two days in leisure and hospitality to four days in professional services which would be between 40% to 80% of the proposed cap.

The cost of the Task Force's proposal on banking paid sick days toward days that could be used for FMLA- eligible purposes was not included in the model as data on usage of paid FMLA leave in the United States is less commonly available. Initial estimates would indicate that these costs would be significantly lower than those of paid sick leave, but further study on this issue is encouraged.

It should be noted that the model was meant to capture the additional costs imposed on companies by the proposed policies, and does not cover the already existing costs employers incur to replace sick employees when they take unpaid time off. Moreover, it did not include any potential cost-savings to employers, which could result due to improved employee morale, decreased turnover, or increased productivity. While any potential cost-savings are not included in the model given the lack of data available specific to the Chicago region, the task force considered studies that outline these potential benefits.

3. What might be the additional cost to employers due to employees who might abuse sick leave?

Why does this matter?

Employers have voiced concern that some employees may take time off when they are not actually sick or caring for a sick family member.

The Working Group reviewed this question in two separate parts:

- A. Cost Model Developed by the Civic Consulting Alliance
- B. Broader Studies on Business Impacts

A. Cost Model Developed by the Civic Consulting Alliance – what does the data say?

Data Type: A (Facts and Data)

Source: Cost Model Developed by the Civic Consulting Alliance

Detail in: Appendix 15

The model utilized by the Civic Consulting Alliance to answer question #2 above (cost to employers) assumed a sick leave usage rate of 40-80%. The ranges are based on different assumptions about usage, ranging from 40% (low) to 80% (high). For reference, national estimates based on a 2012 study by Barthold and Ford on sick leave usage range from two days in leisure and hospitality to four days in professional services.

Assuming the worst case scenario from the standpoint of businesses – 100% usage by all employees – the cost is projected to rise to no more than 2% of base wages.

B. Broader Studies on Business Impacts – what does the data say?

Data Type: B2 (non-peer reviewed working papers)

Sources:

- Drago and Lowell, “San Francisco’s Paid Sick Leave Ordinance: Outcomes for Employers and Employees” (2011) (Appendix 16)

The Working Group reviewed an economic study that discussed positive impacts of paid sick leave on employers and employees in San Francisco. Employee use of sick leave as identified below is consistent with the model developed by the Civic Consulting Alliance.

Drago and Lowell, ‘San Francisco’s Paid Sick Leave Ordinance: Outcomes for Employers and Employees’ (2011)

The nation’s first policy allowing all workers to earn and use paid sick days was implemented in San Francisco in 2007. In general, surveys of workers and employers suggest that the law is functioning well. Most employers support the law and relatively few report adverse effects. Among employees, 59,000 or 17 percent of San Francisco’s workforce, worked in firms that offered no paid sick days in the past, but are now covered, and more than half of all San Francisco employees who now have paid sick days report some benefit due to the law. Evidence suggests that it is rare for employees to misuse paid sick days. More education and enforcement may be needed to address remaining instances of employer non-compliance.

This report provides results from recent surveys of 727 employers and 1,194 employees working in San Francisco regarding the effects of the Paid Sick Leave Ordinance (PSLO). For workers, survey results find:

Despite the availability of either five or nine sick days under the PSLO, the typical worker with access used only three paid sick days during the previous year, and one-quarter of employees with access used zero paid sick days.

Rates of utilization well below the caps of five and nine days suggest that employees view paid sick days as a form of insurance—a valuable benefit when illness strikes, but saved until then and only used as needed. For employers, the findings imply that they will never pay for many paid sick days earned under the PSLO [emphasis added only to direct reader’s attention to conclusion].

4. What is the cost of administering a paid sick leave program?

Why does this matter?

Employers have voiced concern about the cost of tracking and administering paid sick leave.

The Working Group reviewed this question in two separate parts:

- A. Survey of Wilmette Businesses
- B. Survey of North Shore Businesses Impacted by the Paid Sick Leave Ordinance

A. Survey of Wilmette Businesses – what does the data say?

Data Type: C (Surveys about current practices or opinions)
Source: Survey of Wilmette Businesses
Details in Appendix: 3

The survey of Wilmette business owners was unable to determine projected costs to administer a paid sick leave benefit. Instead, the survey identified how businesses currently track and administer their payroll, which found the following:

- 55% of businesses administer payroll manually
- 25% utilize a computer program
- 5% outsource
- 15% utilize some other method to administer payroll

Businesses that outsource their payroll function may have to pay an increased fee to track paid time off. However, of the responding businesses, only 5% are currently outsourcing payroll and all of them already provide employees with paid time off.

B. Survey of North Shore Businesses Impacted by the Paid Sick Leave Ordinance – what does the data say?

Data Type: C (Surveys about current practices or opinions)
Source: Survey of North Shore Businesses
Details in Appendix: 10

The survey of North Shore businesses found (8 respondents out of 70 contacted) that three businesses track sick time manually, three use computer software and one outsources this function. The one business that outsources payroll is a professional office with 22 employees. This business provided paid time and outsourced payroll prior to the Cook County ordinance.

5. What might be the impact to Village residents, teachers, students, patrons, etc. of providing paid sick leave to employees?

Why does this matter?

Paid sick leave is generally mandated to improve public health by 1) Enabling an employee who is ill to stay home from work and/or receive medical care; 2) To enable an employee to care for their family members who are ill.

What does the data say?

Data Type: B2 (non-peer reviewed working papers)

Sources:

- DeRigne, Stoddard-Dare, Quinn, “Workers Without Paid Sick Leave Less Likely To Take Time Off for Illness Or Injury Compared To Those With Paid Sick Leave” (2016) (Appendix 17)
- Pichler and Ziebarth, “The Pros and Cons of Sick Pay Schemes” (2015) (Abstract only provided)
- Drago and Lowell, ‘San Francisco’s Paid Sick Leave Ordinance: Outcomes for Employers and Employees” (2011) (Appendix 16)

The Working Group identified three working papers which identified the public health benefits of providing paid sick leave.

DeRigne, Stoddard-Dare, Quinn, “Workers Without Paid Sick Leave Less Likely To Take Time Off for Illness Or Injury Compares To Those With Paid Sick Leave” (2016)

Paid sick leave is an important employer-provided benefit that helps people obtain health care for themselves and their dependents. But paid sick leave is not universally available to US workers. Little is known about paid sick leave and its relationship to health behaviors. ***Contrary to public health goals to reduce the spread of illness, our findings indicate that in 2013 both full- and part-time working adults without paid sick leave were more likely than workers with that benefit to attend work when ill. Those without paid sick leave were 3.0 times more likely to forgo medical care for their family compared to working adults with paid sick leave benefits. Moreover, the lowest-income group of workers without paid sick leave were at the highest risk of delaying and forgoing medical care for themselves and their family members [emphasis added only to direct reader’s attention to conclusion].*** Policy makers should consider the potential public health implications of their decisions when contemplating guaranteed sick leave benefits.

Pichler and Ziebarth, “The Pros and Cons of Sick Pay Schemes” (2015) (Abstract only provided)

This paper proposes a test for the existence and degree of contagious presenteeism and negative externalities in sickness insurance schemes. First, we theoretically decompose moral hazard into shirking and contagious presenteeism behavior and

derive testable conditions. Then, we implement the test exploiting German sick pay reforms and administrative industry-level data on certified sick leave by diagnoses. The labor supply adjustment for contagious diseases is significantly smaller than for non-contagious diseases. ***Lastly, using Google Flu data and the staggered implementation of US sick leave reforms, we show that flu rates decrease after employees gain access to paid sick leave [emphasis added only to direct reader's attention to conclusion].***

Drago and Lowell, "San Francisco's Paid Sick Leave Ordinance: Outcomes for Employers and Employees" (2011)

The findings that many employees benefitted from the PSLO, and were more often able to keep ill children at home, as well as high levels of employer support, imply that the PSLO generated health benefits. [emphasis added only to direct reader's attention to conclusion]. Health care costs for employers and the public should have declined both because sick individuals and their children could get low-cost preventive care, and by reducing the spread of contagious illnesses in workplaces and schools.

6. What might be the impact providing paid sick leave on employees in Wilmette?

Why does this matter?

A stated benefit of providing paid sick leave is that employees can be healthier or help care for their families (e.g., stay home when a child is sick).

The Working Group reviewed this question in two separate parts:

- A. Studies on the Impact of Paid Sick Leave on Employees
- B. Testimony from Low-Wage Workers

A. Studies on the Impact of Paid Sick Leave on Employees – what does the data say?

Data Type: B2 (non-peer reviewed working papers)

Sources:

- Peipines, Soman, Berkowitz, White, “The lack of paid sick leave as a barrier to cancer screening and medical care-seeking: results from the National Health Interview Survey” (2012) (Appendix 18)
- Drago and Lowell, ‘San Francisco’s Paid Sick Leave Ordinance: Outcomes for Employers and Employees” (2011) (Appendix 16)

The Working Group identified two working papers which discussed the benefits of providing paid sick leave to employees.

Peipines, Soman, Berkowitz, White, “The lack of paid sick leave as a barrier to cancer screening and medical care-seeking: results from the National Health Interview Survey” (2012)

Background: Preventive health care services, such as cancer screening can be particularly vulnerable to a lack of paid leave from work since care is not being sought for illness or symptoms. We first describe the prevalence of paid sick leave by broad occupational categories and then examine the association between access to paid sick leave and cancer testing and medical care-seeking in the U.S. workforce.

Methods: Data from the 2008 National Health Interview survey were analyzed by using paid sick leave status and other health-related factors to describe the proportion of U.S. workers undergoing mammography, Pap testing, endoscopy, fecal occult blood test (FOBT), and medical-care seeking.

Results: More than 48 million individuals (38%) in an estimated U.S. working population of 127 million did not have paid sick leave in 2008. The percentage of workers who underwent mammography, Pap test, endoscopy at recommended

intervals, had seen a doctor during the previous 12 months or had at least one visit to a health care provider during the previous 12 months was significantly higher among those with paid sick leave compared with those without sick leave after controlling for sociodemographic and health-care-related factors.

Conclusions: Lack of paid sick leave appears to be a potential barrier to obtaining preventive medical care and is a societal benefit that is potentially amenable to change [emphasis added only to direct reader’s attention to conclusion].

Drago and Lowell, “San Francisco’s Paid Sick Leave Ordinance: Outcomes for Employers and Employees” (2011)

This report provides results from recent surveys of 727 employers and 1,194 employees working in San Francisco regarding the effects of the Paid Sick Leave Ordinance (PSLO). For workers, survey results find:

- Despite the availability of either five or nine sick days under the PSLO, the typical worker with access used only three paid sick days during the previous year, and one-quarter of employees with access used zero paid sick days.
- More than half of San Francisco employees with access reported benefitting from the PSLO either because their employer became more supportive of usage, the number of sick days provided increased, or they were better able to care for themselves or family members.
- Black, Latino, and low-wage workers were those who most often benefitted from the law, but were also those most likely to report employer non-compliance.
- ***Parents with paid sick days were more than 20 percent less likely to send a child with a contagious disease to school than parents who did not have paid sick days. [emphasis added only to direct reader’s attention to conclusion].***

B. Testimony from Low Wage Workers – what does the data say?

Data Type: I (Information)
Source: Arise Chicago- An organization which advocates on behalf of low-wage workers
Details in Appendix: 19

The Working Group initially intended to survey workers in Wilmette to determine the impact paid sick leave would have on them. A Director from Arise Chicago (a low-wage-employee

advocacy organization) was invited to attend the Working Group's February 5, 2018 meeting, at the suggestion of community members. He expressed serious concerns with the planned worker survey. These concerns were based on the difficulty in obtaining accurate responses from employees based on 1) the absence of an existing relationship of trust between the Village and workers; 2) obstacles to communication posed by literacy and language issues; and 3) the possibility that perceived employer pressure might skew survey results. In addition, the Working Group was concerned about the practical difficulties of accessing workers without employer participation. Arise Chicago instead suggested referring to testimonials from low wage workers given in other forums. Accordingly, the Working Group determined that a survey of employees in Wilmette would not be conducted and instead previous worker testimonials would be included in this report. The worker testimonials speak to the importance of paid sick leave in caring for family members and to prevent employees from going to work while sick. The testimonials can be found in Appendix 19.

7. What might be the impact of providing paid sick leave on businesses in Wilmette?

Why does this matter?

As identified in Questions V2 and V3, certain employers not currently providing paid time off may incur additional costs in offering paid sick leave- the cost of the benefit itself and the cost of tracking eligibility. On the other hand, certain working papers show that employers benefit through cost savings from workforce stability, increased productivity, disease and illness prevention, and lower health care costs.

Data Type: B2 (non-peer reviewed working papers)

Sources:

- DeRigne, Stoddard-Dare, Quinn, “Workers Without Paid Sick Leave Less Likely To Take Time Off for Illness Or Injury Compares To Those With Paid Sick Leave” (2016) (Appendix 17)
- Drago and Lowell, ‘San Francisco’s Paid Sick Leave Ordinance: Outcomes for Employers and Employees” (2011) (Appendix 16)

The Working Group identified two working papers which found that workers without paid sick leave were more likely to attend work ill, and employer profitability did not suffer as a result of providing paid sick leave (based on a survey of San Francisco businesses).

DeRigne, Stoddard, Dare, Quinn, “Workers Without Paid Sick Leave Less Likely To Take Time Off for Illness Or Injury Compares To Those With Paid Sick Leave” (2016)

Paid sick leave is an important employer-provided benefit that helps people obtain health care for themselves and their dependents. But paid sick leave is not universally available to US workers. Little is known about paid sick leave and its relationship to health behaviors. ***Contrary to public health goals to reduce the spread of illness, our findings indicate that in 2013 both full- and part-time working adults without paid sick leave were more likely than workers with that benefit to attend work when ill. Those without paid sick leave were 3.0 times more likely to forgo medical care for their family compared to working adults with paid sick leave benefits. Moreover, the lowest-income group of workers without paid sick leave were at the highest risk of delaying and forgoing medical care for themselves and their family members [emphasis added only to direct reader’s attention to conclusion].*** Policy makers should consider the potential public health implications of their decisions when contemplating guaranteed sick leave benefits.

Drago and Lowell, 'San Francisco's Paid Sick Leave Ordinance: Outcomes for Employers and Employees' (2011)

Survey results find for employers:

- ***Employer profitability did not suffer. Six out of seven employers did not report any negative effect on profitability as a result of the PSLO [emphasis added only to direct reader's attention to conclusion].***
- Most employers reported no difficulty providing sick days to their employees under the ordinance. Approximately one-third of employers reported any difficulties implementing the PSLO, and only one-sixth needed to introduce an entirely new paid sick days policy because of the law. However, some employers (also around one-sixth) are in violation of the law and still did not offer paid sick days at the time of the survey.
- Employers are supportive. Two-thirds of employers support the PSLO and one-third are "very supportive."

8. What might be the impact of providing paid sick leave on Wilmette as a community?

Why does this matter?

There are a number of possible impacts to the community of providing or not providing paid sick leave. These may include but are not limited to: 1) Requiring paid sick leave could lead to increased prices for goods and services, which might discourage customers from patronizing Wilmette businesses; 2) Not requiring businesses to provide paid sick leave might diminish Wilmette's image as a thriving community that values the welfare of its workers; 3) Requiring paid sick leave might diminish the perception of Wilmette as a good place to do business and thus might adversely affect the Village's future economic development efforts and ability to recruit new businesses; 4) Requiring businesses to provide paid sick leave was supported by a majority of Wilmette residents in a 2016 statewide advisory referendum, although the benefit presented in the referendum is different than that contained in the County's paid sick leave ordinance.

The Working Group reviewed this question in three separate parts:

- A. Survey of Wilmette Residents
- B. Survey of North Shore Brokers
- C. Feedback from the Wilmette Faith Community

A. Survey of Wilmette Residents – what does the data say?

Data Type: C (Surveys about current practices or opinions)

Source: Fallon Research & Communications, Inc.

Details in Appendix: 12

The Working Group commissioned a phone survey of 300⁶ Wilmette residents conducted by Fallon Research & Communications, Inc.. This survey research data was gathered through telephone interviews that specially-trained interviewers conducted with 303 randomly-selected adult residents of the Village of Wilmette, Illinois, who had valid residential, VOIP or cellular telephone numbers. The interviews were performed during the period of April 11, 2018 through April 13, 2018. The overall estimated margin of sampling error is +/- 5.62%, based on a confidence level of 95%, although it varies for each individual question. This means that if this survey were repeated, 95 times out of 100 the results would be within plus or minus 5.62% of those provided herein. Some adjustments were made to weight the results toward demographic and geographic characteristics of the village's adult population, in order to account for under- and over-sampling that normally occurs as a result of the random selection process, and to ensure that all major sub-groups are represented in proportion to their actual

⁶ Although only 300 interviews were targeted by the Village, 303 were actually completed because simultaneous interviews were conducted.

percentages. Like all polls, this opinion survey research is subject to other possible sources of error, such as unintentional bias in the wording of questions, data-entry error and nonresponse bias.

While there were a total of 12 questions, the questions directly related to Paid Sick Leave are as follows:

Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?

67% Should follow
24.6 Should not
8.4 Unsure/no answer

Q. 8. Does knowing that the Village has chosen not to follow the Cook County ordinances, so businesses in Wilmette are not required to increase the minimum wage or offer paid sick leave to employees, make your opinion of the Village of Wilmette more favorable, less favorable or does it not affect your views?

15.2% More favorable
44.4 Less favorable
36.5 No effect
3.9 Unsure/no answer

Q. 10. Since research shows that, in order to avoid losing wages, nearly half of food service employees who are ill come to work sick if they don't have paid sick leave, would you be more or less likely to patronize Wilmette restaurants if local restaurants provide paid sick leave or does it have no effect on your decision?

32.3% More likely
8.4 Less likely
55.6 No effect
3.7 Unsure/no answer

Appendix 12 includes the following data and information: 1) General survey results; 2) Cross-tabulated survey results; 3) Report on the investigation of a resident survey incident.

B. Survey of North Shore Brokers and Businesses – what does the data say?

Data Type: C (Surveys about current practices or opinions)

Source: Phone Interviews Conducted by Village of Wilmette Staff (North Shore Brokers)

Details in Appendix: 10

The Minimum Wage & Paid Sick Leave Working Group asked Village staff to interview brokers to help understand the effects of the Cook County Ordinances. Questions for the interviews were developed by the Working Group. Three commercial real estate brokers were interviewed. Each of them has listings throughout the North Shore with a good understanding of local transactions. None of the brokers had heard the issue raised when discussing locations with clients. They stated the most important factors tenants look for when selecting a location were:

- Price of rent
- Physical location
- Access to parking

The three brokers did not think the existence of the Cook County ordinances would have an impact in attracting or dissuading a tenant from locating in a particular community.

C. Feedback from the Wilmette Faith Community – what does the data say?

Data Type: I (Information)

Source: Letters from Members of the Wilmette Clergy

Details in Appendix: 13

At the suggestion of a Village resident, the Working Group solicited feedback from Wilmette Clergy. A total of three Wilmette clergy members submitted written responses, all of whom supported paid sick leave. A copy of the clergy responses can be found in the appendices.

VI. LIST OF APPENDICES

Number	Appendix
1	Summary of Cook County Ordinances
2	Illinois Department of Employment Security (IDES) Low Wage Worker Data
3	Survey Results of Wilmette Businesses
4	Belman, Wolfson and Nawakitphaitoon, “Who is Affected by the Minimum Wage” (2015)
5	Congressional Budget Office, “The Effects of a Minimum-Wage Increase on Employment and Family Income” (2014)
6	Schmitt, “Why Does the Minimum Wage Have No Discernible Effect on Employment” (2013)
7	Jardim, Long, Plotnick, Inwegen, Vigdor, Wething, “Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence From Seattle (2017)
8	Reich, Allegretto, and Godeoy, “Seattle’s Minimum Wage Experience 2015-16” (2017)
9	Luca and Luca, “Survival of the Fittest: The Impact of the Minimum Wage on Firm Exit” (2017)
10	Memorandum from the Village’s Business Development Coordinator <ul style="list-style-type: none"> • Survey of North Shore Municipalities • Survey of North Shore Businesses • Survey of North Shore Brokers • Letter from the Village of Winnetka
11	Memorandum from Village resident Gina Kennedy on the Impacts of Federal Tax Law Changes
12	Resident Survey Results
13	Letters from Wilmette Clergy (3)
14	Illinois Department of Employment Security (IDES) All Workers Data
15	Paid Sick Leave Cost Model Developed by the Civic Consulting Alliance
16	Drago and Lowell, “San Francisco’s Paid Sick Leave Ordinance: Outcomes for Employers and Employees” (2011)
17	DeRigne, Stoddard-Dare, Quinn, “Workers Without Paid Sick Leave Less Likely to Take Time Off for Illness or Injury Compared to those with Paid Sick Leave” (2016)
18	Peipines, Soman, Berkowitz, White, “The lack of paid sick leave as a barrier to cancer screening and medical care-seeking: results from the National Health Interview Survey” (2012)
19	Testimonials from Low Wage Workers Provided by Arise Chicago

Cook County Ordinances on Minimum Wage and Mandatory Paid Sick Leave
(April 5, 2018)

	<u>Minimum Wage</u>	<u>Mandatory Paid Sick Leave</u>
Definition of Employer	Any entity with any place of business in Cook County, or licensed by Cook County, with at least 4 employees one of which is a “Covered Employee” or just 1 “Domestic Worker” [*] [*] Based on Cook County’s Administrative Rules	Any entity with any place of business in Cook County with 1 or more “Covered Employees”
Definition of Covered Employee	Works 2 hours in any 2 week period	Works 2 hours in any 2 week period
Jurisdiction	Employees who perform work in covered portions of Cook County, regardless of the location of their business office, may be entitled to the County Minimum Wage only for that work	Employees who perform work in covered portions of Cook County, regardless of the location of their business office, may be entitled to the County Sick Leave.
Applies to persons under 18	No	Yes
Applies to seasonal/temporary employees	No, for up to the first 90 days of employment (yes on the 91 st calendar day of employment)	No, as a practical matter, if the employer restricts accrual of sick leave benefit time for the first 180 days of employment. After the first 180 days, the employee is eligible for sick leave benefits
Applies to Independent Contractors	No [*] [*] Per the County’s administrative rules whether someone is an employee or independent contractor depends on the application of a multi-factor, fact intensive legal test.	No [*] [*] Per the County’s administrative rules whether someone is an employee or independent contractor depends on the application of a multi-factor, fact intensive legal test.
Gov’t Exemptions	All Units of Government other than Cook County	All Units of Government including Cook County

This matrix was created by the Village of Wilmette utilizing the text of the County Ordinances, County’s administrative rules, and the County FAQs. This matrix is not meant to provide legal advice or is to be considered a legal authority on this matter. The interpretation and enforcement of the Cook County Ordinances is provided by the Cook County Commission on Human Rights which may provide differing opinions than what is contained within these materials.

	<u>Minimum Wage</u>	<u>Mandatory Paid Sick Leave</u>
Other Exemptions	Employees of religious organizations, certain persons with disabilities for which the employer has received authorization from the State of Illinois, individuals in a Subsidized Transitional Program, individuals in a Subsidized Temporary Youth Employment Program	None
Collective Bargaining Agreements	Does not apply to employees covered under Collective Bargaining Agreements (“CBA”) entered into or on before July 1, 2017 and for those CBAs that have waived the ordinance requirements.	Does not apply to employees covered under Collective Bargaining Agreements (“CBA”) entered into or on before July 1, 2017 and for those CBAs that have waived the ordinance requirements.
Current Illinois Law	Non – Tipped Employees: \$8.25/hr. Tipped Employees: Employer must pay a base wage of \$4.95/hr. and make up any shortfall in tips that would result in the employee making less than \$8.25/hr.	No paid sick leave benefits
Benefits provided by Cook County Ordinances	Non – Tipped Employees: July 1, 2017 \$10.00/hr. July 1, 2018 \$11.00/hr. July 1, 2019 \$12.00/hr. July 1, 2020 \$13.00/hr. July 1, 2021 and future: \$13/hr., annual CPI adjustment	Accrual Rate – 1 hour sick leave per 40 hours of work Max. Yearly Accrual – 40 hours per 12 month period Max. Carryover – 20 hours to next 12 month period Total Max. Accrual – 60 hours
Benefits for Tipped Employees	Same as current law, adjusted annually for inflation beginning July 1, 2018	Same as all other covered employees

Work Area Profile Report

Workers Earning \$1250 per month or less

Total Private Primary Jobs

	2015	
	Count	Share
Total Private Primary Jobs	1,347	100.0%

Jobs by Worker Age

	2015	
	Count	Share
Age 29 or younger	628	46.6%
Age 30 to 54	402	29.8%
Age 55 or older	317	23.5%

Jobs by Earnings

	2015	
	Count	Share
\$1,250 per month or less	1,347	100.0%
\$1,251 to \$3,333 per month	0	0.0%
More than \$3,333 per month	0	0.0%

Jobs by NAICS Industry Sector

	2015	
	Count	Share
Agriculture, Forestry, Fishing and Hunting	2	0.1%
Mining, Quarrying, and Oil and Gas Extraction	0	0.0%
Utilities	0	0.0%
Construction	14	1.0%
Manufacturing	9	0.7%
Wholesale Trade	9	0.7%
Retail Trade	460	34.1%
Transportation and Warehousing	1	0.1%
Information	3	0.2%
Finance and Insurance	55	4.1%
Real Estate and Rental and Leasing	46	3.4%
Professional, Scientific, and Technical Services	59	4.4%
Management of Companies and Enterprises	0	0.0%
Administration & Support, Waste Management and Remediation	22	1.6%
Educational Services	54	4.0%
Health Care and Social Assistance	133	9.9%
Arts, Entertainment, and Recreation	40	3.0%
Accommodation and Food Services	298	22.1%
Other Services (excluding Public Administration)	142	10.5%
Public Administration	0	0.0%

Jobs by Worker Race

	2015	
	Count	Share
White Alone	1,042	77.4%
Black or African American Alone	158	11.7%
American Indian or Alaska Native Alone	5	0.4%
Asian Alone	118	8.8%
Native Hawaiian or Other Pacific Islander Alone	3	0.2%
Two or More Race Groups	21	1.6%

Jobs by Worker Ethnicity

	2015	
	Count	Share
Not Hispanic or Latino	1,181	87.7%
Hispanic or Latino	166	12.3%

Jobs by Worker Educational Attainment

	2015	
	Count	Share
Less than high school	102	7.6%
High school or equivalent, no college	171	12.7%
Some college or Associate degree	225	16.7%
Bachelor's degree or advanced degree	221	16.4%
Educational attainment not available (workers aged 29 or younger)	628	46.6%

Jobs by Worker Sex

	2015	
	Count	Share
Male	514	38.2%
Female	833	61.8%



The Village of Wilmette

Minimum Wage and Paid Sick Leave Survey Results

Executive Summary

April 2018

Summary

- ▶ Surveys were sent via email and mail to 558 businesses, of which 254 responded
- ▶ Of the 254 responses, 32 were removed due to not answering any questions beyond the business demographic information, resulting in an overall response rate of 39%
- ▶ Data is presented as follows:
 - Executive Summary
 - General Minimum Wage and Paid Sick Leave Summaries
 - Minimum Wage Summary Excluding Responding Businesses with Less Than 4 Employees
 - Sector Specific Summaries
 - Retail
 - Restaurant
 - Service

Employment Status in an Organization – Insights



- Most responding businesses are from service (25%), retail (24%), professional (15%), and healthcare (15%) sectors
- 53% of responding businesses have been in operation for over 20 years in Wilmette
- 41% of responding businesses have less than 4 employees
- 6% of the responding businesses have 4 or more employees under the age of 18

Minimum Wage – Insights



- 70% of full-time and tipped employees are paid an hourly wage of more than \$13
- 55% of part-time employees earn an hourly wage below \$13
- 48% of responding businesses stated that the lowest starting hourly rate for full-time employees is between \$10.00 – \$14.99
- 54% of responding businesses oppose the increased minimum wage rates and 44% are in favor

Paid Sick Leave – Insights



- 74% of full-time employees receive some form of paid time off
- 65% of part-time employees do not receive any paid time off
- Of those receiving paid time-off, 96% of full-time and 77% of part-time employees have at least 5 days of paid leave per year
- 57% of responding businesses oppose paid-sick-leave regulations and 34% are in favor

Retail Business Insights



Businesses and Employee Tenure

- 62% of retail businesses have been in operation for more than 20 years
- The average tenure of the employees is evenly spread between 1 to 20 years



Number of Employees

- 31% of the businesses have less than 4 employees
- 67% of retail businesses have 10 or less employees
- 16% of businesses have 4 or more employees working less than 90 days in a consecutive year
- 10% of the businesses have 4 or more employees under the age of 18 years

Minimum Wage Insights



- 73% of the full-time retail employees are paid more than \$13/hour
- 56% of part-time retail employees are paid less than \$12/hour
- 63% of the full-time employees earn a starting hourly wage between \$10.00 - \$ 14.99
- 70% retail businesses oppose increasing the minimum wage

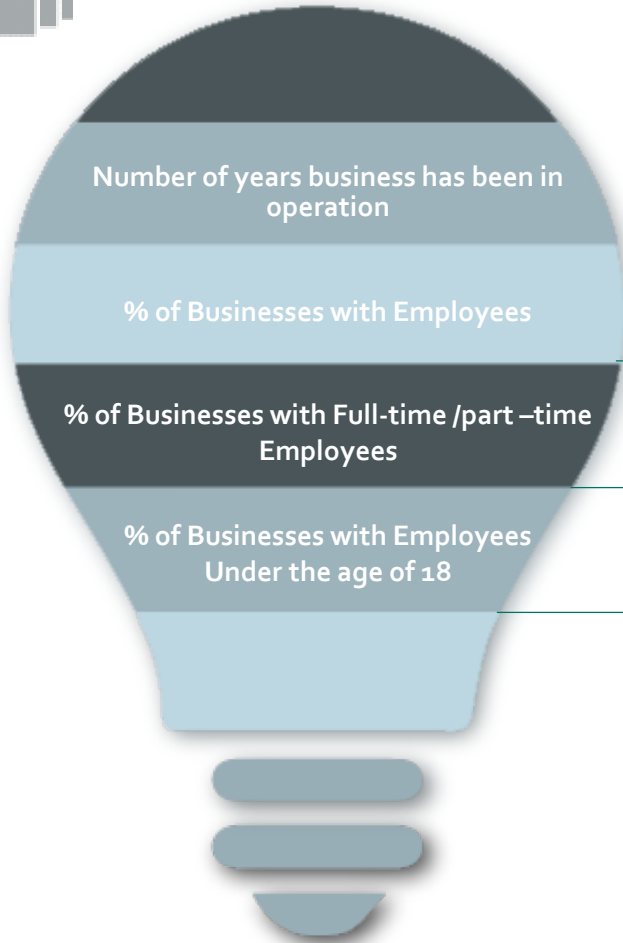


Paid Sick Leave Insights

- 84% of full-time retail employees receive paid time off
- 60% of the part-time retail employees do not receive any paid time off
- 68% retail businesses oppose regulations pertaining to paid sick leave



Service Business Insights



Employment Status in an Organization

- 51% of service businesses have been in operation for more than 20 years
- 46% of responding businesses have less than 4 employees
- 63% of the responding businesses have less than 4 full-time employees while 88% have less than 4 part-time employees
- 96% of the responding businesses have less than 4 employees under the age of 18 years

Minimum Wage/Paid Sick Leave Insights



- 73% of full-time service employees receive an hourly rate of \$13 or more
- 62% of part-time service employees receive an hourly rate of \$13 or more
- 53% of responding businesses oppose the minimum wage rates and 42% are in favor
- 64% of responding businesses oppose regulations pertaining to paid sick leave and 20% are in favor

Business Level Insights



Professional

- 53% of the responding businesses have been in operation for more than 20 years
- 66% of the responding businesses have less than 4 employees
- 60% of the part-time employees are not provided with any paid-off
- 56% of the responding businesses support the minimum wage rate ordinance
- There is an even split of support and opposition to the paid sick leave regulations

Non-Profit

- 79% of the responding businesses have been in operation for more than 20 years
- 92% of the non-profit businesses have less than 4 employees under the age of 18
 - 64% support the minimum wage rate ordinance
 - 64% support the paid sick leave regulations



Healthcare

- 41% of the healthcare businesses have been operational in past 20 years
- 72% of the responding healthcare business do not provide any paid-off to the part-time employees
- 47% of the responding businesses support the minimum wage rate ordinance
- 55% of the responding businesses oppose the paid sick leave regulations

Health care



Restaurant

Restaurant

- 60% of the restaurant businesses have been operational for past 1-10 years
- 61% of full-time employees receive less than \$13 per hour
- 82% of part-time employees receive less than \$13 per hour
- 70% of the responding businesses do not support the minimum wage rate ordinance
- 75% of the responding business do not support the paid sick leave regulations

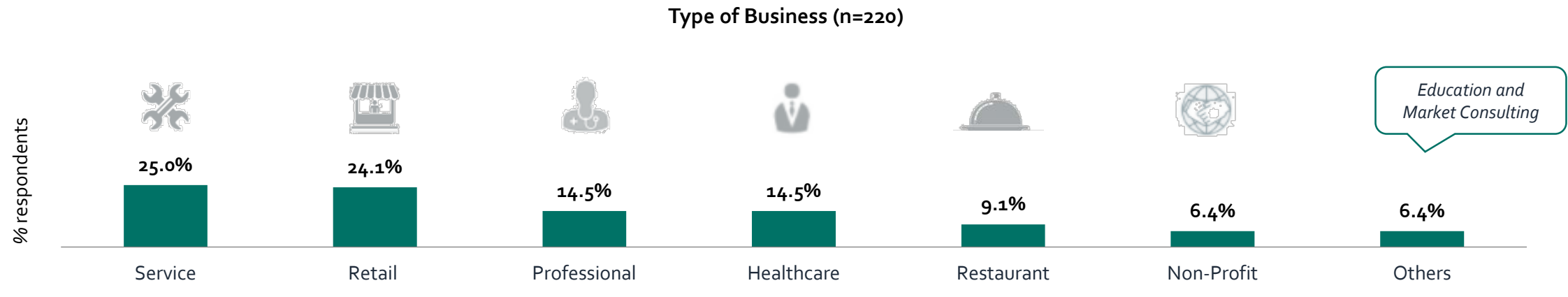




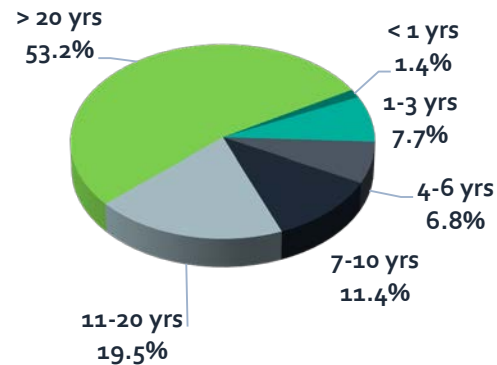
General Minimum Wage and Paid Sick Leave Summaries

General Questions (1/2)

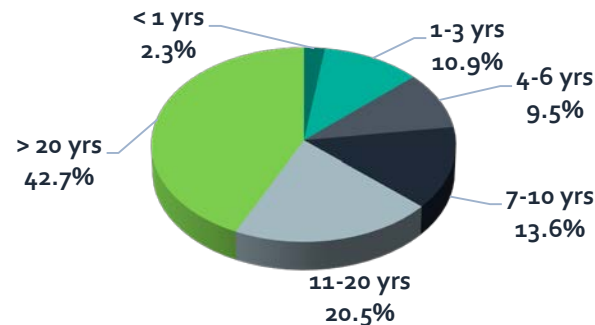
49% of businesses, which participated in the survey, were from service and retail sectors; 54% have been operating their business for more than 20 years



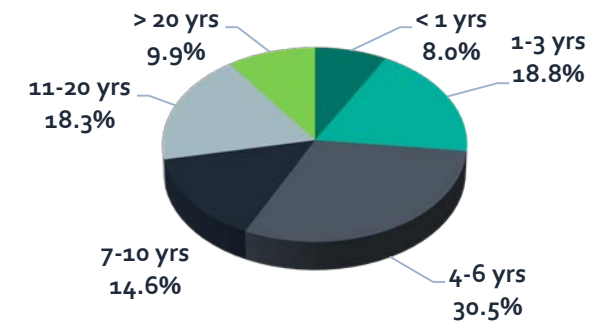
Number of Years Business Has Been Operational (n=220)



Number of Years Business Has Been Operational in Wilmette (n=219)



Average Tenure of Employees (n=213)



42% of responding businesses have less than 4 employees
91% have less than 4 employees under the age of 18



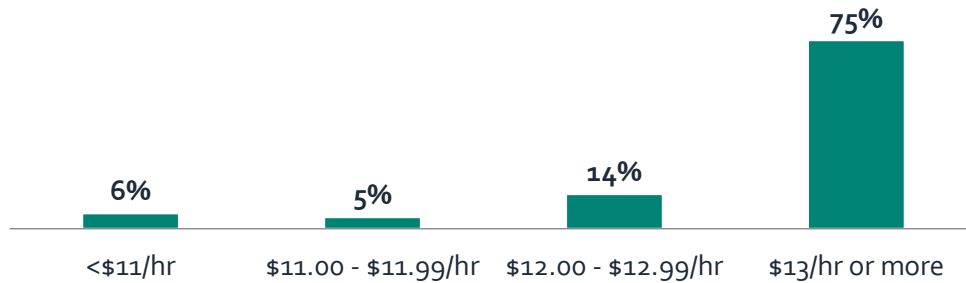
Employee Status in an Organizations

Employee Group	% of Responding Businesses with Number of Employees				
	< 4	4-10	11-25	26-40	> 40
Total Employees (n=220)	41%	29%	18%	5%	7%
Full-time Employees (n=216)	54%	25%	13%	4%	4%
Part-Time Employees (n=212)	71%	18%	8%	1%	2%
Employees Working Less than 90 Days in a Consecutive Year (n=207)	85%	10%	5%	0%	0%
Employees Under the age of 18 (n=200)	94%	4%	1%	0%	1%
Tipped Employees (n=197)	87%	7%	4%	1%	1%

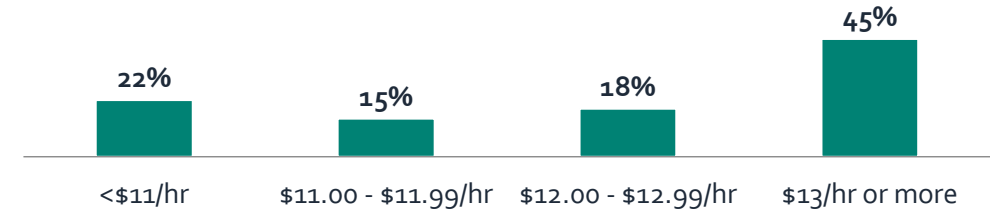
n=responding businesses

63% of employees are paid an hourly wage of more than \$13

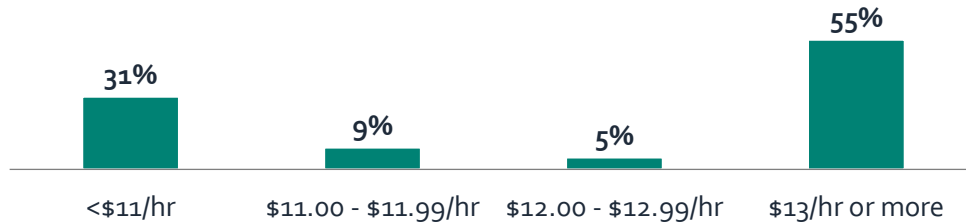
% of Full – time Employees Paid* (n=164)



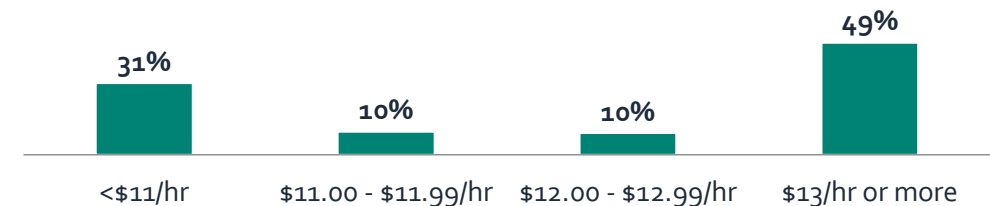
% of Part – time Employees Paid* (n=132)



% of Tipped Employees Paid* (n=24) ⚠️



Lowest Starting Hourly Rate for Full-time Employees (n=186)



⚠️ Low Sample Size

54% of responding businesses oppose the increased minimum wage and 42% support the increase while 2% did not answer (n=220)



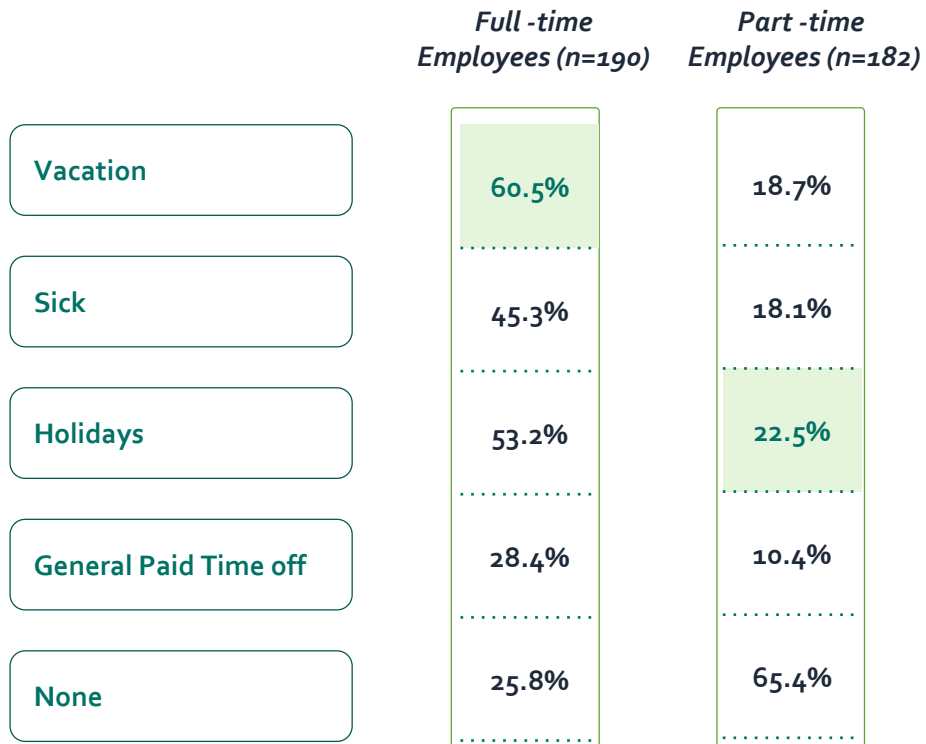
Type of Business (n=220)	No (n=120)	Yes (n=96)	No Response (n=4)
Healthcare (n=32)	50.0%	46.9%	3.1%
Non-profit (n=14)	35.7%	64.3%	0.0%
Professional (n=32)	43.8%	56.3%	0.0%
Restaurant (n=20)	70.0%	25.0%	5.0%
Retail (n=53)	69.8%	30.2%	0.0%
Service (n=55)	52.7%	41.8%	5.5%
Others (n=14)	42.9%	57.1%	0.0%

Reasons for Supporting Minimum Wage Rates	Rank
Fair, living wage	1
Value for workers	2
Assurance for workers	3
Better retention	4
Attract hourly paid workers	5
Equality	6

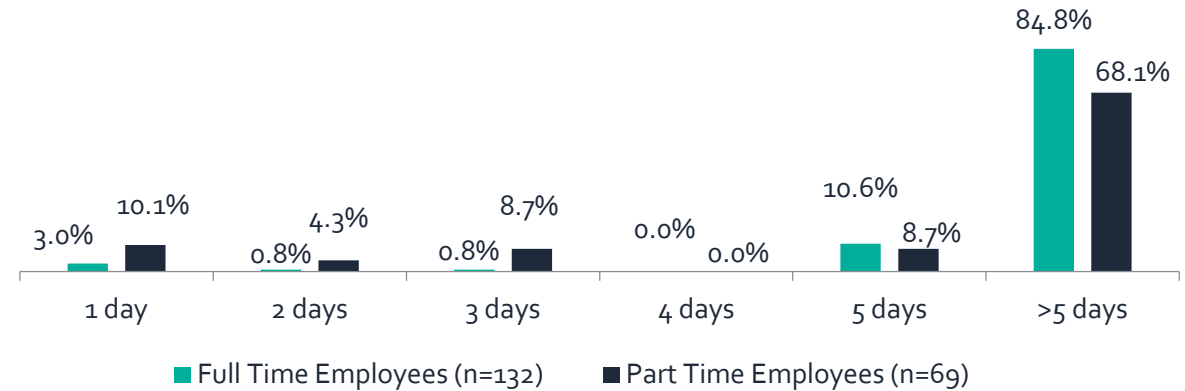
Reasons for Not Supporting Minimum Wage Rates	Rank
Decision of market/type of work	1
Increase in payroll for employer	2
Flexibility requirement (to reduce cost) for small businesses	3
Decrease in employment (particularly unskilled)	4
Services become expensive	5
Reluctance to pay teenagers more	6

74% of full-time employees have some form of paid leave, and 95% of these employees have at least 5 days of paid leave per year while 65% of part-time employees do not receive any paid leave

Paid Time – off



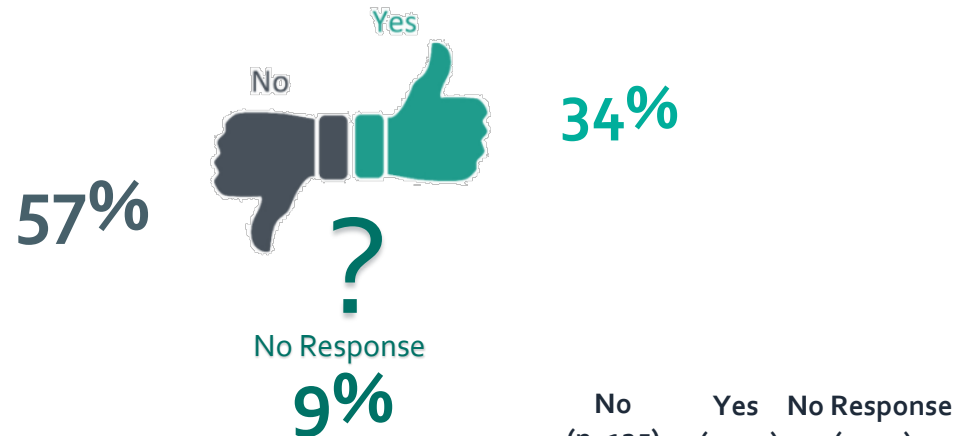
Number of Days Earned Per Year



Track and Administer Paid Time – off (n=168)

Manually	54.4%
Computer Program	25.4%
Outsourced	4.7%
Others	15.4%

57% of responding businesses oppose the paid-sick leave regulations, 34% support, while 9% did not answer



Type of Business (n=220)	No (n=125)	Yes (n=74)	No Response (n=21)
Healthcare (n=32)	53.1%	40.6%	6.3%
Non-profit (n=14)	21.4%	64.3%	14.3%
Professional (n=32)	46.9%	46.9%	6.3%
Restaurant (n=20)	75.0%	25.0%	0.0%
Retail (n=53)	67.9%	26.4%	5.7%
Service (n=55)	63.3%	20.0%	16.4%
Others (n=14)	28.6%	50.0%	21.4%

Reasons for Supporting Paid Sick Leave Regulations	Rank
Fair and reasonable	1
Vacation/sick leave deserved	2
Sick workers not fit for workplace	3
Performance deteriorates when working sick	4

Reasons for Not Supporting Paid Sick Leave Regulations	Rank
Determined by market/business	1
Increases cost	2
Abuse of facility	3
Too many hours offered	4
Unnecessary for part-time/under 18 employees	5
Hard to maintain records	6



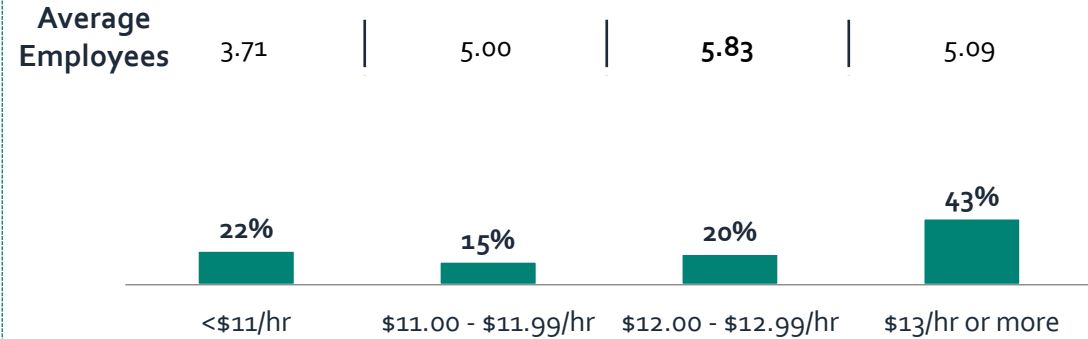
Minimum Wage Survey Results for Responding Business with 4 or More Employees

63% of full and part-time employees are paid an hourly wage of more than \$13

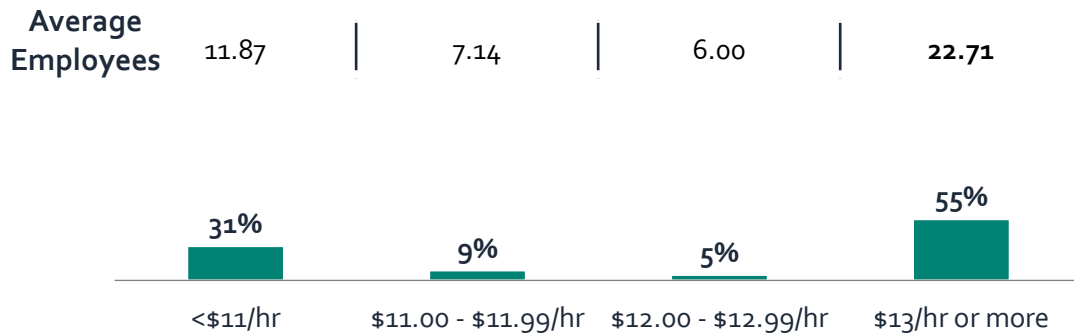
% of Full – time Employees Paid* (n=107)



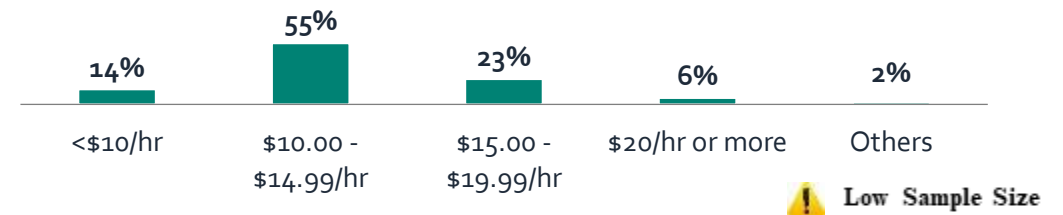
% of Part – time Employees Paid* (n=92)



% of Tipped Employees Paid* (n=25) ⚠️



Lowest Starting Hourly Rate for Full-time Employees (n=118)



61% of responding businesses oppose the increased minimum wage, while 37% support the increase

Acceptance of Minimum Wage Rates (n=128)



Type of Business (n=128)*	No (n=78)	Yes (n=47)	No Response (n=3)
Healthcare (n=16)	75.0%	25.0%	0.0%
Non-profit (n=9)	33.3%	66.7%	0.0%
Professional (n=11)	54.5%	45.5%	0.0%
Restaurant (n=19)	73.7%	21.1%	5.3%
Retail (n=36)	66.7%	33.3%	0.0%
Service (n=29)	55.2%	37.9%	6.9%
Others (n=8)	37.5%	62.5%	0.0%

Reasons for Accepting Minimum Wage Rates (n=49)	Rank
Fair, living wage	1
Value for workers	2
Better retention	3
Assurance for workers	4
Attract hourly paid workers	5
Equality	6

Reasons for Not Accepting Minimum Wage Rates (n=79)	Rank
Decision of market/type of work	1
Increased expenses for employers	2
Flexibility requirement for small businesses	3
Services become expensive	4
Decrease in employment (particularly unskilled)	5
Reluctance to pay teenagers more	6



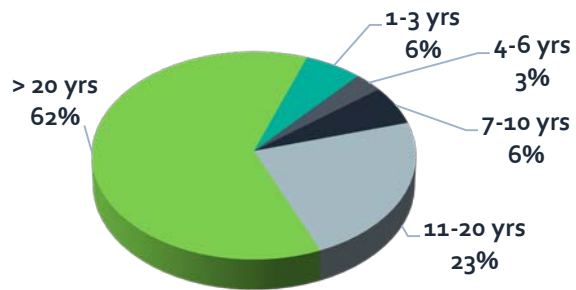
Retail Sector

62% of responding retail businesses have been operational for more than 20 years with average employee tenure fairly evenly spread between 1 and 20 years

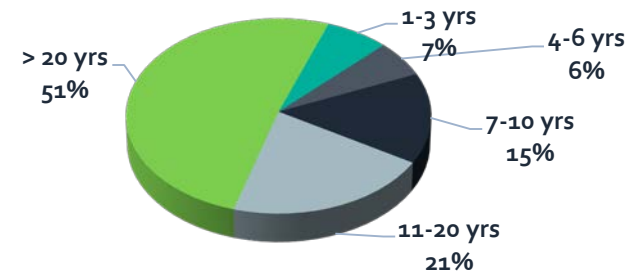


Retail

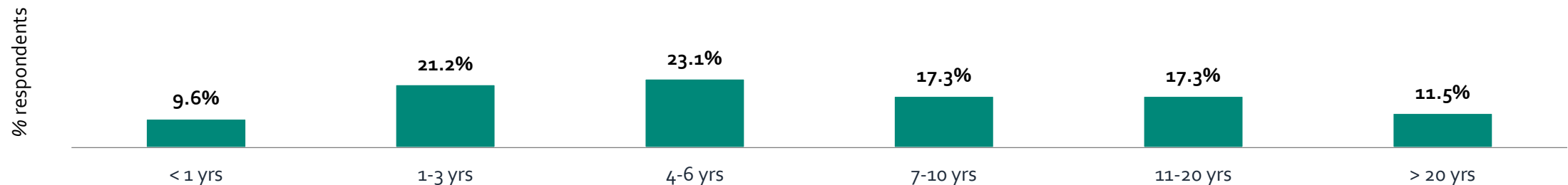
Number of Years Business Has Been Operational (n=53)



Number of Years Business Has Been Operational in Wilmette (n=53)



Average Tenure of Employees (n=52)



31% of responding retail businesses have less than 4 employees, while 90% of the businesses have less than 4 employees under the age of 18 years



Retail



Employee Status in an Organizations

Employee Group	% of Responding Businesses with Number of Employees				
	< 4	4–10	11–25	26–40	> 40
Total Employees (n=52)	31%	37%	17%	5%	10%
Full-time Employees (n=52)	44%	35%	13%	6%	2%
Part-Time Employees (n=52)	59%	29%	8%	2%	2%
Employees Working Less than 90 Days in a Consecutive Year (n=51)	84%	10%	6%	0%	0%
Employees Under the age of 18 (n=48)	90%	6%	4%	0%	0%
Tipped Employees (n=47)	83%	9%	4%	2%	2%

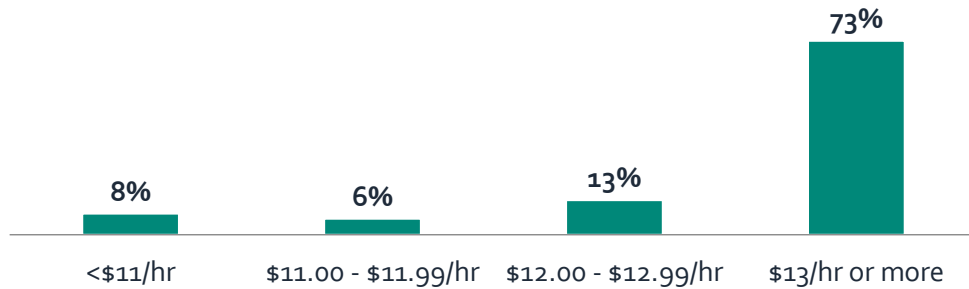
n=responding businesses

73% of the full-time retail employees are paid more than \$13/hour
72% of part-time retail employees are paid less than \$13/hour

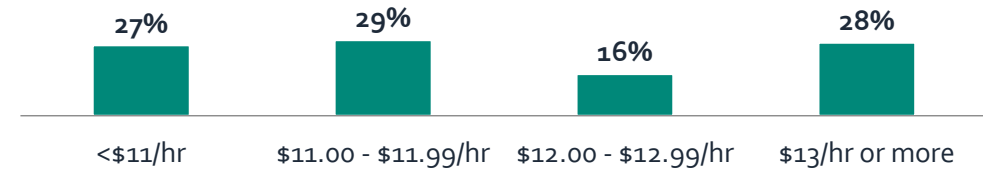


Retail

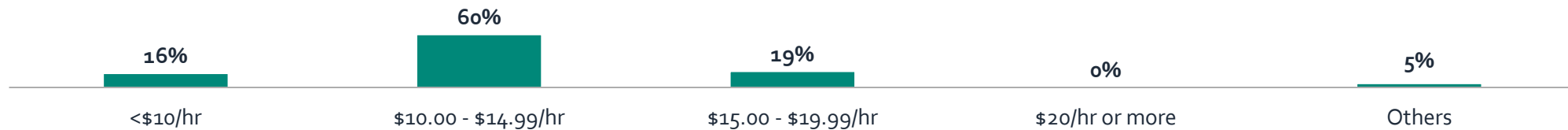
% of Full – time Employees Paid* (n=39)



% of Part – time Employees Paid* (n=32)



Lowest Starting Hourly Rate for Full-time Employee (n=43)

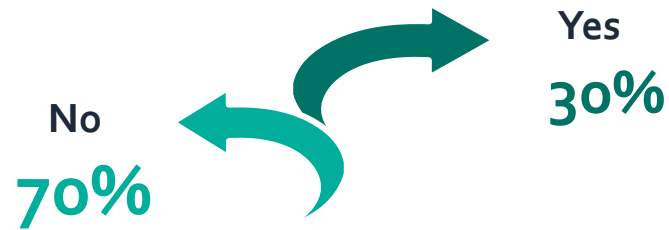


70% of responding retail businesses oppose the higher minimum wage rates



Retail

Acceptance of Minimum Wage Rates (n=53)



Reasons for Not supporting Minimum Wage Rates	Rank
Increase in payroll for employer	1
Flexibility requirement (to reduce cost) for small businesses	2
Decision of market/type of work	3
Decrease in employment (particularly unskilled)	4
Services become expensive	5

Reasons for supporting Minimum Wage Rates	Rank
Fair, living wage	1
Reasonable	2
Assurance for workers	2
Attract hourly paid workers	2
Value for workers	2

83% of full-time retail employees receive paid time off while 60% of part-time retail employees do not receive any paid time off

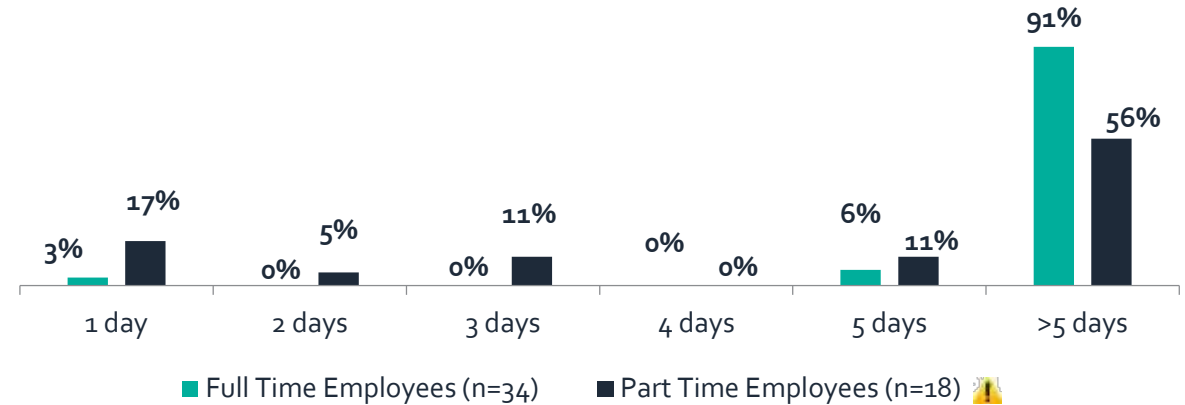


Retail

Paid Time-off

	Full-time Employees (n=45)	Part-time Employees (n=45)
Vacation	67%	22%
Sick	44%	13%
Holidays	51%	18%
General Paid Time Off	33%	18%
None	16%	60%

Number of Days Earned Per Year



Track and Administer Paid Time-off (n=42)

Manually	53%
Computer Program	26%
Outsourced	7%
Others	14%

Low Sample Size

68% of responding retail businesses opposed the paid sick leave regulations



Retail

Acceptance of Paid Sick Leave Regulations (n=53)



Reasons for supporting Paid Sick Leave Regulations	Rank
Fair and reasonable	1
Sick workers not fit for workplace	2
Performance deteriorates when working sick	2



Reasons for Not supporting Paid Sick Leave Regulations (n=36)	Rank
Determined by market/business	1
Increases cost	1
Abuse of facility	2
Too many hours	2
Unnecessary for part-time/under 18 employees	3



Restaurant Sector

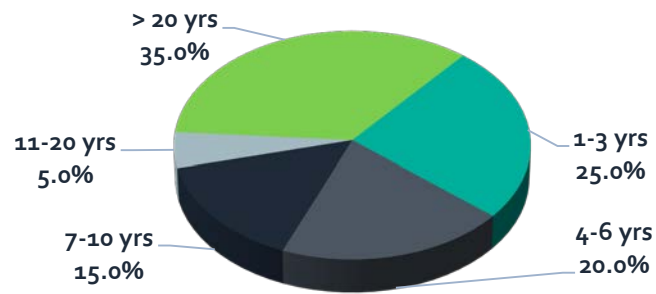
General Questions (1/2)

60% of responding restaurant businesses have been operational for past 1–10 years; 55% of them have an average employee tenure of 1–3 years

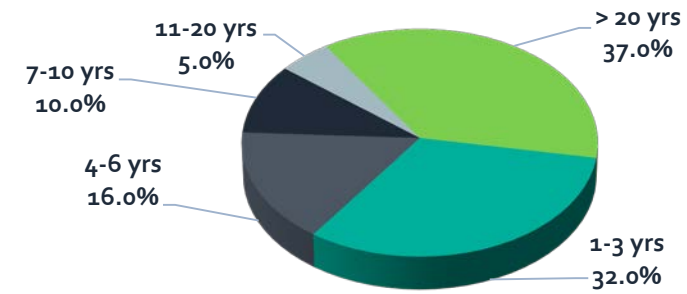


Restaurant

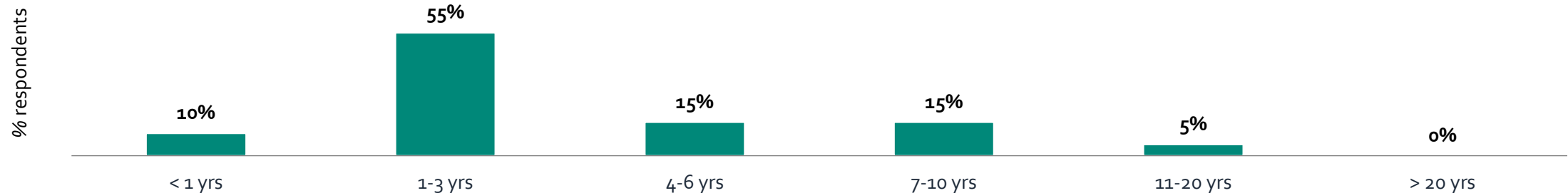
Number of Years Business Has Been Operational (n=20) ⚠️



Number of Years Business Has Been Operational in Wilmette (n=19) ⚠️



Average Tenure of Employees (n=20) ⚠️



⚠️ Low Sample Size

70% of responding restaurants have more than 11 employees



Employee Status in an Organization

Employee Group	% of Responding Businesses with Number of Employees				
	< 4	4-10	11-25	26-40	> 40
Total Employees (n=20)	5%	25%	45%	20%	5%
Full-time Employees (n=20)	25%	35%	35%	5%	0%
Part-Time Employees (n=20)	35%	20%	40%	0%	5%
Employees Working Less than 90 Days in a Consecutive Year (n=20)	55%	25%	15%	5%	0.0%
Employees Under the age of 18 (n=20)	75%	25%	0%	0%	0%
Tipped Employees (n=20)	35%	25%	35%	5%	0%

n=responding businesses

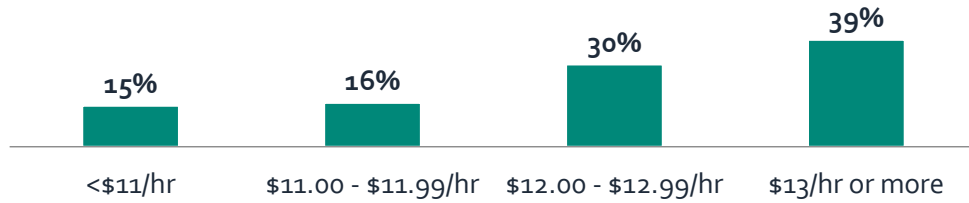
Low Sample Size

A wide range of an hourly wage is observed for full-time, part-time, and tipped employees employed in a restaurant

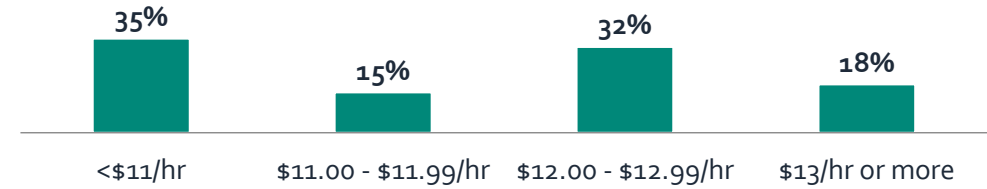


Restaurant

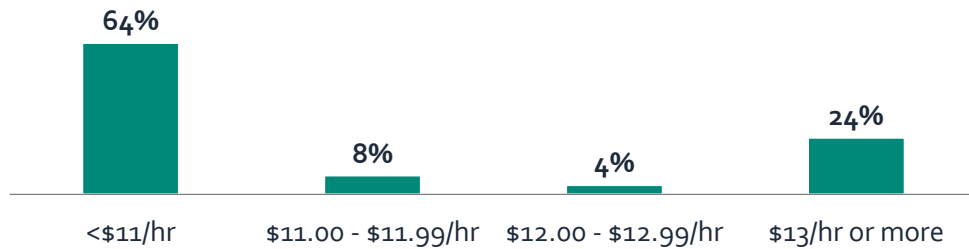
% of Full – time Employees Paid* (n=16) ⚠️



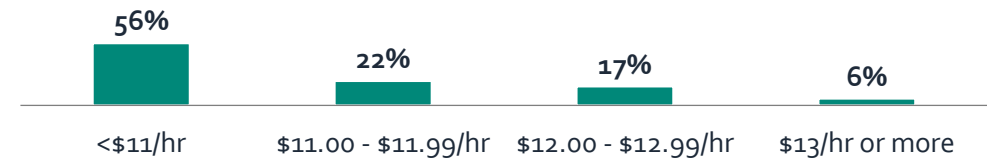
% of Part – time Employees Paid* (n=15) ⚠️



% of Tipped Employees Paid* (n=13) ⚠️



Lowest Starting Hourly Rate for Full-time Employee (n=18) ⚠️



⚠️ Low Sample Size

70% of responding restaurants oppose the minimum wage increases



Acceptance of Minimum Wage Rates (n=20) ⚠️



Reasons for Not supporting Minimum Wage Rates	Rank
Flexibility requirement (to reduce cost) for small businesses	1
Increased expenses for employers	2
Decision of market/type of work	2
Services become expensive	3
Decrease in employment (particularly unskilled)	3
Reluctance to pay teenagers more	4

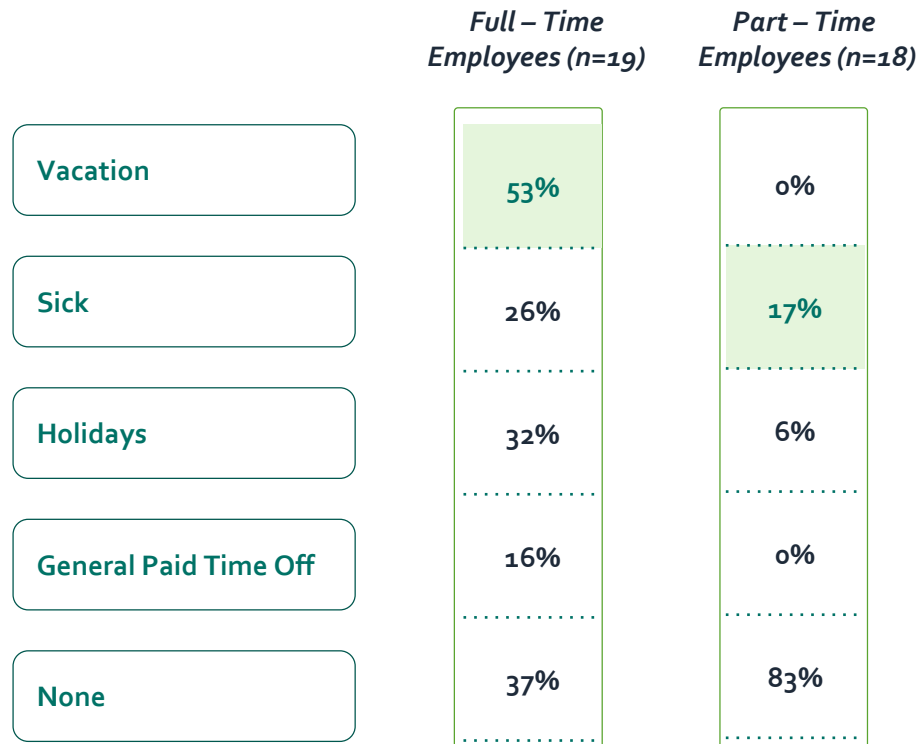
Reasons for supporting Minimum Wage Rates	Rank
Better retention	1
Fair, living wage	2
Assurance for workers	2

⚠️ Low Sample Size

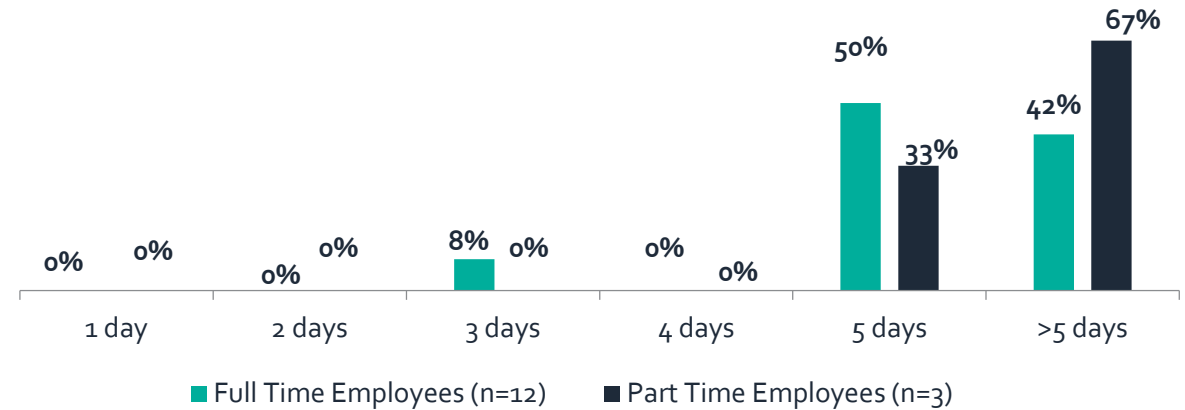
63% of full-time restaurant employees receive some form of paid time off while 83% of part-time restaurant employees do not receive any paid time off

Restaurant

Paid Time-off



Number of Days Earned Per Year



Track and Administer Paid Time-off (n=18)

Manually	45%
Computer Program	33%
Outsourced	0%
Others	22%

Low Sample Size

75% of responding restaurants oppose the paid sick leave regulations



Acceptance of Paid Sick Leave Regulations (n=20) ⚠️



Reasons for supporting Paid Sick Leave Regulations (n=5)	Rank
No Open-End Response	



Reasons for Not supporting Paid Sick Leave Regulations	Rank
Increases cost	1
Abuse of facility	2
Determined by market/business	3
Hard to maintain records	3
Unnecessary for part-time/under 18 employees	4
Too many hours	4

⚠️ Low Sample Size



Service Sector

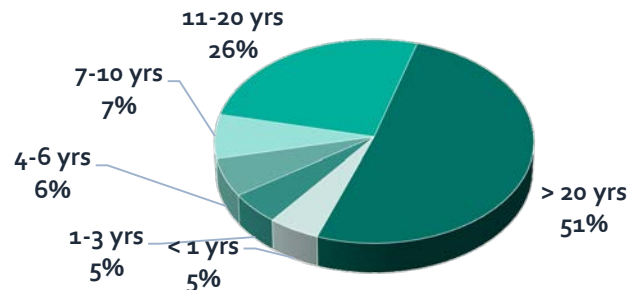
General Questions (1/2)

Nearly 50% of service businesses have been in operations for more than 20 years with average employee tenure fairly evenly spread between 1 and 20 years

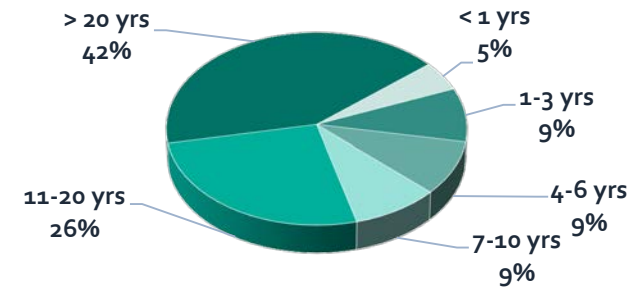


Service

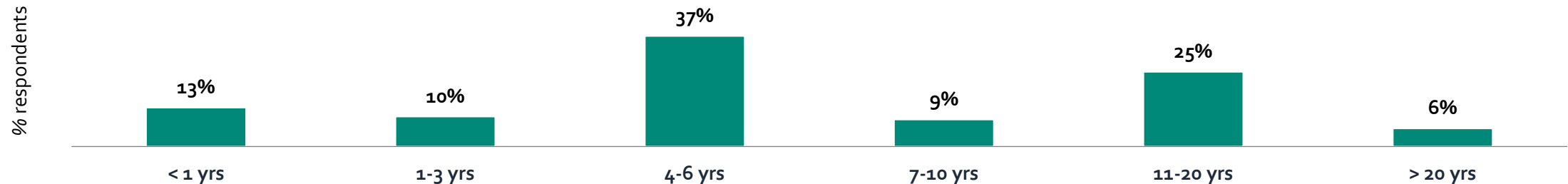
Number of Years Business Has Been Operational (n=55)



Number of Years Business Has Been Operational in Wilmette (n=55)



Average Tenure of Employees (n=52)



46% of responding service businesses have less than 4 employees, while 96% of the businesses have less than 4 employees under the age of 18 years



Service



Employee Status in an Organizations

Employee Group	% of Responding Businesses with Number of Employees				
	< 4	4-10	11-25	26-40	> 40
Total Employees (n=54)	46%	35%	13%	4%	2%
Full-time Employees (n=54)	63%	28%	4%	4%	1%
Part-Time Employees (n=49)	88%	8%	2%	0%	2%
Employees Working Less than 90 Days in a Consecutive Year (n=49)	88%	8%	4%	0%	0%
Employees Under the age of 18 (n=49)	96%	2%	0%	0%	2%
Tipped Employees (n=48)	90%	8%	0%	0%	2%

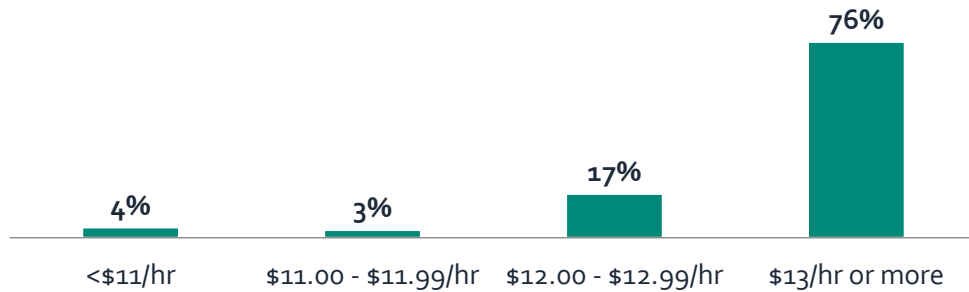
n=responding businesses

A mix of an hourly wage distribution is seen among the employees of responding service businesses

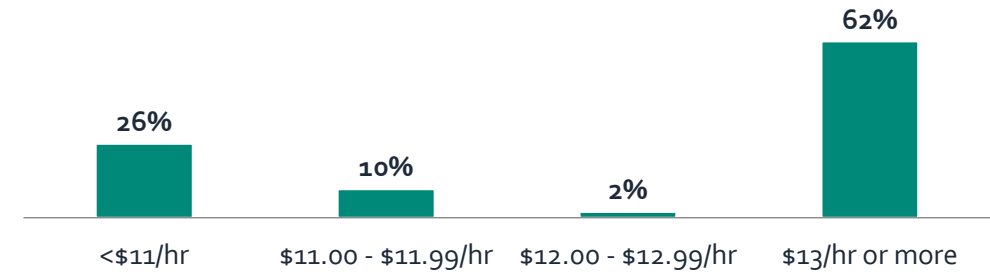


Service

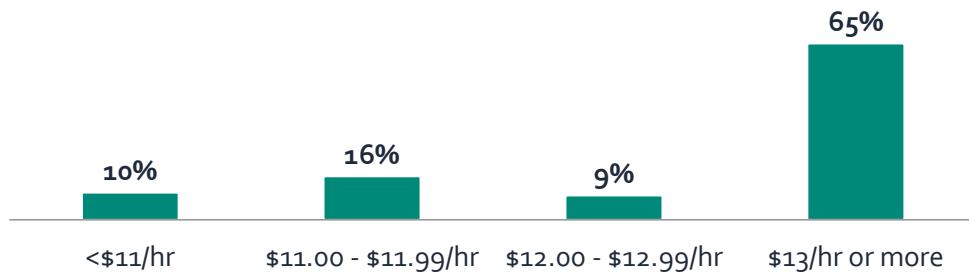
% of Full – time Employees Paid* (n=37)



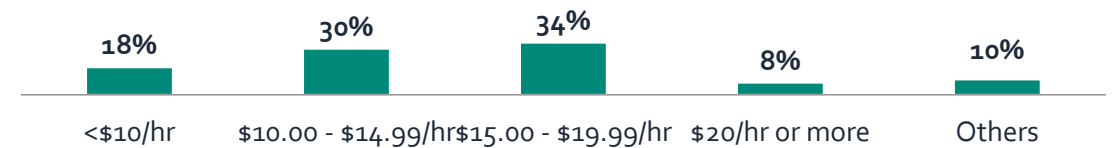
% of Part – time Employees Paid* (n=30)



% of Tipped Employees Paid* (n=9)



Lowest Starting Hourly Rate for Full-time Employee (n=49)



Low Sample Size

53% of responding service businesses oppose the higher minimum wage rate



Service

Acceptance of Minimum Wage Rates (n=55)



Reasons for Not supporting Minimum Wage Rates	Rank
Increased expenses for employers	1
Decision of market/type of work	2
Flexibility requirement for small businesses	3
Incentives can't be offered	4
Decrease in employment	4

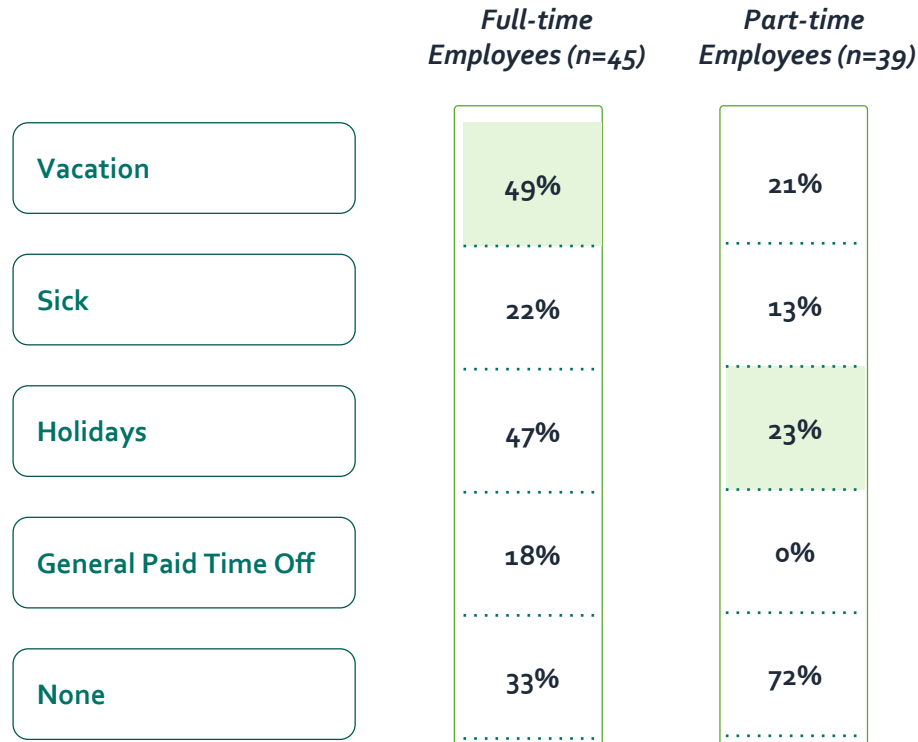
Reasons for supporting Minimum Wage Rates	Rank
Fair, living wage	1
Value for workers	1
Reasonable	2
Attract hourly paid workers	2

35% of full-time service employees and 72% of part-time service employees do not receive any paid time off

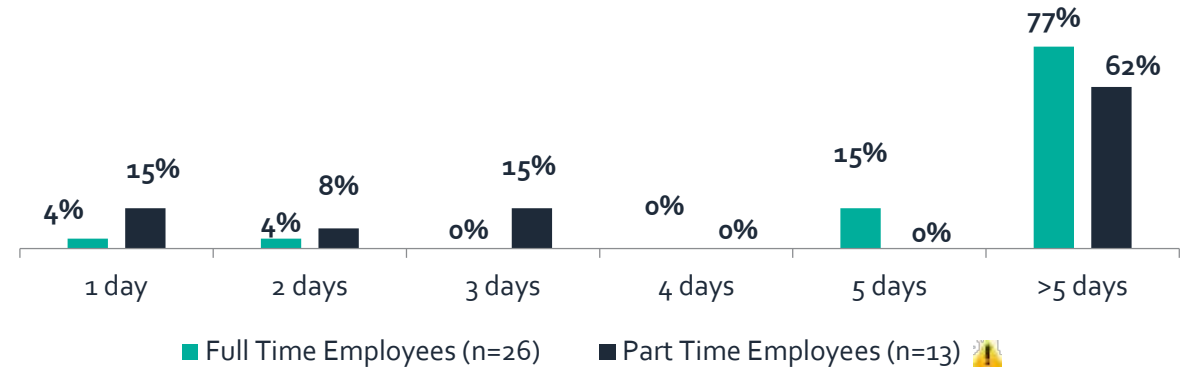


Service

Paid Time-off



Number of Days Earned Per Year



Track and Administer Paid Time-off (n=37)

Manually	73%
Computer Program	11%
Outsourced	8%
Others	8%

⚠️ Low Sample Size

64% of responding service businesses opposed the paid sick leave regulations



Acceptance of Paid Sick Leave Regulations (n=55)



Reasons for supporting Paid Sick Leave Regulations	Rank
Fair and reasonable	1
Sick workers not fit for workplace	2
Performance deteriorates when working sick	2



Reasons for Not supporting Paid Sick Leave Regulations	Rank
Determined by market/business	1
Increases cost	2
Unnecessary for part-time/under 18 employees	3
Hard to maintain records	4
Abuse of facility	5

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Who Is Affected by the Minimum Wage?*

DALE BELMAN, PAUL WOLFSON, and KRITKORN
NAWAKITPHAITON

Prior surveys of empirical research on the minimum wage have been organized around the question “What does the minimum wage affect?” This survey is organized around the question “Who is affected by the minimum wage?” We review the consequences of the minimum wage for teens and young workers, men and women, African Americans and Hispanics, the less educated, workers in low-wage industries, and low-wage/low-income populations. Although there is almost universal agreement that the minimum wage boosts earnings, evidence for a negative employment effect varies between mixed and nonexistent. An important gap in the literature is the paucity of research on low-wage/low-income groups.

Introduction

Surveys and literature reviews of empirical research on the minimum wage are typically organized around specific outcomes that the minimum wage may influence. Employment is the most common (Neumark and Wascher 2007), perhaps in partnership with variables such as unemployment (Brown, Gilroy, and Kohen 1982); the distribution of income (Brown 1999); and wages, poverty, and the costs to employees and shareholders (Card and Krueger 1995). Neumark and Wascher’s (2008) book has chapters devoted to most of these variables as well as skills and prices, as does our book, *What Does the Minimum Wage Do?* (Belman and Wolfson 2014). In examining specific outcomes, the analyses considered in the surveys above almost always consider them not in the abstract, not with respect to entire economies, but for specific subpopulations or industries within an economy; common examples are teenagers and the food-and-drink sector. The result is a patchwork of effects with limited insight into the comprehensive impact of the minimum wage on

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Road, Oxford, OX4 2DQ, UK.

segments of the population. What follows is a survey organized around the question, “who?” rather than “what”; a survey in which the primary dimension for organizing and discussing analyses is the group or industry under study.

There are two benefits to this approach. From a policy perspective, the benefit is that this article brings together in one place the known effects of minimum wage policy on specific groups of people and on specific industries in the economy: teenagers, women, people of color, the restaurant industry, and employees from low-income families. From a research perspective, the article highlights gaps in our knowledge. We use data from the 2012 CPS to understand the importance of the minimum wage to each group and the importance of each group to assessing outcomes of the minimum wage. The combination of the data and research not only clearly reveals the presence of gaps, it also displays the relative importance of each group and industry to understanding the effects of the minimum wage. The most obvious example is that of prime age and older workers and teenagers. Teens are the most studied group in the minimum-wage literature; more than forty studies (one-third of this literature) provide estimates of the effect of the minimum wage on teens. This provides rich information on consequences for employment, accessions and separations, earnings, and the response of school enrollment. Teenagers, however, comprise less than one-fifth of workers who earn no more than the minimum wage and barely one-tenth of those who earn no more than 1.5 times the minimum wage.¹ Older adults, those at least 25 years old, are a majority of these workers. By now, examination of consequences of the minimum wage for prime age and older adults, for whom there is little specific research, is certain to be more revealing than yet another study of teenagers.

After beginning with a discussion of some statistical issues necessary to evaluate this research, we follow the literature, slicing the working-age population along different dimensions in order to focus on demographic groups suspected of being vulnerable to unintended consequences of the minimum wage. In the second section, we begin by splitting the working-age population along standard demographic lines: first by age into teenagers, young adults, and older adults; second by gender; and third by race and ethnicity into Hispanics and non-Hispanic blacks. In the fourth classification, nonstudent workers are distinguished according to the highest level of education achieved, in order to consider the interplay between the minimum wage and low levels of education. The next section is a consideration of the very limited research of effects on

¹ In point of fact, 103 percent rather than 100 percent of the actual minimum wage. This approximation, not uncommon in the literature, allows for spikes in the measured wage distribution thought to reflect rounding and mistakes of respondent recall, without substantially changing the measurements.

full- and part-time workers, followed by a section laying out the more extensive research on specific low-wage industries. Finally, earners are classified by family income categories to examine the concern that in focusing on low-wage workers, the minimum wage is insufficiently focused on low-income workers.²

Methodological Issues

Identification. The issue identified by the phrase “Correlation is not causation” has been of interest to economics, a largely observational science, at least since Working (1927).³ Morgan (1990), and CHRIST, CARL F., Christ (1994). Experimental sciences can resolve the issue either explicitly by systematic variation in causal variables or implicitly by randomly assigning experimental subjects to treatment or control groups. Although these solutions are possible in the study of government participatory programs and occasionally those of private organizations, statistical solutions have received much attention because those of the experimental sciences are unavailable for most topics of economic interest. Prior to the 1991 conference that launched the New Minimum Wage Research (NMWR), minimum wage research had not shared the increasing interest in the possibility of more nearly mimicking controlled experiments (Meyer 1995). Instead, it relied on estimated partial correlations from one-dimensional observational data, primarily time series, and less often cross-sectional, data (Brown, Gilroy, and Kohen 1982).^{4,5}

The conference papers contrasted sharply with earlier practice. Neumark and Wascher (1992) introduced panel data and models to the field, adapting the analytic framework previously applied to time-series data in the hope that greater variation in the data would allow for more careful measurement of the relationship between employment and the minimum wage. Card (1992) and Katz and Kreuger (1992) introduced what we would now recognize as early versions of quasi-experiments (QEs): the latter, a differences analysis, the former

² This review is not exhaustive. We limit ourselves to articles on developed countries, with a heavy emphasis on studies of the U.S. experience. How far back we reach into the literature varies according to how much recent work there has been on a topic; in discussing employment effects, we generally limit our discussion to work published since 2000, but for topics where there are few articles, such as work on education and training, we include earlier studies.

³ See also Christ (1994), Epstein (1987), Morgan (1990).

⁴ In October 1992, *Industrial and Labor Relations Review* (46:1) included a symposium consisting of five papers presented at the conference and Ehrenberg's (1992) introduction.

⁵ Wooldridge (2010: Chapter 21) contains a more recent discussion of this quasi-experimental literature.

a protosynthetic control framework. Following the conference, in perhaps the most widely known analysis of the NMWR and one of the most widely known analyses of its kind, Card and Krueger (1994) introduced the difference-in-differences technique to the study of the minimum wage.

In the next decade or so, panels and QEs separately became much more widely used, with Neumark and Wascher's approach coming to be referred to as the canonical model (Allegretto, Dube, and Reich 2011). Each evolved variations. Eventually, serious questions arose concerning the estimated standard errors and the validity of statistical inference based on them (discussed in the next section), but the most important substantive criticism of each changed little during this time. For the QEs, this was doubt about generalizing from what were essentially a few short-term case studies. For the panel model, this concerned the absence of any response to the objection, "Correlation is not causation," and ultimately about poor identification of the effect of the minimum wage because treatment and control groups were not carefully matched.

The concerns about the QEs are straightforward and easy to understand but the identification issue is a bit more subtle, so a few words of elaboration are in order. Is the minimum wage endogenous, and if so, what is an appropriate correction? In their critique of Neumark and Wascher (1992), Card, Katz, and Krueger (1994) objected that the Kaitz index was endogenous and by construction would vary inversely with employment even without any correlation between employment and the minimum wage.⁶ Even without this artificial introduction of endogeneity, if both minimum-wage legislation and leisure are normal goods, the minimum wage and labor supply (and thus employment) are correlated, without causation running from one to the other.

Little progress was made toward resolving either of these issues until three analyses that showed how the canonical model could be transformed, step-by-step, to one that looked like a set of QEs (Allegretto, Dube, and Reich 2009, 2011; Dube, Lester, and Reich 2010). Because many state policies that affect labor demand and supply are both correlated when considered contemporaneously across states and contemporaneously homogeneous within multi-state regions (Allegretto, Dube, and Reich 2009) the census divisions can be understood as regional economies within which effects of the minimum wage can be disentangled from those of other factors. Two of the studies (Allegretto, Dube, and Reich 2009; Dube, Lester, and Reich 2010) take this one step further, using county-level employment data to define local labor markets at the level of neighboring counties. When adjacent counties straddle a state

⁶ The Kaitz index is the coverage adjusted relative minimum wage. It is calculated as the ratio of the minimum wage to an average wage multiplied by the proportion of the relevant labor force covered by the minimum wage.

boundary and the states in question have contemporaneously different minimum wages, different parts of the labor market sort naturally into either the treatment or control group.

In the simplest specification of both the state-level and county-level data, all variation in the minimum wage is used (along with some other control variables) to explain all variation in employment just as in the canonical model. The incorporation of one set of dummy variables transforms this specification into a set of regional economies. In the two studies that use county-level employment data, a different set of dummies transforms the analysis into a collection of QEs.⁷ In all three analyses, the simple, canonical model generates results that resemble those of other analyses that rely on the canonical model. In all three, the specifications that correspond to QEs have wage effects of the same size as, or larger than, those in the canonical model but the employment effects are uniformly not statistically significant and more often than not the point estimate of the minimum wage term is positive. The marriage of the two approaches, long-term panel and QE, responds to the major criticisms of each of the earlier approaches, that in the canonical model the effect of the minimum wage is not well identified and that each QE is a short-term case study that cannot be reliably generalized.

In response to this line of work, Neumark, Salas, and Wascher (2014) objected that the method for identifying observations to be controls and their matching to the treatment observations is *ad hoc*, and questioned whether the observations identified as controls are well suited for that function. The apparent solution is the synthetic control technique (Abadie, Diamond, and Hainmueller 2010), which Sabia, Burkhauser, and Hansen (2012) introduced to this literature as the third of three QE analyses of the rise in New York's minimum wage in 2005 and 2006. The synthetic control technique forms an optimal counterfactual (the synthetic control) for the treated subject from a weighted mean of a prespecified list of untreated subjects. Done properly, the synthetic control closely resembles the treated subject before the treatment and it is necessary to calculate only the *ex post* difference instead of a difference-in-differences. Of the twenty-five states that did not experience a rise in the minimum wage during 2002–2006, this technique selected four to be part of the synthetic control; the two with greatest weight were among the control subjects that Sabia, Burkhauser, and Hansen (2012) used in their two more conventional QEs. Despite this, the *ex ante* match was sufficiently poor that they felt it necessary to calculate the difference-in-differences using the synthetic con-

⁷ The dummy variables effectively remove local labor markets with a uniform minimum wage from the analysis.

trol. The resulting point estimates were negative but not significant at a 0.05 level according to their synthetic *t*-distribution.

The synthetic control technique is difficult to combine with the canonical framework of long panels because the minimum wage rises both frequently and asynchronously across states. Neumark, Salas, and Wascher (2014) begin the technique for a subsample of about one-quarter of the minimum wage changes that occurred in the United States from 1990q1–2011q2 but reject it because

this subset of minimum wage increases (there are a total of 544 minimum wage increases in our sample period) appeared to be unusual in that it did not generate a significantly negative minimum wage effect using the modified panel data estimator described earlier. (p. 639)

Instead, they develop an *ad hoc* technique that bears at least a superficial resemblance to that of Abadie, Diamond, and Hainmueller (2010) and for which

the counterfactual observations could include observations with minimum wage increases; however, because these counterfactual observations contribute to both the estimated employment rate and the estimated minimum wage variables (as well as the other controls), this is not problematic. (p. 639)

Including states with minimum wage increases in the pool of potential donors to the synthetic control not only deprives their approach of much of the intuitive appeal that adheres to this technique but also makes it difficult to understand precisely what its advantage is over not just a more conventional QE, but even over the canonical model. Both the qualitative similarity and the lack of a statistically significant difference between these results and those of the canonical model reinforce these doubts. Dube and Zipperer (2013) apply the technique of Abadie, Diamond, and Hainmueller (2010) to part of the subsample that Neumark, Salas, and Wascher (2014) rejected in favor of the whole sample. They report only three out of nineteen statistically significant effects on employment at the 0.10 level (one positive, two negative)—about what one would expect from a sample of this size if there is no effect.

After more than two decades of NMWR, the central points of methodological dispute are no longer about the appropriate analytical framework, as they were for much of that period. They are now about the best way to implement the QE framework and how best to select or construct appropriate controls for treatment observations.

Biased or inconsistent standard errors. Complicating assessment of the literature are problems with the estimated standard errors that raise serious questions about the reliability of the statistical inference it contains. The most common is serial correlation in panels and repeated cross-sections that is unaccounted for in the calculations. Bertrand, Duflo, and Mullainathan (2004), BDM hereafter, is the best-known work to call attention to this issue, and Hansen (2007a, 2007b) and Wolfson (2011) have confirmed their concern. Donald and Lang (2007) have called attention to a less common problem, inconsistent standard errors in the simple two-by-two difference-in-differences framework where it is plausible that the treatment and control dummies are picking up random shocks in addition to any effect of policy. Having little confidence in inference from analyses likely to be suffering from these problems, we generally do not discuss them; where we do, we note the problem. In our supplementary tables, which list all the analyses and their results, we note those that are problematic. On the other hand, despite agreeing with Kuehn (2014) about both the importance of careful matching of control and treatment groups, and that there is no reason that best practice in the minimum-wage literature should be broadly different from that elsewhere in labor policy analysis, the current lack of widespread agreement on this issue discourages us from using it to prune our list of studies.

Effects by Subgroup

By age. Teens and young adults have been the focus of much of the research on the minimum wage. Belman and Wolfson (2014) discuss more than forty analyses that consider the effect of the minimum wage on teenagers and young adults. As Table 1 shows, this focus is both understandable and unfortunate. Let us turn first to “understandable.” Under the broad heading “Row Percentages,” the left side of Table 1 displays the cumulative wage distribution (relative to the effective minimum wage) of teenagers and young adults, as well as some basic labor-force statistics for each. About one-quarter of teenagers are employed (line 1a), of whom about one-third earn no more than the effective minimum wage; one-half earn no more than 1.1 times the effective minimum wage; and four-fifths and nine-tenths, respectively, earn no more than 1.25 and 1.5 times the effective minimum wage.⁸ About 60 percent

⁸ The effective minimum wage is the higher of the state or federal minimum wage in the state where the individual works. We use the two terms interchangeably unless otherwise indicated. We use 0.97 to 1.03 times the minimum wage instead of the exact minimum wage to allow for rounding and reporting errors. All calculations are derived from the 2012 CPS outgoing rotation files.

TABLE 1 (cont.)

Line	Category	Row Percentages						Column Percentages									
		EPOP	UPOP	NILF	*MW	≤1.03	≤1.1	≤1.25	≤1.5	Share of Total Emp.	Share of Unemp.	Share of NILF	*MW	≤1.03	≤1.1	≤1.25	≤1.5
7a	Food svcs & drinking Places				28%	41%	61%	74%	6%				29%	27%	21%		17%
7b	Accommodations				14%	20%	36%	52%	1%			2%	2%	2%	2%		2%
7c	Retail				10%	17%	35%	49%	12%			19%	21%	22%	20%		20%
8a	< \$20K (fam. inc.)	35%	9%	56%	15%	24%	45%	62%	17%	32%	27%	25%	25%	25%	25%	22%	22%
8b	\$20K-\$34K (fam. inc.)	49%	6%	45%	9%	14%	28%	44%	18%	21%	22%	21%	21%	22%	22%	23%	23%
8c	\$35K-\$49K (fam. inc.)	60%	5%	35%	6%	9%	18%	29%	23%	22%	22%	23%	22%	22%	23%	24%	24%

NOTES: All calculations are based on the 2012 CPS outgoing rotation files.

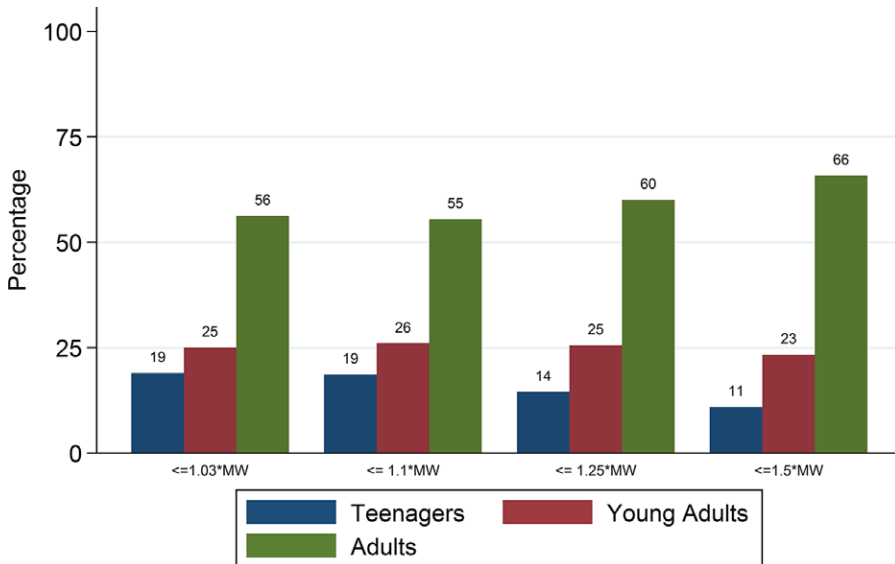
"Some college" excludes individuals who received any degree, including an associate's degree.

"Total" in "Share of total" refers to individuals at least 16 years old. EPOP: employment-to-population ratio; UPOP: unemployment-to-population ratio; NILF: not in the labor force; Share of Total: percentage of employment, unemployed, and those not in the labor force; Share of Emp: Percentage of the employed; Share of Unemp: percentage of those who are unemployed; Share of NILF: percentage of those not in the labor force; HS: high school; Part time: work less than 35 hours per week; Svcs: services; fam inc.: family income.

of young adults are employed (line 1b), of whom one-sixth earn no more than the minimum wage; one-quarter earn no more than 110 percent of the minimum wage; and almost one-half and two-thirds earn, respectively, no more than 125 percent and 150 percent of the minimum wage. Employed members of both groups, but especially teenagers, are heavily concentrated in the wage distribution near the minimum wage.

Now let us turn to “unfortunate,” and consider the right side of Table 1, under the broad heading of “Column Percentages.” Teenagers comprise only 3 percent of all employment and together with young adults comprise only 12 percent of all employment. Although their shares of low wage earners are disproportionately large, teens comprise only 19 percent of those earning the exact minimum wage and less than 1.1 times the minimum (which we shall refer to as “very-low wages”), and this figure drops to 14 percent and 11 percent, respectively, of those earning no more than 1.25 and 1.5 times the minimum (which we shall refer to as “low wages”). Together, teens and young workers comprise less than half of those who earn no more than the minimum wage. As we move up the wage distribution, this fraction falls until it is roughly one-third of those who earn no more than 150 percent of the minimum wage. Figure 1 shows this visually: older adults, those 25 and older, comprise more than half of each wage band. While

FIGURE 1
AGE COMPOSITION OF CUMULATIVE WAGE DISTRIBUTION: WAGE BANDS RELATIVE TO THE EFFECTIVE
MINIMUM WAGE



the focus on young workers may be useful in determining whether the minimum wage ever has a specific effect, it obscures a balanced understanding of the effects of the minimum wage.

More than one-third of the studies considered in this survey examine some aspect of the effect of the minimum wage on teenagers and young adults: employment (37), hours (10), wages or earnings (13), human capital formation (16), and labor market dynamics including labor force participation (15). Considering only employment, and not including those subject to the BDM critique, two report a positive response (Giuliano 2013; Portugal and Cardoso 2006), eleven a negative response (Bazen and Marimoutou 2002; Campolieti, Fang and Gunderson 2005b; Eckstein, Ge, and Petrongolo 2011; Kalenkoski and Lacombe 2008; Pacheco 2011; Pereira 2003; Sabia 2009a, 2009b; Sabia, Burkhauser, and Hansen 2012; Thompson 2009; Yuen 2003), seven no response (Allegretto, Dube, and Reich 2009, 2011; Lee and Suardi 2011; Neumark and Nizalova 2007; Stewart 2002, 2004a, 2004b), and six mixed responses (Addison, Blackburn, and Cotti 2013; Bazen and Le Gallo 2009; Hyslop and Stillman 2007; Orrenius and Zavadny 2008; Pinoli 2010; Williams and Mills 2001).

This large number of studies poses challenges for assessing the results. We are faced with many estimates based on different methodologies and measures of the minimum wage. Negative elasticities comprise the largest category of outcomes but they are almost perfectly balanced by those which report zero or positive elasticities. This balance is strengthened by the seven studies reporting mixed effects.

This approach can be supplemented with two meta-regression analyses of research on teenage and young adult employment. Going beyond counts and averaging, meta-regression provides a more systematic weighing of results, and controls for differences in precision and methodology. Doucouliagos and Stanley (2009) estimate meta-regressions using 1474 estimates from sixty-four studies of minimum wage consequences for teen employment in the United States, published between 1981 and 2006. Meta-estimates of the employment elasticity are negative, but small in magnitude and never close to statistical significance. Belman and Wolfson (2014) estimate meta-regressions for studies published from 2001 to 2013. The effect on employment of teenagers and young adults is negative and small in magnitude, with elasticities that range from -0.117 and -0.070 and significant in a 5-percent hypothesis test when all countries are treated identically. Estimates for studies of the United States are smaller in magnitude, ranging from -0.008 to -0.02 and are not close to achieving significance in a 10-percent one-tailed test. Together, these meta-regression results suggest that the minimum wage does not negatively affect youth employment in the United States but may in other countries.

Estimates of the effect of the minimum wage on teenagers and young adults' hours of work are also mixed, with four articles reporting a negative effect (Pacheco 2011; Pereira 2003; Sabia 2009a, 2009b), two reporting no effect (Allegretto, Dube, and Reich, 2009, 2011), one a positive effect (Hyslop and Stillman 2007), and one a mixed effect (Orrenius and Zavodny 2008). There are no patterns across these studies suggesting that effects differ when considering more finely defined groups, such as teen women or those in retail.

What of other employment-related outcomes? Hyslop and Stillman (2007) find the unemployment rate does not respond to minimum wage increases; Flinn (2006) reports an increase in unemployment, employment, and labor-force participation; and Partridge and Partridge (1998) report that both higher minimum wages and broader coverage of minimum wages raise teen unemployment. Ahn, Arcidiacono, and Wessels (2011) report that teenagers from better educated households increased their labor-force participation. Three studies report that the accession rate falls following a minimum wage increase (Dube, Lester, and Reich 2014; Portugal and Cardoso 2006; Thompson 2009); Pinoli (2010) reports results for anticipated minimum wage increases that are consistent with these studies but reports no decline for those that are not; Giuliano (2013) reports that the effect varies. Two studies report a decline in the separation rate (Dube, Lester, and Reich 2014; Portugal and Cardoso 2006) and one reports no effect (Giuliano 2013). Given limited numbers of estimates and the mix of results, there appears to be no systematic effect for unemployment or participation. The evidence on accessions and separations is richer, pointing toward a decline in hiring and a possible fall in the separation rate.

Most studies of young workers' wages or earnings report a positive effect (Allegretto, Dube, and Reich 2011; Dube, Lester, and Reich 2014; Giuliano 2013; Lam et al. 2006; Orrenius and Zavodny 2008). Two partial exceptions are Hyslop and Stillman (2007), who find a positive effect on the weekly earnings of 16–17-year-olds, but not for 18–19-year-olds, and Neumark and Wascher (2011), who find a positive effect on hourly, but not weekly, earnings. Estimated elasticities range from 0.07 to 0.6. The virtual unanimity of the evidence supports a conclusion that increases in the minimum wage raise teen and young adult earnings.

Estimates for school enrollment are mixed but suggest that increases in the minimum wage may reduce enrollment among some groups of students. One study (Warren and Hamrock 2010) finds a negative effect on schooling, three report no effect (Baker 2005; Campoletti, Fang, and Gunderson 2005a; Hyslop and Stillman 2007), one finds a positive effect (Mattila 1981), and three report mixed results (Chaplin, Turner, and Pape 2003; Cunningham 1981; Pacheco

and Cruickshank 2007).⁹ The mixed results suggest that although higher minimum wages do not reduce enrollments among all teens, there could be a negative effect for some groups. Chaplin, Turner, and Pape (2003) find that the minimum wage reduces school continuation in states where teens may leave school at 16 or younger, but not otherwise. The extension of coverage to teens in New Zealand increased, while higher real minimum wages reduced, teen enrollment (Pacheco and Cruickshank 2007). Together, these results suggest that there may be a negative effect on the schooling of some groups of teens, but the evidence is mixed.

Although there is nothing inherently wrong with researchers focusing on consequences of the minimum wage for teens and young adults, it reflects a misplaced perception that the minimum wage is fundamentally a matter of concern primarily to young workers. Young workers are more likely to be employed at the minimum wage than are older workers, but the majority of those directly affected by the minimum wage or in jobs paying close to the minimum wage are at least 25 years old. While the effect of the minimum wage on older workers has been considered in the context of research on the less educated and less skilled, more direct research on older workers is an avenue for future research.

By gender. According to the recent literature, the minimum wage has stronger effects on women than men. The evidence for men largely indicates that it has no effect on employment, while the evidence for women leans in that direction but is more mixed. Single mothers have been of particular interest and we discuss this group both in our review of demographic data and research.

Women comprise not quite half of either part of the labor force: 47 percent of the employed and 46 percent of the unemployed are female (Table 1, line 2b). Single mothers, a subgroup singled out in some analyses, account for 6 percent and 10 percent of the employed and unemployed, respectively (Table 1, line 2a). While their share among the employed is roughly in proportion to their 5-percent share of the 16 and older population, they are over-represented among the unemployed.

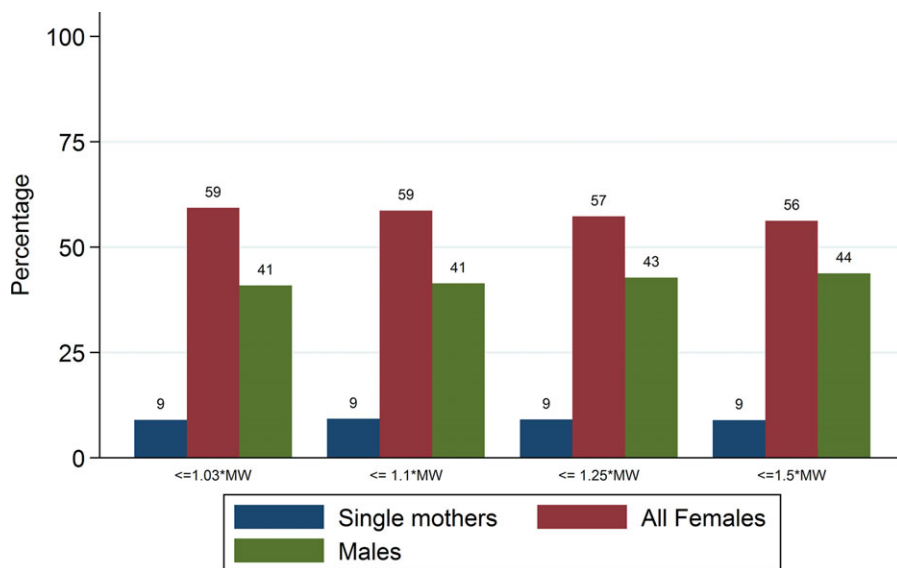
Women are over-represented at the bottom of the cumulative wage distribution. Despite making up less than half of the employed labor force, women are 59 percent of those who earn no more than the minimum wage and no more than 1.1 times the minimum wage, and more than 55 percent of those who earn no

⁹ The effects of serial correlation in panels on estimates of standard errors, as well as the Moulton (1990) critique, have not been established for studies of the effect of the minimum wage on school enrollment, but there is reason to believe that, absent clustering of standard errors, statistical inference will be unreliable. Articles for which this may be an issue are not discussed in the body of this study, but appear in the Appendix.

more than 1.25 and 1.5 times the minimum wage. Nevertheless, few employed women earn very low wages; only 7 percent earn no more than the minimum wage, and only 11 percent earn no more than 1.1 times the minimum wage. As we turn to low (rather than very low) wages, the fractions rise substantially; 21 percent of women earn no more than 1.25 times the minimum wage and 32 percent no more than 1.5 times the minimum wage. For single mothers, the proportions are higher for very low wages; 9 percent earn no more than the minimum wage and 15 percent earn no more than 1.1 times the minimum wage. The proportions are much higher for low wages, with 43 percent of all employed single mothers earning no more than 1.5 times the minimum wage. Single mothers comprise 9 percent of very low and low-wage employment.

Figure 2 clearly brings out two features. One is the stable fraction of low-wage employment made up of single mothers. The other is that although women are slightly in the minority in the employed labor force, they are in the majority at the lowest wage levels, a majority that only slowly declines as the definition of “low-wage employment” is relaxed. The employment shares of men and women converge above three times the minimum wage; continuing up the wage distribution, the employment share of men surpasses that of women.

FIGURE 2
GENDER COMPOSITION OF CUMULATIVE WAGE DISTRIBUTION: WAGE BANDS RELATIVE TO THE EFFECTIVE MINIMUM WAGE



Studies of the employment effect for men are consistent in reporting no effect but that is not the case for women. Similar to men, women's wages rise with the minimum wage. Of the eight studies of women's employment, four suggest no effect on women's employment (Orrenius and Zavodny 2008; Stewart 2002, 2004a, 2004b), one finds negative effects (Laroque and Salanié 2002), and three find mixed effects (Neumark and Wascher 2011; Pinoli 2010; Sabia 2008). Sabia (2008) and Pinoli (2010) report negative effects for women with limited education but no effect in samples not stratified by education. Neumark and Wascher (2011) find either no effect or a small positive effect on the employment of women with no more than a high school degree. Laroque and Salanié (2002) report a negative effect for a sample of married women in France. Of the three hours studies, one finds no effect (Connolly and Gregory 2002), and two mixed effects (Orrenius and Zavodny 2008; Stewart and Swaffield 2008). Finally, Pedace and Rohn (2011) find that higher minimum wages are associated with longer unemployment duration for older women and women in lower skilled occupations. Overall, the evidence for women's employment is mixed, with several studies finding a negative effect on the employment and hours of some groups of women, while other research finds no decline in employment or hours for most women.

Studies of wage effects suggest that higher minimum wages are associated with higher wages and earnings for women and that the effects are larger than for men. Nine of the eleven articles that estimate the effect of the minimum wage on women's earnings find a positive effect on hourly or weekly earnings (Butcher 2005; Dinardo, Fortin, and Lemieux, 1996; Easton 2006; Harvey and Bernstein 2003; Lee 1999; Neumark and Wascher 2011; Orrenius and Zavodny 2008; Stewart 2004a), one reports a mixed effect (Sabia 2008), and one reports no effect (Grossberg and Sicilian 1999). Neumark and Wascher (2011) report a positive effect on wages, but do not find an effect on annual earnings in a sample which includes those who have no labor earnings. Studies of wage distributions suggest that higher minimum wages affect the first two to three deciles of the wage distribution of women (Butcher 2005; Dinardo, Fortin, and Lemieux, 1996; Harvey and Bernstein 2003; Lee 1999). Research on inequality consistently finds that the decline in the real minimum wage after the 1970s had a markedly greater impact in increasing earnings inequality among women than among men.¹⁰

Increases in the minimum wage do not reduce male employment, but may reduce hours of work. Five of seven employment studies find no male employment effect (Mulheim 2008; Orrenius and Zavodny 2008; Stewart 2002, 2004a, 2004b), one finds a small negative effect (Eckstein, Ge, and Petrongolo 2011),

¹⁰ Sabia (2008) finds that the minimum wage does not affect whether single women are above or below the poverty line.

and one a positive effect for young men (Stewart 2004b). Stewart and Swaffield (2008) report a negative hours effect after one year for British men bound by the minimum wage, Orrenius and Zavodny (2008) report no effect in the United States for native-born or immigrant men with low education. In addition, Pedace and Rohn (2011) report that increases in the minimum wage increase unemployment duration for men without high school degrees and reduce unemployment duration for older men and those with a high school degree.

Eight of nine studies of wages, wage distributions, or wage growth conclude that higher minimum wages are associated with higher earnings among men paid low wages (Autor, Manning, and Smith 2010; Butcher 2005; Easton 2006; Eckstein, Ge, and Petrongolo 2011; Krashinsky 2008; Lee 1999; Sicilian and Grossberg 1993; Stewart 2004a). The target groups include bound workers, care home assistants, immigrants, less educated men and less educated white men, and low-wage men in the United States and U.K. Harvey and Bernstein (2003) find that increases in the minimum wage do not affect the male wage distribution. Grossberg and Sicilian (1999) indicate that wage growth was slower among men hired at exactly the minimum wage than among men hired at slightly higher wages, Easton (2006) and Stewart (2004a) report higher wage growth for minimum-wage workers.¹¹

Three studies of wage distributions and inequality find that lower minimum wages are associated with higher wage inequality (Autor, Manning, and Smith 2010; DiNardo, Fortin, and Lemieux, 1996; Lee 1999), but one reports that “the lower deciles appear to be little affected by minimum-wage legislation” (Harvey and Bernstein 2003).¹²

By race. Some objections to minimum wage increases focus on their possibly disparate effect on racial and ethnic minorities, primarily Hispanics and non-Hispanic African Americans (“blacks” hereafter for brevity).¹³ Turning first to Hispanics, 60 percent are employed, 7 percent unemployed, and about one-third are not in the labor force (Table 1, line 3a). Ten percent of employed Hispanics earn no more than the minimum wage, 16 percent earn no more

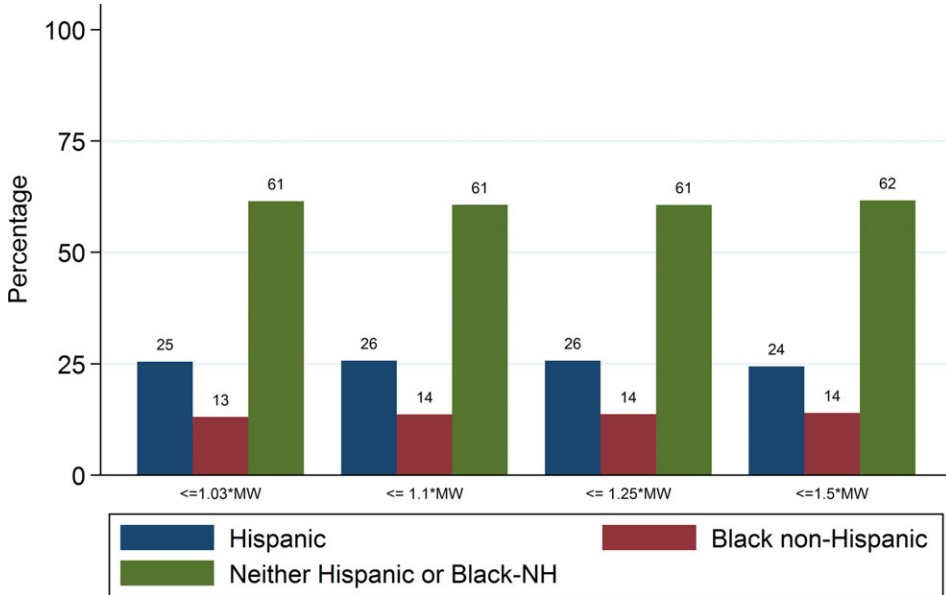
¹¹ The differences among these results are related to differences in samples and the measure of the minimum wage. Grossberg and Sicilian (1999) have data by firm, and classify workers as minimum wage if they were hired at exactly the minimum wage, while Easton (2006) uses a repeated cross section in which the minimum wage variable denotes the value of the applicable minimum wage by state, and Stewart (2004a) measures the effect on bound workers in a U.K. sample.

¹² An old study of the effect on training reports that higher minimum wages are associated with reduced training for black males but not for white males (Leighton and Mincer 1981) while another, slightly less old, reports that men hired at the minimum wage receive the same amount of training as those hired at low wages (Sicilian and Grossberg 1993).

¹³ In August 2014, a web search on the (unquoted) phrase “minimum wage hurts minorities” returned nearly three-quarters of a million results.

FIGURE 3

RACE/ETHNIC COMPOSITION OF CUMULATIVE WAGE DISTRIBUTION: WAGE BANDS RELATIVE TO THE EFFECTIVE MINIMUM WAGE



than 1.1 times, 30 percent no more than 1.25 times, and nearly half no more than 1.5 times the minimum wage. Hispanics comprise 15 percent of the employed and 20 percent of the unemployed labor force, as well as a quarter of the lower tail of the wage distribution for each of the cut-offs considered. Compared to Hispanics, blacks are a smaller percentage of the employed, and a larger percentage of the unemployed, labor force, and comprise a smaller proportion of the lower left tail of the wage distribution, ranging from two-thirds that of Hispanics at very low wages to four-fifths at merely low wages (Table 1, line 3b). As part of the labor force, blacks are a smaller share of the employed than Hispanics (11 percent), an equal share of the unemployed (20 percent), and make up a fairly stable 13–14 percent of low-wage employment.

Figure 3 breaks out the shares of Hispanics and blacks in low-wage employment and is reminiscent of Figure 1, which breaks out teenagers and young adults. In each figure, the identified groups are substantial minorities of low-wage employment, but in neither do they sum to form a majority of low-wage employment.

Most analyses of the effect of the minimum wage on blacks and Hispanics have focused on teenagers, but there are exceptions. In their analysis of the

interplay between the earned income tax credit (EITC) and the minimum wage, Neumark and Wascher (2011) separately examine the effects on single black and Hispanic mothers, aged 21–44, on childless black and Hispanic individuals aged 21–34 with no more than a high school degree, and on the male subset of the latter group. Carrington and Fallick (2001) break out blacks in their study of the low-wage workforce in the decade following completing schooling; Long (1999) does the same for both blacks and Hispanics in his study of minimum wage earners in the early 1990s. Leighton and Mincer (1981) distinguish between black and white men in their study of the minimum wage and on-the-job training.

Employment, the level and growth of wages and earnings, human capital formation, and measures of economic well-being related to poverty are among the consequences of the minimum wage that have been studied with regard to racial and ethnic minorities. Starting with employment, Allegretto, Dube, and Reich (2011) report that higher minimum wages do not affect the employment of Hispanic or black teens, but that the hours of Hispanic teens decline. Neumark and Wascher (2011) find that the minimum wage and EITC together increase employment of single black or Hispanic mothers (aged 21–44), but reduce employment of childless blacks or Hispanics (aged 21–34) with no more than a high school degree. Using a search model to study the labor market experience of southern teenage boys, Ahn et al. (2010) conclude that although the unemployment rate is similar for 17-year-old white and 19-year-old black males, the minimum wage is the source of unemployment for whites but firms' perception of a skills mismatch is the source for blacks. Neumark and Nizalova (2007) report that minimum-wage increases result in long-term reductions in employment and hours, leading to scarring of black teenagers and young adults relative to their white counterparts.¹⁴

Effects on wages and earnings are similarly mixed. Allegretto, Dube, and Reich (2011) report that increases in the minimum wage raise the earnings of white and black teens, but not the earnings of Hispanic teens. Neumark and Wascher (2011) find a strong, positive combined effect of the EITC and minimum wage on the earnings of single black and Hispanic mothers, aged 21 to 44; a small negative effect of the minimum wage by itself (that almost exactly cancels its positive effect for the whole sample) on wages of childless black and Hispanics, aged 21–34, with no more than a high school degree; and large negative effects of the minimum wage and EITC in combination on earnings

¹⁴ The authors posit that scarring (which is supposed to affect individuals years after their experience with a high minimum wage) is the result of reductions in contemporaneous employment due to the minimum wage; yet the contemporaneous effect that they report for black teenagers is not close to statistical significance.

of this population. Neumark and Nizalova (2007) report that the minimum wage raises the contemporaneous wage of black teenagers, but scars both them and young black adults so that the wage and the weekly earnings of blacks in their late twenties are lower than they would be otherwise. Again, as noted in the previous footnote, the posited mechanism causing scarring is not consistent with their estimates.¹⁵

Little work has examined the consequences of the minimum wage for school enrollment by race, and what there is, is either old or has issues with reliable statistical inference. Leighton and Mincer (1981) is the most recent analysis that examines the effect on training and allows for differences across races (they report evidence of a reduction for black men when they examine the National Longitudinal Survey of Youth (NLSY) data from the late 1960s, but not for either black or white men using data from the Panel Study of Income Dynamics). There is next to no work that examines the effect on income, inequality, or poverty by race; Sabia and Nielsen (2015) is an exception, breaking out the effects of the minimum wage on several measures of poverty and financial or material hardship for young blacks (aged 16–24). In general they find no effect of the minimum wage on these measures of economic well-being overall or specifically for young blacks. Similarly, they report that the minimum wage does not have a positive or negative effect on individuals' likelihood of being in poverty or being in financial or material hardship.

Several topics remain to be explored in greater depth. First, there are hints of different consequences of the minimum wage for Hispanics and blacks. This appears both in the CPS data tabulated in Table 1, in which Hispanics are more likely to be in minimum-wage or close to minimum-wage jobs than blacks, and in the research which distinguishes between Hispanics and blacks. In that research, hours are more likely to be negatively affected and earnings are less likely to be positively affected for Hispanics. None of the research addresses reasons for this; the reasons may be related to the immigration status of part of the Hispanic labor force, language issues, or relative levels of human capital. The work of Neumark and Wascher (2011) also suggests a complex pattern of outcomes within racial and ethnic groupings, as well as interactions between the minimum wage and the EITC. This sophistication provides useful insights and should, where appropriate, be followed in future research.

¹⁵ Long (1999) reports results for wage growth of blacks and Hispanics who initially earn the minimum wage, and Carrington and Fallick (2001) perform a similar analysis for blacks in the years immediately after leaving school. Both are descriptive; neither asserts causality running from the minimum wage to patterns of wage growth.

By education. An implication of human capital theory is that those with low levels of education are more likely to be employed at low-wage levels, especially at or near the minimum wage. The cross-tabulation of educational attainment and wage levels in rows 4a–4c of Table 1 is consistent with this.¹⁶ Likely as a result, a number of analyses have examined how educational attainment moderates the consequences of the minimum wage.

What story do these education-by-wage cross-tabs tell? The general pattern in the row percentages is that those with no college have lower employment rates and are considerably more likely not to be in the labor force than the population as a whole. Workers affected by the minimum wage are very disproportionately those who did not complete high school, and disproportionately those who completed high school but either had no college experience or did not complete a degree. The column percentages indicate that those without a high school diploma make up the group most concentrated in low-wage jobs but do not make up the largest group of low-wage employees. That group, which is much larger by share of employment, consists of those whose formal education ended with completion of high school. Figure 4 reinforces this point and clearly indicates the strong connection between less education and low-wage employment. Those without any postsecondary education comprise more than one-half of those in low-wage jobs; only one-fifth of those in low-wage jobs have a postsecondary degree of any kind.

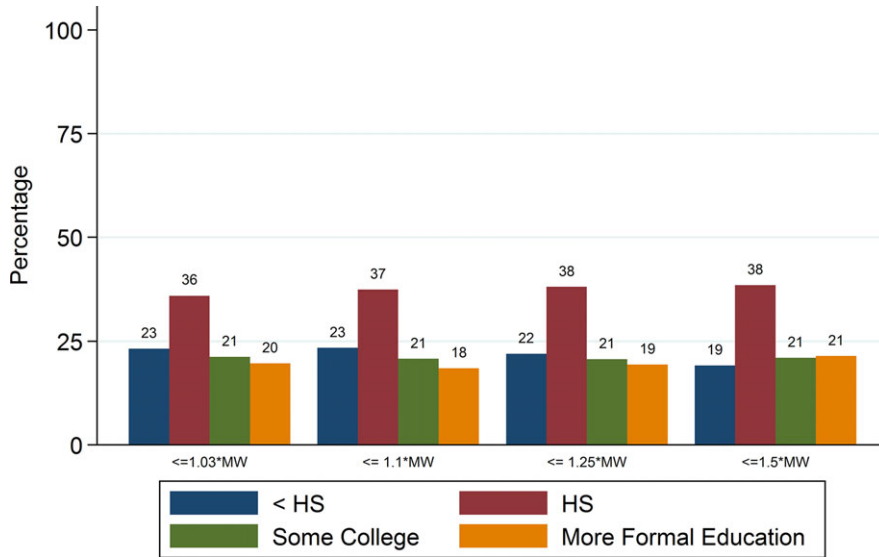
Reversing the usual focus of minimum-wage research on employment, seven studies consider the effect of the minimum wage on the compensation of individuals with limited education, while only four consider the effect on employment and hours. The definition of lesser educational attainment varies considerably among studies. Whereas Sabia (2008) and Orrenius and Zavodny (2008) distinguish between those with and without a high school degree, Neumark and Wascher (2011) draw the line at whether an individual's education went beyond high school and Eckstein, Ge, and Petrongolo (2011) study only individuals whose education ended with a high-school degree.

Evidence about consequences of the minimum wage for employment and hours of less educated individuals is mixed. According to Sabia (2008), there is a negative employment and hours response for single mothers with less than a high-school degree, but no effect for single mothers who have completed high school. Orrenius and Zavodny (2008) find that minimum-wage increases do not affect the employment of men or women with less than a high school education regardless of their immigration status. Eckstein, Ge, and Petrongolo (2011) find a very small negative employment effect for white male high-

¹⁶ To avoid conflating those who have lower levels of final educational attainment with those who have not completed their education, the calculations exclude those who are enrolled in school.

FIGURE 4

EDUCATIONAL COMPOSITION OF CUMULATIVE WAGE DISTRIBUTION (NO STUDENTS): WAGE BANDS
RELATIVE TO THE EFFECTIVE MINIMUM WAGE



school graduates who have been in the labor force more than 1 year. In Neumark and Wascher's (2011) complex specification, the minimum wage appears by itself, interacted separately both with the EITC and with a dummy variable for low educational achievement, and with both variables together. Neither of the two interactions with low educational achievement is statistically significant although both are negative.

There is broad, if not universal, agreement that higher minimum wages are associated with higher wages and earnings for less educated workers. Eckstein, Ge, and Petrongolo (2011) report that the minimum wage elasticity of the average wage of white male high-school graduates is between 0.1 and 0.2, rising with the time since completing school. Orrenius and Zavodny (2008) report a positive effect for immigrants with less than a high-school education, but no effect for the similarly educated native born. Sabia (2008) reports a positive elasticity close to one for single mothers with less than a high-school education, but no effect for those with at least a high-school degree. Krashinsky (2008) finds that the decline in the real minimum wage between the 1970s and 1990s reduced the earnings of white men with a high-school degree or less, and Easton (2006) reports that higher minimum wages and the concomitant wage growth had a positive effect on the earnings of those with no more

than a high-school education. It is difficult to determine the effect of the minimum wage on wages and unconditional earnings in Neumark and Wascher's (2011) complex specification. Estimates of the direct effects on both wages and earnings are consistently large and statistically significant, but those for interactions with the EITC and less educated dummy vary in sign and significance. The net effect of a 10-percent increase in the minimum wage on earnings, inclusive of those without labor earnings, is not statistically significant. Autor, Katz, and Kearney (2008) find that a minimum wage which is 10-percent higher reduces the ratio of college graduates' wages to those of high school graduates' by less than 1 percent, less than one-tenth the magnitude of the effect of aggregate labor-supply conditions.

Do employers offset a higher minimum wage with reductions in benefits? Simon and Kaestner (2004) find no evidence of reductions in health or pension coverage among less-educated workers. Marks (2011) reports that higher minimum wages are associated with reduced employer-provided health insurance, but not pension coverage, for those who are employed by small firms and lack a high school degree.

By part time/full time. The only study that distinguishes between effects on full- and part-time workers is Connolly and Gregory's (2002) analysis of the 1998 implementation of the national minimum wage in the United Kingdom. They report that it did not reduce the hours of part-time or full-time workers, or affect the transition between full- and part-time status.

Examination of CPS data suggests that this is a gap in the literature where further research is likely to be productive. Table 1 displays the wage distributions of part-time workers and the small fraction who work variable hours, both when students are included (lines 5a and 5b) and when they are not (lines 6a and 6b).¹⁷ Part-time workers make up 18 percent of all employees (Table 1, line 5a); 17 percent to 27 percent of part-time workers earn very low wages and 47 percent to 62 percent earn low or very low wages. Not only do a large share of part-time workers earn low or very low wages, half of those who earn very low wages report working part time, and they are also 38 percent to 45 percent of those who earn low wages.¹⁸ At 4 percent, variable hours workers

¹⁷ Workers are classified as full time if they report averaging 35 hours per week or more, part time if they work fewer than 35 hour per week on average. Those who report that their hours vary are placed in their own category.

¹⁸ Excluding students from consideration thins the lower end of the part-time workers' wage distribution (line 6a), but not by much; it remains the case that more than half of nonstudent part-time workers earn less than 1.5 times the minimum wage. Because students are more likely to work part time, the share of part-time workers in low-wage jobs drops by about 20 percent (not percentage points) at each cutoff in the distribution.

are a much smaller share of all employees, and slightly smaller shares of these workers earn very low or low wages than is true of part timers (Table 1, lines 5b and 6b). Variable-hours workers comprise about one-tenth of those who earn low wages.

By industry. Although studying the minimum wage through its effects on specific demographic groups is the most common approach, an alternative is to study a specific industry or industries. For much the same reason that teenagers and young adults are the most frequently studied demographic groups, the Restaurant and Retail sectors, which have disproportionately high representation among low-paid workers, are the most studied in this approach.¹⁹ Between 28 percent and 41 percent of those employed in the Restaurant sector (“Food Services and Drinking Places”) earn very low wages and between 61 percent and 74 percent earn no more than low wages (Table 1, line 7a). Although the sector is only 6 percent of total employment, it comprises between 27 percent and 29 percent of those earning very low wages and between 17 percent and 21 percent of those earning no more than low wages. Employment in the Retail sector is less concentrated at very low wages, but between one-third and one-half of retail workers earn no more than low wages (Table 1, line 7c). Employment in the Retail sector is 12 percent of the total, but low-wage and very-low-wage employment is about 20 percent of those totals.²⁰

Thirty-two studies of specific industries and of groups of low-wage industries have appeared in recent years. Nineteen have examined the effect of the minimum wage on restaurants. Employment is studied most frequently (14 analyses), followed by wages (9), prices (5), gross employment flows (4), hours (3), and benefits (2).

Studies of the employment response, with findings ranging from none to a decline, deliver no consistent message. Six report no effect (Addison, Blackburn, and Cotti 2012; Dube, Lester, and Reich 2010, 2011, 2012; Dube, Naidu, and Reich 2007; Hirsch, Kaufman, and Zelenska 2011), three report mixed effects (Addison, Blackburn, and Cotti 2013; Persky and Baiman 2010; Ropponen 2011), and four report negative effects (Aaronson and French 2007; Even and Macpherson 2014; Powers 2009; Singell and Terborg 2007). Skedinger (2006) reports a negative effect for the Restaurant and Hotel sectors in Sweden, which had a negotiated rather than legislated minimum wage. Singell

¹⁹ The older Standard Industrial Classification system used the term “Eating and Drinking Places.” For restaurants, the North American Industrial Classification System uses “Food Services and Drinking Places.” We use the word “restaurant” for both.

²⁰ Because several studies in recent years have also considered the Accommodations industry, we include those figures as well without discussion.

and Terborg (2007) examine the employment response of the hotel sector and report a positive effect on employment, but discount it. Three studies of hours report no effect (Even and Macpherson 2014; Persky and Baiman 2010; Powers 2009).²¹

The minimum wage is universally found to raise wages in the Restaurant industry; all nine analyses report a positive effect (Addison, Blackburn, and Cotti 2012, 2013; Anderson and Bodvarsson 2005; Dube, Lester, and Reich 2010, 2011, 2012; Dube, Naidu, and Reich 2007; Even and Macpherson 2014; Hirsch, Kaufman, and Zelenska 2011). Dube, Naidu, and Reich (2007) find larger increases in restaurants that have larger fractions of bound workers. Hirsch, Kaufman, and Zelenska (2011) report that minimum wage increases result in wage compression as those who had been earning above the new minimum realize smaller wage gains than those below. Considering wage norms, Spriggs (1993–1994) finds that differences in human-relations strategies are correlated with the effects of the minimum wage on a restaurant's wage structure.

The five analyses that examine the response of restaurant prices to the minimum wage are generally consistent despite some minor ambiguity about the effect. Aaronson (2001) examines price indices for the United States and Canada, and price data for three fast-food chains; the price indices clearly show a response but results from the chain-level data are murky, with McDonald's hamburgers responding strongly while prices at KFC and Pizza Hut show more variation in response. MacDonald and Aaronson (2006) study less aggregated U.S. price indices than Aaronson (2001); Fougère, Gautier, and Le Bihan (2010) perform a similar analysis of data for the French Restaurant sector. Both report a good deal of price rigidity, and MacDonald and Aaronson (2006) report that a minimum-wage increase of 10 percent roughly doubles the probability that the price of an item at a fast-food restaurant will increase in the same month. Both studies report elasticities, conditional on an increase, in the range of 0.10. Examining the response of restaurant prices to the 2004 increase in the San Francisco minimum wage, Dube, Naidu, and Reich (2007) report higher prices for the fast-food sector but not the full-service sector. Finally, Lee, Schluter, and O'Roark (2000) conduct an input-output analysis of the food sector that allows them, in combination with strong assumptions about technology and profit shares, to estimate a ceiling on the elasticity of restaurant prices with respect to the minimum wage of about 0.1.

²¹ Our meta-analysis finds no effect on employment and hours in Restaurants in the United States but a small, statistically significant negative effect when the only international study, Skedinger's (2006) study of Sweden, is included.

Turning to gross employment flows, Dube, Naidu, and Reich (2007) find no effect on separations; Dube, Lester, and Reich (2011, 2012) report a drop in the rates of both accessions and separations; and Skedinger (2006) reports a drop in the former and a mixed effect on the latter. Two studies have considered the effect on employee benefits in the industry; Dube, Naidu, and Reich (2007) also find no decline in health insurance coverage in response to increases in the minimum wage, while Alpert (1986), using now-dated methods, reports a decline in coverage for four of twenty benefits.

The balance of studies consider a mixed set of industries. For the United States, the focus of these studies has been the Retail sector, not surprising given the concentration of low-paid workers in retail. It should also be no surprise, given the relatively higher wage distribution in this sector, that the results for any effects there are somewhat weaker than for the Restaurant sector. Some U.S. studies consider several specific retail industries (Addison, Blackburn, and Cotti 2009), others the Retail sector as a whole (Sabia 2009a); some a specific retailer (Giuliano 2013) and still others consider low-wage industries without regard to sector (Belman and Wolfson 2010; Wolfson and Belman 2001, 2004). Of the nine articles that measure the effect on employment or hours, three find a negative effect (Orazem and Mattila 2002; Sabia 2009a, 2009b); four find no effect (Addison, Blackburn, and Cotti 2009; Belman and Wolfson 2010; Potter 2006; Wolfson and Belman 2001); one finds a positive effect (Giuliano 2013); and one finds a mix, with a preponderance of no effect (Wolfson and Belman 2004).

The preponderance of the evidence concerning the effect on wages or earnings in retail is positive, but not uniformly so (Addison, Blackburn, and Cotti 2009, 2010; Belman and Wolfson 2010; Giuliano 2013; Orazem and Mattila 2002; Sabia 2009a; Wolfson and Belman 2001, 2004). Addison, Blackburn, and Cotti (2009) report that increases in the minimum wage result in increases in the average wages of convenience stores, specialty food stores, liquor stores, sporting goods stores, general merchandise stores, department stores, miscellaneous retailers, and gasoline stations, but not in grocery stores or food-and-beverage stores. When they divide the sample between states with and without right-to-work laws, the minimum wage consistently has a positive effect on retail earnings, including grocery stores and food-and-beverage stores, in the states with right-to-work laws. There is no earnings effect in grocery stores or food-and-beverage stores in states without right-to-work legislation. Wolfson and Belman's (2001, 2004) work finds minimum-wage increases result in higher average wages for 70 percent of the industries in their sample.

Little research exists concerning consequences of the minimum wage for other dimensions of employment. In their analyses of the Economic Opportunity Pilot Project, a survey of three thousand small- and middle-sized firms in

the Midwest and South in which small and low-wage firms were oversampled, Grossberg and Sicilian (1999; Sicilian and Grossberg 1993) report that individuals hired at exactly the minimum wage are more likely to quit their jobs than those hired at low but above minimum wages. Men hired at the minimum in these firms have lower rates of wage growth, but women's wage growth is not affected. Being hired at the minimum wage does not affect training for either gender.

Studies of other countries are consistent with U.S. studies in finding that higher minimum wages result in higher wages in home care and in agriculture, as well across more broadly defined sectors. Evidence of negative employment effects is stronger in these studies than in the U.S. studies. Machin and Wilson (2004) and Machin, Manning, and Rahman (2003) report negative employment effects for the home-care industry in the U.K. Georgiadis (2013) reports that the minimum wage does not affect the accession or separation rates in home care in the U.K.

Effects on Earnings, Family Income, and Poverty

The central purpose of the minimum wage is the improvement of the conditions of low-wage workers.²² A key matter is whether the minimum wage is sufficiently well targeted that the increase goes not just to workers with low earnings but mainly to low-income individuals and households. Critics argue that this is not the case since many of those earning the minimum wage are teenagers and young adults from higher income households.

For the employed, there is a broad positive association between an individual's place in the wage distribution and family income; individuals from families in the lowest quartile of the income distribution are substantially over-represented among those working at or close to the minimum wage. Lines 8a–8c of Table 1 show the family incomes of earners from the lower part of the family income distribution. Ten percent and 15 percent of employed individuals, respectively (25 percent altogether), belong to the first two family income bands: *Less than \$20,000 per year* and *at least \$20,000 per year and less than \$35,000 per year*. In combination with the third band, *at least \$35,000 per year and less than \$50,000 per year*, they contain 49 percent, just under half, of all earners.

²² Section 2 of the Fair Labor Standards Act states: (A) The Congress hereby finds that the existence, of industries engaged in commerce or in the production of goods for commerce, of labor conditions detrimental to the maintenance of the minimum standard of living necessary for health, efficiency, and general well being of workers. . . .”

Two facts leap out immediately from lines 8a–8c. The first is the strong positive association between family income and the employment rate, and, related to this, the very low employment rate for those in the lowest category of family income. Individuals aged 16 and older whose families are in the bottom income category are employed at much lower rates than their counterparts in less impoverished families. The second is that although the fraction that is unemployed is higher in these very poor families, the fraction that is not in the labor force is much higher, about 20 percent (not percentage points) higher than for the next income category and 60 percent higher than for the next income category above that. In the lower half of the family income distribution, both employment and labor-market participation have a very strong positive association with income.

Individuals in the lowest family income category find themselves in very-low-wage jobs at about 2.5 times the rate as all earners, and in all low-wage jobs at more than two times the rate (Table 1, line 8a). Considerably more than half of all employed individuals from the lowest family income category (62 percent) work in low-wage jobs. Forty-four percent of individuals in the next income category (between \$20,000 and \$34,000) are employed in low-wage jobs, about 1.5 times the rates for all earners (Table 1, line 8b). In the highest of these three income categories, the rates are close to, and even occasionally less than, economy-wide averages (Table 1, line 8c). Turning to the “Column Percentages”, we see that although individuals from the lowest income category make up only 10 percent of employment, they make up 25 percent of those in very-low-wage jobs, and more than 20 percent of those in all low-wage jobs. Employed individuals in the next income category make up 15 percent of employment and slightly more than 20 percent of employment in both very-low-wage jobs and all low-wage jobs. Individuals from these two lowest categories of family income make up only one-quarter of employment but almost half of employment in very-low-wage or all low-wage jobs. Individuals in the third income category are distributed in low-wage and very low positions in very close proportion to their employment share.

The effect of the minimum wage on low-wage populations. How then does the minimum wage affect the economic condition of the low-wage/low-income populations it was intended to aid? 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.

Eight studies of employment, most of data from the U.K., have focused specifically on low-wage workers. Luttmer (2007) reports that increases in the U.S. federal minimum wage between 1989 and 1992 reduced employment among the lowest wage group but increased employment among the second lowest wage group, the low skilled. He suggests that there would be no net employment effect for a more broadly defined low-wage group. The six studies of the U.K. examine employment outcomes of bound workers, those whose current wage is less than the enacted minimum wage. Three report no employment effect for bound workers (Mulheirn 2008; Stewart 2002, 2004a); Connolly and Gregory (2002) find no hours effect for bound women working part or full time; and Dolton, Bondibene, and Wadsworth (2012) report mixed but mostly positive employment effects. Robinson and Wadsworth (2007) find no change in second job holding but a decline in hours on the second jobs of bound workers. While the U.K. studies find no evidence of employment, Laroque and Salanié's (2002) study of married French women from 1990 to 1998 report an elasticity of -0.7 for women bound by the new minimum.

Is a higher minimum wage associated with higher earnings among low-wage workers? Ten of eleven studies using U.S. data report a positive effect on absolute or relative wages (Aaronson, Agarwal, and French 2011; Autor, Katz, and Kearney 2008; Autor, Manning, and Smith 2010; Card and DiNardo 2002; Dinardo, Fortin, and Lemieux 1996; Lee 1999; Lemieux 2002, 2006; Luttmer 2007; Reich and Hall 2001). Studies of wage inequality consistently find that higher minimum wages are associated with reduced wage inequality; the magnitude of the effect remains in play. Analyses of data from the U.K. also report that higher minimum wages raise the earnings of bound workers (Lam et al. 2006; Dolton, Bondibene, and Wadsworth 2012; Stewart 2004a) and evidence of a spillover effect for workers who are not bound by the new minimum wage (Lam et al. 2006). The sole negative effect with regard to wages covers the late 1970s and early 1980s when legislated increases in the minimum wage failed to keep pace with price and wage inflation (Currie and Fallick 1996). The importance of the minimum wage is closely connected to how long individuals earn the minimum wage or close to the minimum wage. If a large proportion of those currently employed at the minimum wage rapidly move to substantially higher earnings, the level of the minimum wage is less important than if they remain at or near the minimum wage.²³ Carrington and

²³ Smith and Vavrichek (1992), Long (1999), and Even and MacPherson (2003) estimate the likelihood of minimum wage earners remaining at the minimum wage in future years. Their results are less useful than Carrington and Fallick (2001) because they divide the world between those earning exactly the minimum wage and all other employees, and because they only have data on individuals for one or two years after observing an employee at the minimum wage.

Fallick (2001) provide the richest analysis of the careers of minimum-wage earners using the NLSY to follow individuals for 10 years following completion of their schooling. Using a broad definition of minimum-wage worker, individuals earning within 25 cents of the minimum wage, and tracking how individuals move through wage bands up to \$2.00 per hour above the minimum wage, Carrington and Fallick (2001) report

Most workers who begin their careers in minimum wage jobs eventually gain more experience and move on to higher paying jobs; some workers spend substantial portions of their early careers consistently working in minimum wage jobs.

Grossberg and Sicilian (1999) report that men, but not women, hired at the exact minimum wage have lower wage growth than men hired at low but above minimum wages. They also find being hired at the minimum wage does not affect the training received by men or women. In their earlier work with the same data, Sicilian and Grossberg (1993) found that higher minimum wages are associated with reduced quits.²⁴ In later work using data from the NLSY, they confirmed this for women but reported a mixed effect for men (Grossberg and Sicilian 2004).

Effects on earnings, family income, and poverty. Research on the effect of the minimum wage on the incomes of low-income populations is problematic because of conceptual and methodological issues. Of the sixteen analyses, most investigate whether higher minimum wage levels raise individuals and families out of poverty. As we discuss in our book (Belman and Wolfson 2014), this narrows the focus to a population, which, because of its limited involvement with employment and the labor market, is unlikely to be affected by the minimum wage. Such research largely misses the effect on the incomes of low-income families with members who are employed.

Most of these studies use various forms of state-by-year data structures and, due to their vintage, do not correct for the likelihood of biased standard errors. The four remaining studies (Dube 2014; Sabia 2008; Sabia and Burkhauser 2010; Sabia and Nielsen 2015) reach contradictory conclusions about the effect of the minimum wage on poverty and low-income families. Considering total income including transfer payments, Sabia (2008) finds that the minimum wage affects neither the probability of being in poverty nor the income-to-poverty ratio, both for all women and for women with less than a high-school education. Broadening the research to the effect on post-transfer income for all

²⁴ Similar to Currie and Fallick's (1996) anomalous result for wages, the analysis of Sicilian and Grossberg (1993) relies on data from a period of high inflation.

families, Sabia and Burkhauser (2010) also find no effect on families' being in or near poverty. Sabia and Nielsen (2015) use the Survey of Income Program Participation (SIPP) to estimate the effect of the minimum wage on poverty levels, on material hardship, and on the income-to-needs ratio of a variety of groups: workers age 16 to 64, the employed, those age 16 to 29 without a high-school diploma, and blacks age 16 to 24. The authors find little to no evidence for a minimum-wage effect, although there is some evidence that 16–to–29-year-olds without a high-school diploma are more likely to be in or near poverty when minimum wages are higher. Dube (2014) uses the technique of re-centered influence factor regression with the 1990 to 2012 March CPS to examine the effect of the minimum wage on the income-to-needs ratio. Although he reports no effect on the income-to-needs ratio using Sabia and Burkhauser's (2010) specification, addition of divisional time trends, state recession fixed effects, or state linear trends to the model results in a positive and statistically significant relationship between the minimum wage and family income for income-to-needs ratios in the interval from 0.5 to 1.25.²⁵

Overview

What have we learned by viewing the minimum wage literature through the lens of “who?” rather than “what?” Table 2 presents a summary of results with an object of study (who?) in each row and the most commonly studied effects (what?) in the columns. Turning first to teenagers (the smallest but most studied of all groups considered) and young adults, estimates of the employment effect are about equally split between a negative effect (11 analyses) and a range that takes in no effect (7 analyses), a positive effect (2 analyses), and a mixed set of effects with results varying by sample, specification, or the specific subgroup examined (6 analyses). In combination with the meta-regression estimates, a reasonable conclusion is that there is either a *de minimis* effect or no effect on employment in the United States (and perhaps a negative effect in other developed countries). Raising the minimum wage reduces the accession rate of teenagers and young adults, and perhaps the separation rate as well. The minimum-wage effect with the strongest supporting evidence is that of raising employed teenager's wages (five analyses with positive effects versus two with mixed effects)—not surprising given the concentration of teen work-

²⁵ These studies provide a framework and direction for further research, but do not resolve the issue of the relationship between the minimum wage and family income. Resolution of technical issues with differences between the CPS and SIPP, as well as the need to better understand the effect of the minimum wage on earned as well as post-transfer income, remain open.

TABLE 2
THE BOX SCORE: A TABULATION OF RESULTS BY GROUP AND OUTCOMES

	Employment (+, -, 0, mixed)	Hours (+, -, 0, mixed)	Wages and Earnings (+, -, 0, mixed)	Poverty & Inequality* (+, -, 0, mixed)
Age				
Teens and other young workers	(2, 11, 7, 6)	(1, 4, 2, 1)	(5, ., ., 2)	(., ., ., .)
Gender				
Men	(1, 1, 5, .)	(., 1, 1, .)	(8, ., 1, .)	(., 3, 1, ., .)
Women	(., 1, 4, 3)	(., ., 1, 2)	(9, ., 1, 1)	(1, 4, ., .)
Race/Ethnicity				
Black	(., ., 2, 1)	(., .1, .)	(1, ., ., 2)	(., ., 1, .)
Hispanic	(., ., 1, 1)	(., 1, ., .)	(., ., 1, 1)	(., ., ., .)
Vulnerable Groups				
Low education	(., 2, 2, .)	(., 1, 1, .)	(5, ., 2, 1)	(., 1, 1, .)
Part time	(., ., ., .)	(., ., 1, .)	(., ., ., .)	(., ., ., .)
Industries				
Food service	(., 4, 6, 3)	(., ., 3, .)	(9, ., ., .)	(., ., ., .)
Retail and other industries	(1, 3, 5, .)	(., ., ., .)	(7, ., ., 1)	(., ., ., .)
Low Wage/Low Income				
	(1, 1, 5, 1)	(., ., ., .)	(14, 1, ., .)	(., 9, 3, .)

NOTES: The rows of the table are the group of interest, the columns are the four most commonly estimated outcomes. The numeric entries in each cell are the number of articles reporting an outcome of a particular sign. The first entry is the number of articles that report positive outcomes that pass a two-tailed, 5%-significance test. The second entry is the number of articles that report negative outcomes that pass a two-tailed, 5%-significance test. The third is the number of articles reporting outcomes that do not pass a 5%, two-tailed test. The fourth and final is the number of articles reporting mixed, some combination of positive, negative and nonsignificant outcomes.

*A negative result is associated with a reduction in poverty and/or inequality

ers at very low wages. No current research parses the effect on young workers between an effect on bound workers and a spillover effect. Evidence on the schooling and training of young workers leans toward there being no effect, but results are mixed.

Turning toward effects on each gender, a majority of studies of women find no effect on employment, but some report that women with less education suffer reduced employment as a result of higher minimum wages. What evidence exists for a negative employment effect among men suggests that adjustments come through reductions in hours. There is strong, if not universal, evidence that higher minimum wages raise wages in the lower tail of the distributions of both men's and women's wages. Evidence on other outcomes such as wage growth and training is too limited and mixed to support any conclusions. A

concern in research by gender is that broad divisions of the workforce include too many individuals not affected by the minimum wage to detect minimum wage effects. For example, the lack of employment effects in most full gender samples may be due to the majority of the sample working at sufficiently high wages as to be unaffected by the minimum wage. The almost universal finding of wage effects for both genders may lessen this concern.

Blacks and Hispanics are two groups often cited in statements of concern about disparate effects of the minimum wage. They make up slightly less than one-ninth and one-sixth of employment, respectively, and disproportionately earn low wages, though whites are a majority in each of the low-wage bands that we defined. Making sense of this body of research is difficult, not only because both the populations examined and the results vary considerably across studies, but also because surprisingly little research considers the effects of the minimum wage on racial and ethnic minorities. The only evidence for employment effects that inspires confidence cuts both ways, that of Neumark and Wascher (2011); the EITC and minimum wage together stimulate employment of single black and Hispanic mothers, but this is at least somewhat offset by negative effects on the employment of low-skilled childless blacks and Hispanics. It appears that the minimum wage leads to higher wages for black and Hispanic teenagers, and, for single black and Hispanic mothers in combination with the EITC, but not for low-skilled childless blacks and Hispanics.²⁶ In a study of the restaurant industry Spriggs (1993–1994) concludes that restaurants that hired a larger share of African Americans minimized the cost of wage increases by limiting increases to employees who previously were paid less than the new minimum (p. 223). The unstated implication is that these employees were at the bottom of the wage distribution and immediately affected by an increase in the minimum wage. This topic is one where the cliché “more research is needed” applies, and there are indications that it is important to distinguish between blacks and Hispanics. Finally, the (admittedly very thin) literature provides no evidence that on average the minimum wage has effects on training, school enrollment, poverty status, or financial or material hardship of any group of blacks or Hispanics.

Studies of consequences for those with less education typically involve a binary distinction between less educated and more educated, with little agreement on the classification of those who completed high school and went no further. Comparison of outcomes is therefore difficult. The few studies of the employment effects on less educated workers suggest small or

²⁶ Neumark and Nizarova (2007) assert that it leads to lower wages for blacks some years later, but, as we discuss, their evidence undercuts their hypothesized transmission mechanism.

no effect for broad segments of that group but there is evidence of adverse consequences for single mothers with less than a high-school degree. With respect to compensation, the evidence suggests that higher minimum wages are usually but not universally associated with higher wages and earnings for those with less education. With one exception, studies of broad populations report a positive effect on less educated groups, as do studies from more narrowly defined targets, also with a single exception. As for benefits, available evidence indicates that the minimum wage does not affect pension coverage, but reduces health coverage for employees of small firms who have not completed high school.

In the United States, the most studied industry is Restaurants, followed by Retail, while in the U.K., the industry of choice is the Home Care sector. For the U.S. Restaurant industry, estimates of the employment effect are mixed, including four each of no effect and of a negative effect, and three reporting mixed effects. The far fewer studies of hours of employment agree on no effect. All studies of restaurant wages agree that the minimum wage increases them, and that is also the case for prices, at least in the Fast-Food sector. There is limited but clear evidence that the minimum wage reduces the accession rate, and contradictory evidence about separations. The evidence on employment and wages in the Retail sector is generally similar but in each case, likely due to wages in retail being higher and thus less sensitive to the minimum wage, the evidence is weaker. Studies of the U.K. Home Care industry agree that the National Minimum Wage has reduced employment and raised wages, but the one study of gross flows reports no effect on separations or accessions.

The minimum wage was intended to improve the conditions of those working at low wages and from families with inadequate income. Consistent with this purpose, individuals from the (employee weighted) bottom deciles and quartile of the family income distribution are considerably more likely than those from the upper half and upper quartile to be earning the minimum wage or close to the minimum wage. British research on bound workers finds no employment effect; there is too little U.S. research to support a conclusion for this country. Extensive research on wage effects finds that higher minimum wages are associated with higher earnings and reduced income inequality in the United States and the U.K., although the magnitude of the effect in the United States is a subject of some disagreement. Laroque and Salanié's (2002) study of French married women reminds us of the importance of national institutions. While a majority of those earning the minimum wage move to higher wages fairly quickly, a substantial minority of individuals who have completed their schooling spend years working in positions which pay close to the minimum wage. Employment

at the exact minimum wage is reported to reduce wage growth among men, relative to men hired above the minimum wage, but women's wage growth is not affected. Whether the minimum wage affects the incomes of low-income families, one of the most important issues in assessing the effect of the minimum wage, has not been studied sufficiently. A simple count, three analyses reporting no effect on poverty and one reporting a decline in poverty, leans toward there not being an effect. Given the centrality of this issue, this would be amongst the most fruitful areas for future research.

What guidance for future research does organization by "who?" provide? It both highlights gaps in the literature and identifies groups and issues which have been thoroughly investigated. The effect of the minimum wage on teen employment has been well plowed; older employees, Hispanics, part-time employees, and low-wage employees are fallow (and thus fertile) fields for research. Similarly, U.S. literature has focused on employment effects, but, in contrast with research in the U.K., has been less concerned with effects on wages, earnings, and income. As earnings and income data are almost always available in the data used to study employment, broadening the issues under study will require modest additional effort.

Another step toward broadening research and making it more useful is more careful construction of comparison groups, a point that Kuehn (2014) has forcefully made. For example, in studies that focus on education level, there is no reason to use a bivariate division rather than several: viz., less than high school, high school and no more, some college, a college degree or more. This would provide better insights into the relationship between the minimum wage and education, facilitate comparison across studies, and would reduce the likelihood of false negatives or positives associated with comparison groups' being largely comprised of individuals who are unlikely to be affected by the minimum wage.

Perhaps the most important direction for future research is to focus on the low-wage/low-income groups that are the target for minimum-wage legislation. Young workers, women with less education, and minority groups are proxies for the group most in need of study. The primary effect of the minimum wage on employment, on earnings, on income, on education, and the other outcomes of enduring interest must be for individuals who are bound by a minimum wage change, whose earnings are close to the minimum wage, or who have low family income. Techniques that allow for estimation across the income and wage distribution, and that correct for endogeneity make it possible to estimate outcomes by wage and income, and provide more policy-relevant results than organization of research around groups which proxy for the primary group of interest.

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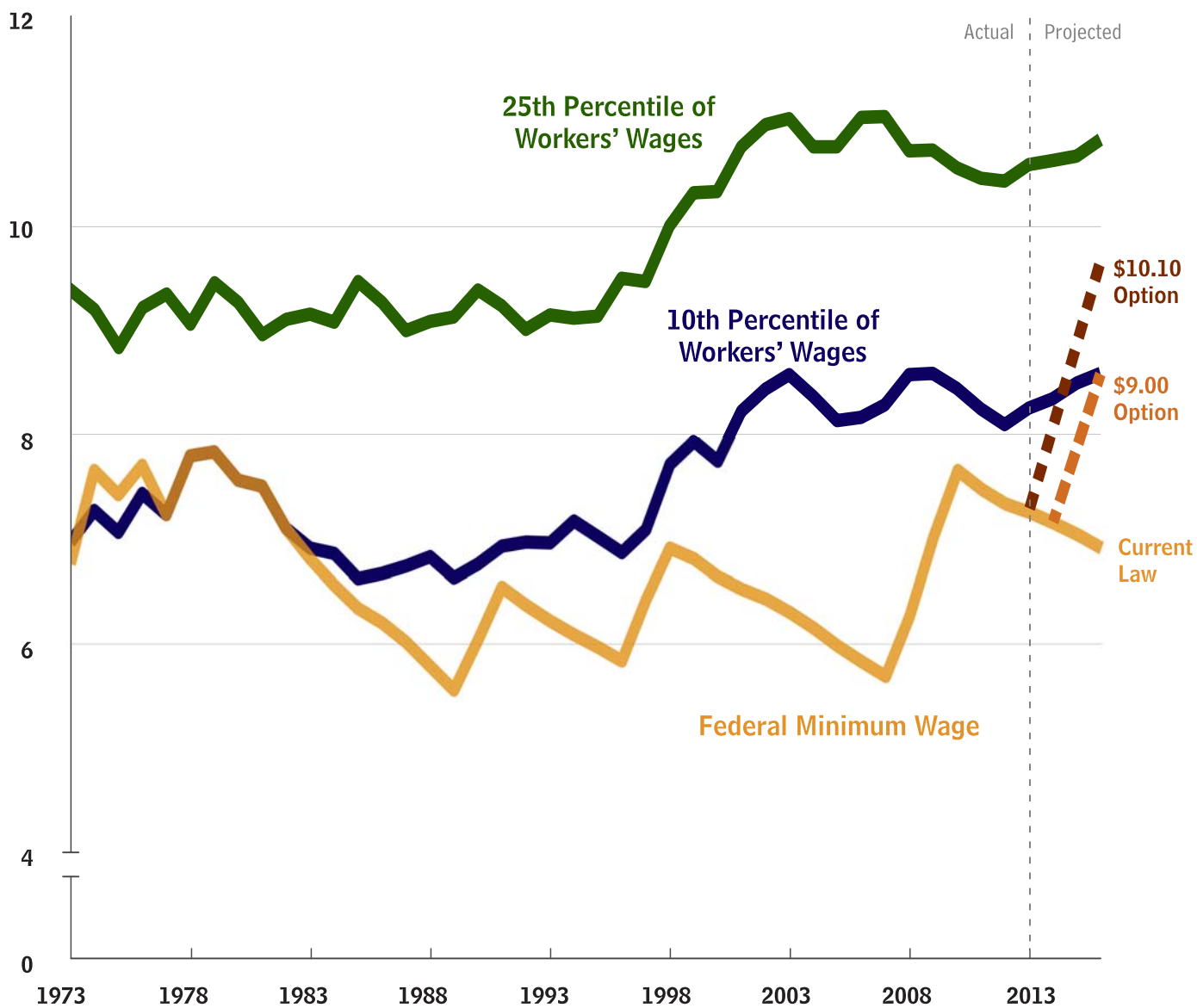
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CBO

The Effects of a Minimum-Wage Increase on Employment and Family Income

2013 Dollars per Hour



FEBRUARY 2014

Notes

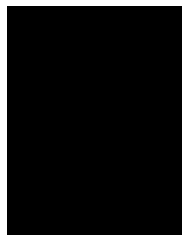
Estimates of the effect on employment of the options to increase the minimum wage are rounded to the nearest 100,000 workers.

Numbers in the text, tables, and figures may not add up to totals because of rounding.



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The Effects of a Minimum-Wage Increase on Employment and Family Income

Summary

Increasing the minimum wage would have two principal effects on low-wage workers. Most of them would receive higher pay that would increase their family's income, and some of those families would see their income rise above the federal poverty threshold. But some jobs for low-wage workers would probably be eliminated, the income of most workers who became jobless would fall substantially, and the share of low-wage workers who were employed would probably fall slightly.

What Options for Increasing the Minimum Wage Did CBO Examine?

For this report, the Congressional Budget Office (CBO) examined the effects on employment and family income of two options for increasing the federal minimum wage:

- A "\$10.10 option" would increase the federal minimum wage from its current rate of \$7.25 per hour to \$10.10 per hour in three steps—in 2014, 2015, and 2016. After reaching \$10.10 in 2016, the minimum wage would be adjusted annually for inflation as measured by the consumer price index.
- A "\$9.00 option" would raise the federal minimum wage from \$7.25 per hour to \$9.00 per hour in two steps—in 2015 and 2016. After reaching \$9.00 in 2016, the minimum wage would not be subsequently adjusted for inflation.

What Effects Would Those Options Have?

The \$10.10 option would have substantially larger effects on employment and income than the \$9.00 option would—because more workers would see their wages rise;

the change in their wages would be greater; and, CBO expects, employment would be more responsive to a minimum-wage increase that was larger and was subsequently adjusted for inflation. The net effect of either option on the federal budget would probably be small.

Effects of the \$10.10 Option on Employment and Income.

Once fully implemented in the second half of 2016, the \$10.10 option would reduce total employment by about 500,000 workers, or 0.3 percent, CBO projects. As with any such estimates, however, the actual losses could be smaller or larger; in CBO's assessment, there is about a two-thirds chance that the effect would be in the range between a very slight reduction in employment and a reduction in employment of 1.0 million workers (see Table 1).

Many more low-wage workers would see an increase in their earnings. Of those workers who will earn up to \$10.10 under current law, most—about 16.5 million, according to CBO's estimates—would have higher earnings during an average week in the second half of 2016 if the \$10.10 option was implemented.¹ Some of the people earning slightly more than \$10.10 would also have higher earnings under that option, for reasons discussed below. Further, a few higher-wage workers would owe their jobs and increased earnings to the heightened demand for goods and services that would result from the minimum-wage increase.

1. In addition to the people who became jobless, some workers earning less than \$10.10 per hour and not covered by minimum-wage laws would also not have increased earnings.

Table 1.**Estimated Effects on Employment, Income, and Poverty of an Increase in the Federal Minimum Wage, Second Half of 2016**

	\$10.10 Option^a	\$9.00 Option^b
Change in Employment		
Central estimate ^c	-500,000 workers	-100,000 workers
Likely range ^d	Very slight decrease to -1.0 million workers	Very slight increase to -200,000 workers
Number of Workers With Hourly Wages Less Than the Proposed Minimum Whose Earnings Would Increase in an Average Week ^e	16.5 million	7.6 million
Change in Real Income (2013 dollars, annualized) ^f		
Families whose income is below the poverty threshold	\$5 billion	\$1 billion
Families whose income is between one and three times the poverty threshold	\$12 billion	\$3 billion
Families whose income is between three and six times the poverty threshold	\$2 billion	\$1 billion
Families whose income is six times the poverty threshold or more	-\$17 billion	-\$4 billion
Change in the Number of People Below the Poverty Threshold ^g	-900,000	-300,000

Source: Congressional Budget Office based on monthly and annual data from the Census Bureau's Current Population Survey.

- The minimum wage would rise (in three steps, starting in 2014) to \$10.10 by July 1, 2016, and then be indexed to inflation.
- The minimum wage would rise (in two steps, starting in 2015) to \$9.00 by July 1, 2016, and would not be subsequently indexed to inflation.
- Uses values at or near the midpoints of estimated ranges for key inputs.
- In CBO's assessment, there is a two-thirds chance that the actual effect would be within this range.
- Some of the people with hourly wages slightly above the proposed minimum wage would also have increased earnings under the options.
- Changes in real (inflation-adjusted) income include increases in earnings for workers who would receive a higher wage, decreases in earnings for workers who would be jobless because of the minimum-wage increase, losses in income for business owners, decreases in income because of increases in prices, and increases in income generated by higher demand for goods and services.
- Calculated using before-tax family cash income. Poverty thresholds vary with family size and composition. The definitions of income and of poverty thresholds are those used to determine the official poverty rate and are as defined by the Census Bureau. CBO projects that in 2016, the poverty threshold (in 2013 dollars) will be about \$18,700 for a family of three and \$24,100 for a family of four.

The increased earnings for low-wage workers resulting from the higher minimum wage would total \$31 billion, by CBO's estimate.² However, those earnings would not go only to low-income families, because many low-wage workers are not members of low-income families. Just 19 percent of the \$31 billion would accrue to families with earnings below the poverty threshold, whereas

29 percent would accrue to families earning more than three times the poverty threshold, CBO estimates.³

Moreover, the increased earnings for some workers would be accompanied by reductions in real (inflation-adjusted) income for the people who became jobless because of the minimum-wage increase, for business owners, and for consumers facing higher prices. CBO examined family

2. All effects on income are reported for the second half of 2016; annualized (that is, multiplied by two); and presented in 2013 dollars.

3. Poverty thresholds vary with family size and composition; CBO projects that in 2016, the poverty threshold (in 2013 dollars) will be about \$18,700 for a family of three and \$24,100 for a family of four.

income overall and for various income groups, reaching the following conclusions:

- Once the increases and decreases in income for all workers are taken into account, overall real income would rise by \$2 billion.
- Real income would increase, on net, by \$5 billion for families whose income will be below the poverty threshold under current law, boosting their average family income by about 3 percent and moving about 900,000 people, on net, above the poverty threshold (out of the roughly 45 million people who are projected to be below that threshold under current law).
- Families whose income would have been between one and three times the poverty threshold would receive, on net, \$12 billion in additional real income. About \$2 billion, on net, would go to families whose income would have been between three and six times the poverty threshold.
- Real income would decrease, on net, by \$17 billion for families whose income would otherwise have been six times the poverty threshold or more, lowering their average family income by 0.4 percent.

Effects of the \$9.00 Option on Employment and Income.

The \$9.00 option would reduce employment by about 100,000 workers, or by less than 0.1 percent, CBO projects. There is about a two-thirds chance that the effect would be in the range between a very slight increase in employment and a reduction in employment of 200,000 workers, in CBO's assessment. Roughly 7.6 million workers who will earn up to \$9.00 per hour under current law would have higher earnings during an average week in the second half of 2016 if this option was implemented, CBO estimates, and some people earning more than \$9.00 would have higher earnings as well.

The increased earnings for low-wage workers resulting from the higher minimum wage would total \$9 billion; 22 percent of that sum would accrue to families with income below the poverty threshold, whereas 33 percent would accrue to families earning more than three times the poverty threshold, CBO estimates.

For family income overall and for various income groups, CBO estimates the following:

- Once the increases and decreases in income for all workers are taken into account, overall real income would rise by \$1 billion.
- Real income would increase, on net, by about \$1 billion for families whose income will be below the poverty threshold under current law, boosting their average family income by about 1 percent and moving about 300,000 people, on net, above the poverty threshold.
- Families whose income would have been between one and three times the poverty threshold would receive, on net, \$3 billion in additional real income. About \$1 billion, on net, would go to families whose income would have been between three and six times the poverty threshold.
- Real income would decrease, on net, by \$4 billion for families whose income would otherwise have been six times the poverty threshold or more, lowering their average family income by about 0.1 percent.

Effects of a Minimum-Wage Increase on the Federal Budget.

In addition to affecting employment and family income, increasing the federal minimum wage would affect the federal budget directly by increasing the wages that the federal government paid to a small number of hourly employees and indirectly by boosting the prices of some goods and services purchased by the government. Most of those costs would need to be covered by discretionary appropriations, which are capped through 2021 under current law.

Federal spending and taxes would also be indirectly affected by the increases in real income for some people and the reduction in real income for others. As a group, workers with increased earnings would pay more in taxes and receive less in federal benefits of certain types than they would have otherwise. However, people who became jobless because of the minimum-wage increase, business owners, and consumers facing higher prices would see a reduction in real income and would collectively pay less in taxes and receive more in federal benefits than they would have otherwise. CBO concludes that the net effect on the federal budget of raising the minimum wage would probably be a small decrease in budget deficits for several years but a small increase in budget deficits thereafter. It is unclear whether the effect for the coming decade as a whole would be a small increase or a small decrease in budget deficits.

The Current Federal Minimum Wage

The federal minimum wage was established by the Fair Labor Standards Act of 1938 (FLSA) and currently applies to about two-thirds of workers in the public and private sectors. Workers whose compensation depends heavily on tips (such as waiters and bartenders) are subject to a special arrangement: The regular minimum wage applies to their compensation including tips, and a lower cash minimum wage applies to their compensation excluding tips. The FLSA also has exceptions for workers and employers of certain types, including a provision permitting employers to pay teenage workers \$4.25 per hour during their first 90 days of employment.⁴

The nominal federal minimum wage has risen over the years. The most recent changes, which took effect in July 2007, raised the minimum wage in three steps from \$5.15 per hour (in nominal dollars) to \$7.25 in July 2009, where it stands today.⁵ However, the real value of the minimum wage has both risen and fallen, as the nominal increases have subsequently been eroded by inflation (see Figure 1).⁶ That erosion was most pronounced between January 1981 and April 1990 and between September 1997 and July 2007—each a period of nearly 10 years during which the nominal value of the minimum wage was unchanged.

Many states and localities have minimum-wage laws that apply, along with federal law, to employers within their jurisdiction. In recent years, states and localities have been particularly active in boosting their minimum wage; as of January 2014, 21 states and the District of Columbia had a minimum wage that was higher than the federal one. In 11 of those states, the minimum wage is adjusted automatically each year with inflation, and in four more, plus the District of Columbia, future increases have

already been legislated. In California, for example, the minimum wage is scheduled to increase from \$8.00 to \$9.00 in July 2014 and to \$10.00 in January 2016. Some localities also have minimum wages that are higher than the applicable state or federal minimum wage; in San Francisco, for instance, the minimum wage is \$10.74 per hour. Another 20 states have minimum wages equal to the federal minimum wage (and linked to it, in some cases). In some of those states, the state laws apply to some workers and employers who are not covered by the FLSA. At the moment, about half of all workers in the United States live in states where the applicable minimum wage is more than \$7.25 per hour. The applicable minimum wage in those states ranges from \$7.40 to \$9.32 per hour (see Figure 2).

Minimum-wage workers are sometimes thought of primarily as teenagers from nonpoor families who are working part time, but that is not the case now. Of the 5.5 million workers who earned within 25 cents of the minimum wage in 2013, three-quarters were at least 20 years old and two-fifths worked full time. Their median family income was about \$30,000, CBO estimates. (Some of the family incomes within that group of workers were substantially higher or lower than that amount, in part because the number of working adults in their families varied.)

Two Options for Increasing the Federal Minimum Wage

Lawmakers have proposed various options for increasing the federal minimum wage, including several that would increase it to \$10.10 per hour and subsequently index it

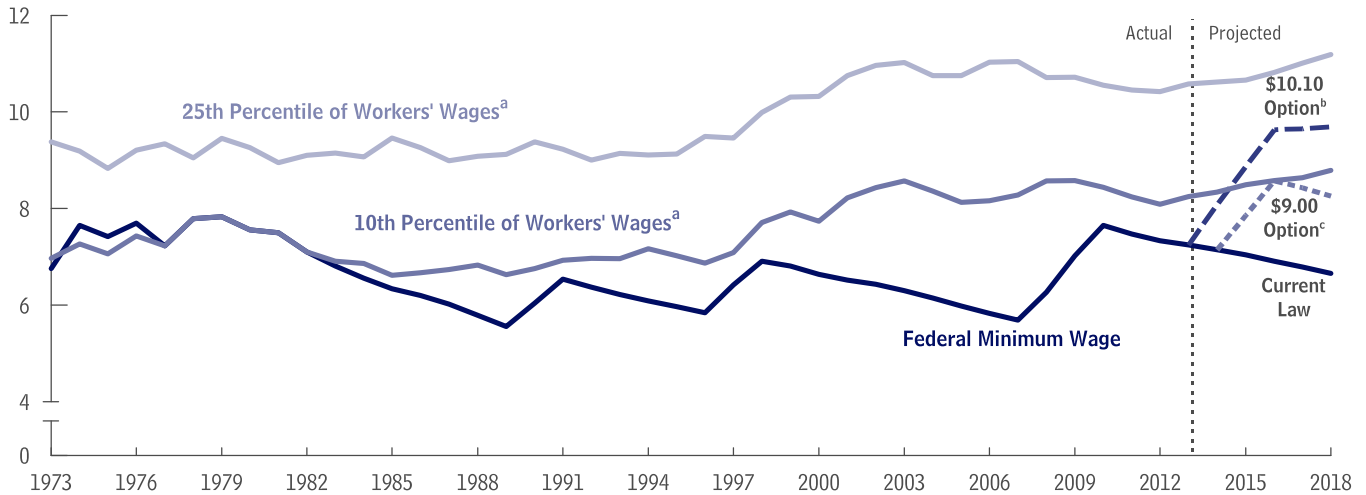
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4. For details about the FLSA's minimum-wage requirements, see Fair Labor Standards Act of 1938, as amended, 29 U.S.C. §201 et seq. (2012). See also Department of Labor, "Minimum Wage and Overtime Pay" (accessed January 8, 2014), www.dol.gov/compliance/guide/minwage.htm.
 5. After CBO completed its analysis of increasing the federal minimum wage, the President issued an executive order, entitled "Minimum Wage for Contractors," that established a minimum wage of \$10.10 per hour for certain individuals working under new contracts with the federal government, beginning on January 1, 2015. That order slightly reduces the number of workers who would be affected by increasing the federal minimum wage and thus slightly reduces the estimated effects presented in this report.

-
6. Adjusted for inflation, the federal minimum wage reached its historical peak in 1968. In that year, its value in 1968 dollars was \$1.60, which is equal to \$8.41 in 2013 dollars if the conversion is done with the price index for personal consumption expenditures published by the Bureau of Economic Analysis. CBO generally uses that index when adjusting labor market data for inflation, considering it a more accurate measure than a common alternative—the consumer price index for all urban consumers (CPI-U), which is published by the Bureau of Labor Statistics (BLS). According to many analysts, the CPI-U overstates increases in the cost of living because it does not fully account for the fact that consumers generally adjust their spending patterns as some prices change relative to other prices and because of a statistical bias related to the limited amount of price data that BLS can collect. The value of \$1.60 in 1968 dollars is equal to \$10.71 in 2013 dollars if the conversion is done with the CPI-U.

Figure 1.

Workers' Hourly Wages and the Federal Minimum Wage, 1973 to 2018

(2013 dollars per hour)



Source: Congressional Budget Office based on monthly data from the Census Bureau's Current Population Survey and on data from the Department of Labor.

Note: CBO converted wages to 2013 dollars using the price index for personal consumption expenditures published by the Bureau of Economic Analysis. For example, nominal values in 2016 of \$10.10 and \$9.00 were adjusted downward to account for projected inflation between 2013 and 2016. After 2016, the minimum wage under the \$10.10 option would increase slightly in the 2013 dollars shown in this figure because it would be indexed to the consumer price index, which would grow faster than the price index for personal consumption expenditures, CBO projects. Values for the federal minimum wage—both actual values and projected values under the \$10.10 option, the \$9.00 option, and current law—are as of July 1 of each year.

- a. The hourly wage of workers not paid hourly was estimated as their weekly earnings divided by their usual hours worked per week. Values after those for 2013 are projected under current law.
- b. The minimum wage would rise (in three steps, starting in 2014) to \$10.10 by July 1, 2016, and then be indexed to inflation.
- c. The minimum wage would rise (in two steps, starting in 2015) to \$9.00 by July 1, 2016, and would not be subsequently indexed to inflation.

for inflation.⁷ CBO has assessed the impact of such an option, as well as the impact of a smaller increase that would boost the minimum wage to \$9.00 per hour and would not link future increases to inflation. (See Appendix A for information about how CBO conducted its assessments.) The options that CBO analyzed would not change other provisions of the FLSA, such as the one that applies to wages for teenage workers during their first 90 days of employment.

A \$10.10 Option

CBO examined an option that would increase the federal minimum wage from \$7.25 per hour to \$8.20 on July 1,

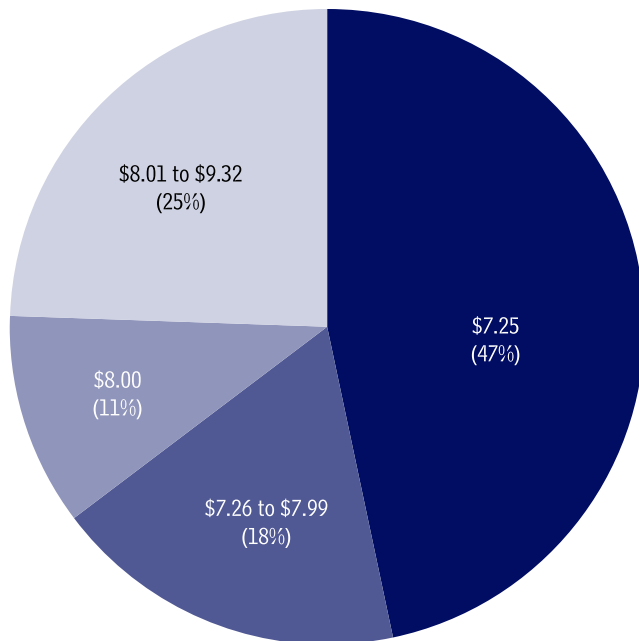
2014; to \$9.15 one year after that; and to \$10.10 after another year. The increase in the minimum wage between 2014 and 2016 under this option would be about 40 percent, roughly the same percentage as the total increase from 2007 to 2009 but larger than several earlier increases. Each year after that, the minimum wage would rise with the consumer price index.⁸

In addition, this option would raise the minimum cash wage for tipped workers from \$2.13 per hour to \$4.90 in three steps timed to coincide with the changes in the minimum wage. Then, starting in 2017, the minimum

7. See, for example, S. 460, the Fair Minimum Wage Act of 2013; S. 1737, the Minimum Wage Fairness Act; and H.R. 3939, the Invest in United States Act of 2014. Another proposal (H.R. 3746, the Fair Minimum Wage Act of 2013) would increase the minimum wage to \$11.00 and subsequently index it for inflation.

8. The \$10.10 option is based on the provisions of S. 460, the Fair Minimum Wage Act of 2013. (The FLSA and S. 460 also apply to Puerto Rico and certain other U.S. territories, but because of limitations in available data, CBO's analysis is limited to the effects of minimum-wage increases on employment and family income in the 50 states and the District of Columbia.)

Figure 2.
Shares of All Workers, by States' Applicable Minimum Wage, 2014



Source: Congressional Budget Office based on monthly data from the Census Bureau's Current Population Survey and on data from the Department of Labor.

Note: As of January 1, 2014, 21 states and the District of Columbia had a minimum wage above the federal minimum wage. The highest was \$9.32 in the state of Washington.

cash wage for tipped workers would rise by 95 cents each year until it reached 70 percent of the minimum wage (which would occur in 2019, by CBO's estimate); in subsequent years, it would be tied to inflation.

A \$9.00 Option

CBO also examined a smaller change that would increase the federal minimum wage from \$7.25 per hour to \$8.10 on July 1, 2015, and to \$9.00 on July 1, 2016. The minimum cash wage for tipped workers would increase when the minimum wage increased, and by the same percentage. The increase in the minimum wage would start one year later than it would under the \$10.10 option. Like previous minimum-wage increases, this one would not be indexed to subsequent inflation. This \$9.00 option is more similar than the \$10.10 option to minimum-wage increases studied in the economics literature in a number of respects: the size of the increase, the portion of the workforce that it would affect, and the fact that its real value would be eroded over time.

How Increases in the Minimum Wage Affect Employment and Family Income

In general, increases in the minimum wage probably reduce employment for some low-wage workers. At the same time, however, they increase family income for many more low-wage workers.

Employment

According to conventional economic analysis, increasing the minimum wage reduces employment in two ways. First, higher wages increase the cost to employers of producing goods and services. The employers pass some of those increased costs on to consumers in the form of higher prices, and those higher prices, in turn, lead the consumers to purchase fewer of the goods and services. The employers consequently produce fewer goods and services, so they hire fewer workers. That is known as a scale effect, and it reduces employment among both low-wage workers and higher-wage workers.

Second, a minimum-wage increase raises the cost of low-wage workers relative to other inputs that employers use to produce goods and services, such as machines, technology, and more productive higher-wage workers. Some employers respond by reducing their use of low-wage workers and shifting toward those other inputs. That is known as a substitution effect, and it reduces employment among low-wage workers but increases it among higher-wage workers.

However, conventional economic analysis might not apply in certain circumstances. For example, when a firm is hiring more workers and needs to boost pay for existing workers doing the same work—to match what it needs to pay to recruit the new workers—hiring a new worker costs the company not only that new worker's wages but also the additional wages paid to retain other workers. Under those circumstances, which arise more often when finding a new job is time-consuming and costly for workers, increasing the minimum wage means that businesses have to pay the existing workers more, whether or not a new employee was hired; as a result, it lowers the additional cost of hiring a new employee, leading to increased employment. There is a wide range of views among economists about the merits of the conventional analysis and of this alternative.

The low-wage workers whose wages are affected by increases in the minimum wage include not only those workers who would otherwise have earned less than the

minimum but also, in some cases, workers who would have earned slightly more than the minimum. After a minimum-wage increase, some employers try to preserve differentials in pay that existed before—for example, so that supervisors continue to be paid more than the people they supervise—by raising the wages of people who previously earned a little more than the new minimum. Also, some wages determined by collective bargaining agreements are tied to the federal minimum wage and could therefore increase. As a result, an increase in the minimum wage causes some workers who would otherwise have earned slightly more than the new minimum wage to become jobless, for the same reasons that lower-wage workers do; at the same time, some firms hire more of those workers as substitutes for the workers whose wages were required to be increased.

The change in employment of low-wage workers caused by a minimum-wage increase differs substantially from firm to firm. Employment falls more at firms whose customers are very sensitive to price increases, because demand for their products or services declines more as prices rise, so those firms cut production more than other firms do. Employment also falls more at firms that can readily substitute other inputs for low-wage workers and at firms where low-wage workers constitute a large fraction of input costs. However, when low-wage workers have fewer employment alternatives overall, employment can fall less at firms that offset some of the increased costs with higher productivity from employees' working harder to keep their better-paying jobs and with the lower cost of filling vacant positions that results from higher wages' attracting more applicants and reducing turnover. Some firms, particularly those that do not employ many low-wage workers but that compete with firms that do, might see demand rise for their goods and services as their competitors' costs rise; such firms would tend to hire more low-wage workers as a result.

The change in employment of low-wage workers also differs over time. At first, when the minimum wage rises, some firms employ fewer low-wage workers, while other firms do not; the reduced employment is concentrated in businesses and industries where higher prices result in larger reductions in demand. Over a longer time frame, however, more firms replace low-wage workers with inputs that are relatively less expensive, such as more productive higher-wage workers. Thus, the percentage reduction in employment of low-wage workers is generally greater in the long term than in the short term, in

CBO's assessment. (However, the total reduction in employment might be smaller in the long term; that total depends not only on the percentage reduction in employment of low-wage workers but also on the number of such workers, which could decline over time if wage growth for low-wage workers exceeded any increase in the minimum wage, all else being equal.)

Employers might respond to an increase in the minimum wage in ways other than boosting prices or substituting other inputs for low-wage workers. For example, they might partly offset a minimum-wage increase by reducing other costs, including workers' fringe benefits (such as health insurance or pensions) and job perks (such as free meals). As a result, a higher minimum wage might increase total compensation (which includes benefits and perks) less than it increased cash wages alone. That, in turn, would give employers a smaller incentive to reduce their employment of low-wage workers. However, such benefit reductions would probably be modest, in part because low-wage workers generally receive few benefits related to pensions or health insurance. In addition, tax rules specify that employers who reduce low-wage workers' nonwage benefits can face unfavorable tax treatment for higher-wage workers' nonwage benefits. Employers can also partly offset higher wages for low-wage workers by reducing either formal training or informal mentoring and coaching. The evidence on how much employers reduce benefits, training, or other costs is mixed. (For examples of such evidence, see Appendix B.)

An increase in the minimum wage also affects the employment of low-wage workers in the short term through changes in the economywide demand for goods and services. A higher minimum wage shifts income from higher-wage consumers and business owners to low-wage workers. Because those low-wage workers tend to spend a larger fraction of their earnings, some firms see increased demand for their goods and services, boosting the employment of low-wage workers and higher-wage workers alike. That effect is larger when the economy is weaker, and it is larger in regions of the country where the economy is weaker.

Low-wage workers are not the only ones whose employment can be affected by a minimum-wage increase; the employment of higher-wage workers can be affected as well, in several ways. Firms that cut back on production tend to reduce the number of both higher-wage workers and low-wage workers. But once a minimum-wage

increase makes higher-wage workers relatively less expensive, firms sometimes hire more of them to replace a larger number of less productive low-wage workers. Another factor affecting higher-wage workers is the increase in the economywide demand for goods and services. All in all, a higher minimum wage tends to increase the employment of higher-wage workers slightly, according to CBO's analysis.

Family Income

For most families with low-wage workers, a higher minimum wage boosts family income, because of the increase in earnings that many of those workers (including those whose wages were slightly above the new minimum) receive. A much smaller number of low-wage workers become jobless and therefore experience a decline in earnings because of the higher minimum wage.

For families with low-wage workers, the effect of a higher minimum wage depends on how many such workers are in a family, whether those workers become jobless (and, if so, for how long), and whether there are other changes in family income. For instance, the decline in income from losing a job can be offset in part by increases in nonlabor income, such as unemployment compensation, or by increases in the work of other family members.

For business owners, family income (including income for shareholders) falls to the extent that firms' profits are reduced. In addition, real family income for many people tends to fall a bit, because the increase in prices of goods and services reduces families' purchasing power.

The effects on total national income of an increase in the minimum wage differ in the long term and in the short term. In the long term, the key determinant of the nation's output and income is the size and quality of the workforce, the stock of productive capital (such as factories and computers), and the efficiency with which workers and capital are used to produce goods and services (known as total factor productivity). Raising the minimum wage probably reduces employment, in CBO's assessment. In the long term, that reduction in the workforce lowers the nation's output and income a little, which means that the income losses of some people are slightly larger than the income gains of others. In the short term, by contrast, the nation's output and income can deviate from the amounts that would typically arise from a given workforce, capital stock, and productivity in response to changes in the economywide demand for

goods and services. Raising the minimum wage increases that demand, in CBO's assessment, because the families that experience increases in income tend to raise their consumption more than the families that experience decreases in income tend to reduce their consumption. In the short term, that increase in demand raises the nation's output and income slightly, which means that the income losses of some people are slightly smaller than the income gains of others.

CBO's Findings About Employment and Family Income

CBO estimated the effects on employment and family income of both the \$10.10 option and the \$9.00 option for raising the federal minimum wage.⁹ CBO's estimates are for the second half of 2016 because that would be the point at which the minimum wage reached \$10.10 under the first option and \$9.00 under the second. In either case, the increase in the minimum wage would have two principal effects on low-wage workers: The large majority would have higher wages and family income, but a much smaller group would be jobless and have much lower family income. Once the other changes in income were taken into account, families whose income would be below six times the poverty threshold under current law would see a small increase in income, on net, and families whose income would be higher under current law would see reductions in income, on net. In addition, in either case, higher-wage workers would see a small increase in the number of jobs.

Increases in the minimum wage would raise the wages not only for many workers who would otherwise have earned less than the new minimum but also for some workers who would otherwise have earned slightly more than the new minimum, as discussed above. CBO's analysis focused on workers who are projected to earn less than \$11.50 per hour in 2016 under current law (who, in this report, are generally referred to as low-wage workers). People with certain characteristics are more likely to be in that group and are therefore more likely to be affected by increases in the minimum wage like those that CBO examined. For example, in 2016, 88 percent of the

9. For an estimate of the effect on employment of a previous proposal to increase the minimum wage, see Congressional Budget Office, private-sector mandate statement for S. 277, the Fair Minimum Wage Act of 2001 (May 9, 2001), www.cbo.gov/publication/13043.

people earning such wages will be at least 20 years old, 56 percent will be female, and 91 percent will not have attained a bachelor's degree, CBO estimates (see Table 2).

Effects of the Options on Employment

According to CBO's central estimate, implementing the \$10.10 option would reduce employment by roughly 500,000 workers in the second half of 2016, relative to what would happen under current law.¹⁰ That decrease would be the net result of two effects: a slightly larger decrease in jobs for low-wage workers (because of their higher cost) and an increase of a few tens of thousands of jobs for other workers (because of greater demand for goods and services).¹¹ By CBO's estimate, about 1½ percent of the 33 million workers who otherwise would have earned less than \$11.50 per hour would be jobless—either because they lost a job or because they could not find a job—as a result of the increase in the minimum wage.

Those job losses among low-wage workers would be concentrated among people who are projected to earn less than \$10.10 an hour under current law. Some workers who would otherwise have earned between \$10.10 and \$11.50 per hour would also see an increase in their wages, which would tend to reduce their employment as well, CBO estimates. However, some firms might hire more of those workers as substitutes for the lower-paid workers whose wages had been increased. Those two factors would probably be roughly offsetting, CBO anticipates, so the number of such workers who were employed would probably not change significantly.

The overall reduction in employment could be smaller or larger than CBO's central estimate. In CBO's assessment, there is about a two-thirds chance that the effect of the \$10.10 option would be in the range between a very slight decrease in employment and a decrease of

1.0 million workers; thus, there is a one-third chance that the effect would be either above or below that range. The most important factors contributing to the width of the range are uncertainty about the growth of wages over the next three years (which influences the number of workers who would be affected by the minimum-wage increase, as well as the extent to which the increase would raise their wages) and uncertainty about the responsiveness of employment to an increase in wages. For example, if wage growth under current law was slower than CBO projects, implementing the increase would result in more people with increased wages and a greater reduction in employment than CBO's central estimate suggests.

Under the \$9.00 option, employment would decline by about 100,000 workers in the second half of 2016, relative to what it would be under current law, according to CBO's central estimate. That estimate is much smaller than the central estimate for the \$10.10 option for three reasons: Fewer workers would be affected; the change in their wages would be smaller; and four aspects of the \$9.00 option would make employment in 2016 less responsive to a minimum-wage increase, CBO expects.¹² The first of those four aspects is that the \$9.00 option is not indexed to inflation, so some employers would probably refrain from reducing employment, knowing that inflation would erode the cost of paying higher wages. Second, under the \$9.00 option, the second half of 2016 arrives one year after the initial increase in the minimum wage—rather than two years, as under the \$10.10 option—and employers would be less likely to reduce employment soon after an increase in the minimum wage than they would be over a longer period. Third, because the cost of paying higher wages under the \$9.00 option is smaller than that of the \$10.10 option, CBO expects that fewer employers would find it desirable to incur the adjustment costs of reducing employment (such as installation of new equipment). Fourth, the \$9.00 option would apply to a smaller share of the workforce. Four percent of the labor hours in the economy will be worked

10. A central estimate is one that uses values at or near the midpoints of estimated ranges for key inputs.

11. In this report, phrases referring to changes in the number of jobs are used interchangeably with phrases referring to changes in employment. Technically, however, if a low-wage worker holds multiple jobs and loses one of them, that would represent a reduction of one job but no change in employment (because the worker would remain employed). About 5 percent of low-wage workers will hold more than one job under current law, CBO projects. Therefore, for any given reduction in employment, the reduction in the number of jobs will be slightly larger.

12. Under the \$9.00 option, the central estimate of the responsiveness of employment to a change in the applicable minimum wage is -0.075 for teenagers, for example, which means that the employment of teenagers would be reduced by three-quarters of one percent after a 10 percent change in the minimum wage. The equivalent estimate under the \$10.10 option is -0.10. See Appendix A for more information.

Table 2.**Projected Characteristics of Low-Wage Workers, Second Half of 2016**

Characteristic	Percentage of All Workers With Characteristic Who Will Be Low-Wage	Percentage of Low-Wage Workers With Characteristic
Age		
16 to 19	87	12
20 and older	22	88
All	24	100
Sex		
Female	28	56
Male	21	44
All	24	100
Educational Attainment		
Less than high school	58	21
High school graduate or some college	30	70
Bachelor's degree	7	10
All	24	100
Hours Worked per Week		
Fewer than 35	58	47
35 or more	16	53
All	24	100
Number of Employees in Firm		
Fewer than 50	30	48
50 or more	19	52
All	24	100

Source: Congressional Budget Office based on monthly and annual data from the Census Bureau's Current Population Survey.

Note: Low-wage workers are people who are projected, under current law in the second half of 2016, to be paid less than \$11.50 per hour.

by people who will earn up to \$9.00 per hour under current law and who would either receive a wage increase or be jobless if the \$9.00 option was implemented, CBO estimates. In contrast, about 10 percent of labor hours will be worked by people who will earn up to \$10.10 per hour under current law and who would either receive a wage increase or be jobless if the \$10.10 option was implemented. Thus, the \$9.00 option would cause a correspondingly smaller increase in costs, which employers would be likely to absorb less through reductions in employment and more in other ways.

In CBO's assessment, there is a two-thirds chance that the effect of the \$9.00 option would be in the range between a very slight increase in the number of jobs and a loss of 200,000 jobs.¹³ If employment increased under either option, in CBO's judgment, it would probably be because increased demand for goods and services (resulting from the shift of income from higher-income to

lower-income people) had boosted economic activity and generated more jobs than were lost as a direct result of the increase in the cost of hiring low-wage workers.

CBO has not analyzed the effects of either option on the number of hours worked by people who would remain employed or on the decision to search actively for work and join the labor force by people who would not

13. In a recent survey, leading economists were asked whether they agreed with the statement that "raising the federal minimum wage to \$9 per hour would make it noticeably harder for low-skilled workers to find employment." When the results were weighted by the respondents' confidence, 40 percent of the economists agreed with the statement, 38 percent disagreed, and 22 percent were uncertain. However, the survey did not specify how large a drop in employment was meant by "noticeably harder . . . to find employment." See University of Chicago Booth School of Business, "Minimum Wage" (published February 26, 2013; accessed January 8, 2014), <http://tinyurl.com/aa52pfo>.

otherwise be working. Therefore, the agency has not reported the effects of the options on full-time-equivalent employment or on the unemployment rate.

Effects of the Options on Family Income

Among the 33 million low-wage workers earning less than \$11.50 per hour in the second half of 2016 under current law, CBO estimates, real earnings would increase by \$31 billion as a result of higher wages if the \$10.10 option was implemented. (All amounts of income reported for that period are annualized—that is, multiplied by two—and reported in 2013 dollars.) About 16.5 million workers who will earn less than \$10.10 per hour under current law would receive higher wages, CBO estimates, and some workers who will earn between \$10.10 and \$11.50 per hour under current law would receive higher wages as well.¹⁴ Most of the additional income would accrue to families with fairly low income, but a substantial portion would also be received by low-wage workers in higher-income families—29 percent and 6 percent by families who would otherwise have had income greater than three and six times the federal poverty threshold, respectively.

That increase in income resulting from higher wages would be accompanied by reductions of a similar amount in real income from several other sources: decreases in earnings for workers who would be jobless because of the minimum-wage increase; losses in income for business owners; and increases in prices of goods and services, which would reduce people's purchasing power. In addition, a few higher-wage workers would be employed and earn more because of increased demand for goods and services resulting from the minimum-wage increase.

Once all those factors are taken into account, CBO estimates that the net changes in real income would be an increase of about \$5 billion for families whose income would have been below the poverty threshold under current law; an increase of \$12 billion for families whose income would have been between one and three times the poverty threshold; an increase of \$2 billion for families whose income would have been between three and six times the poverty threshold; and a *decrease* of \$17 billion for families whose income would have been greater than

that (see Figure 3). (In 2016, six times the poverty threshold will be roughly \$120,000 for a family of three and \$150,000 for a family of four, CBO projects.) According to CBO's estimates, the increase in earnings for the few low-wage workers living in that last group of families would be more than offset by income reductions, in part because the losses in business income and in real income from price increases would be concentrated in those families (see Table 3).

Families whose income will be below the poverty threshold in 2016 under current law will have an average income of \$10,700, CBO projects (see Table 4 on page 14). The agency estimates that the \$10.10 option would raise their average real income by about \$300, or 2.8 percent. For families whose income would otherwise have been between the poverty threshold and 1.5 times that amount, average real income would increase by about \$300, or 1.1 percent. The increase in average income would be smaller, both in dollar amounts and as a share of family income, for families whose income would have been between 1.5 times and six times the poverty threshold. And for families whose income would otherwise have been greater than six times the poverty threshold, the total effect of the \$10.10 option would be a reduction in average real income of about \$700, or 0.4 percent. But the effects of a minimum-wage increase on family income would vary even among families with similar incomes under current law. For example, many families with income less than six times the poverty threshold would see their income rise; but income for a smaller set of those families would decline, because some low-wage workers would lose jobs that they would otherwise have.

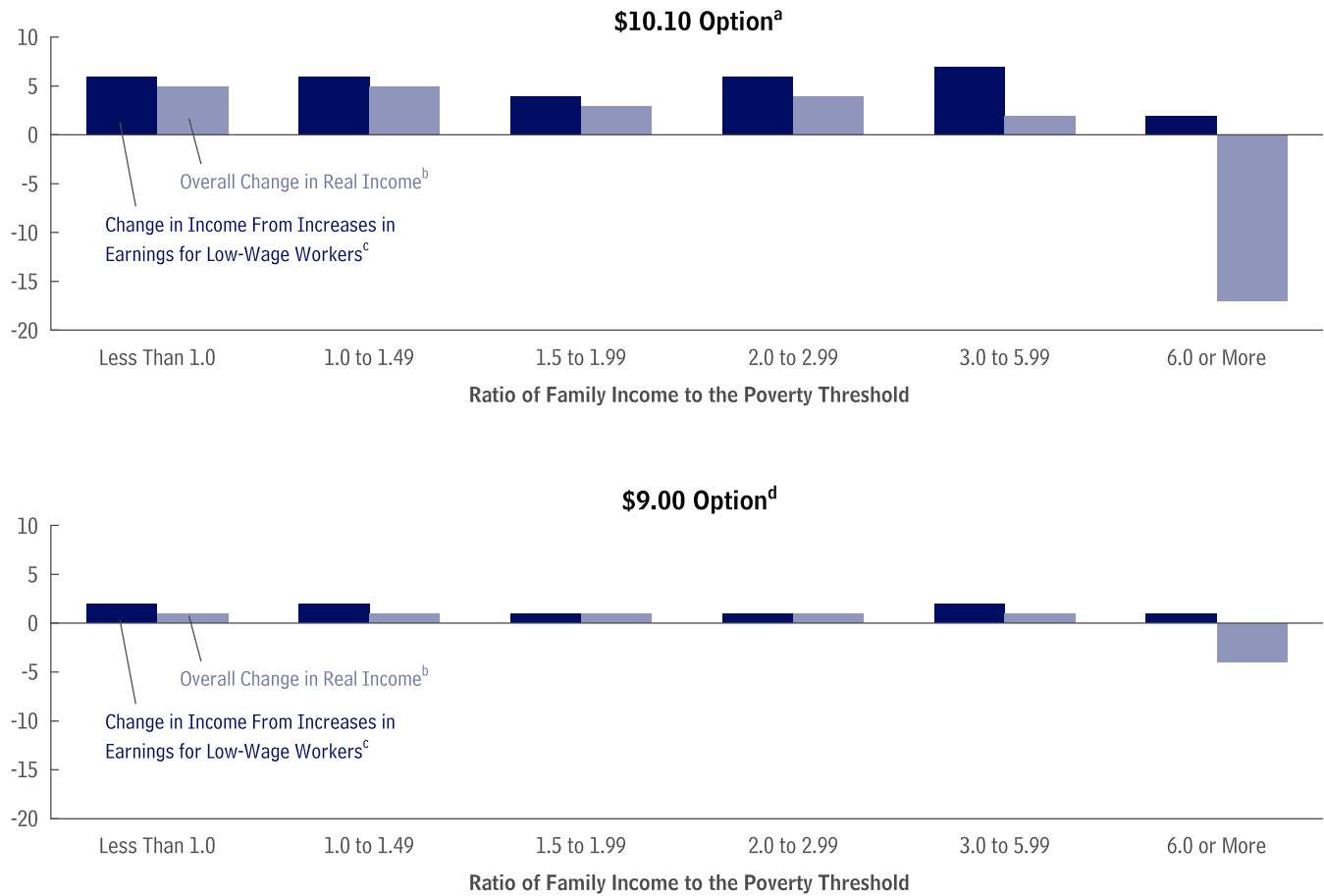
Under current law, CBO projects, there will be roughly 45 million people in families whose income is below the poverty threshold in 2016. The \$10.10 option would reduce that number by about 900,000, or 2 percent, according to CBO's estimate. That estimate takes into account both families whose income would increase and move them out of poverty and families whose income would fall and move them into poverty. The estimate uses a measure of family income called cash income, which is used to determine the official poverty rate. Cash income includes earnings and cash transfers from the government, such as Supplemental Security Income benefits. It excludes noncash transfers, such as benefits from Medicaid and the Supplemental Nutrition Assistance Program (SNAP, formerly known as the Food Stamp program); taxes; and tax credits, such as the earned

14. CBO did not estimate the number of workers in the latter group who would receive higher wages as a result of the increase in the minimum wage; instead, it applied an estimated average percentage increase in wages to all workers in that group. See Appendix A for more information.

Figure 3.

Estimated Effects on Real Family Income of an Increase in the Federal Minimum Wage, Second Half of 2016

(Billions of 2013 dollars, annualized)



Source: Congressional Budget Office based on annual data from the Census Bureau's Current Population Survey.

Note: Calculated using before-tax family cash income. Poverty thresholds vary with family size and composition. The definitions of income and of poverty thresholds are those used to determine the official poverty rate and are as defined by the Census Bureau. CBO projects that in 2016, the poverty threshold (in 2013 dollars) will be about \$18,700 for a family of three and \$24,100 for a family of four.

- a. The minimum wage would rise (in three steps, starting in 2014) to \$10.10 by July 1, 2016, and then be indexed to inflation.
- b. Changes in real (inflation-adjusted) income include increases in earnings for workers who would receive a higher wage, decreases in earnings for workers who would be jobless because of the minimum-wage increase, losses in income for business owners, decreases in income because of increases in prices, and increases in income generated by higher demand for goods and services.
- c. Increases in earnings for workers who are projected, under current law, to be paid less than \$11.50 per hour.
- d. The minimum wage would rise (in two steps, starting in 2015) to \$9.00 by July 1, 2016, and would not be subsequently indexed to inflation.

income tax credit (EITC). (Because the EITC provides cash to many lower-income families, it is sometimes compared with the federal minimum wage in discussions about how to boost lower-income families' resources; see Box 1 on page 15.)

Implementing the \$9.00 option would have a smaller effect on family income and on the number of people in

poverty than implementing the \$10.10 option would. About 7.6 million workers who will earn less than \$9.00 per hour under current law would receive higher wages, CBO estimates, and so would some workers who will earn more than \$9.00 per hour under current law. Once all factors are taken into account, CBO estimates that the net changes in total real income would be an increase of about \$1 billion for families whose income

Table 3.
Projected Shares of Workers, by Family Income Group, Second Half of 2016

Ratio of Family Income to the Poverty Threshold	Percentage of All Workers	Percentage of Low-Wage Workers ^a
Less Than 1.0	6	20
1.0 to 1.49	6	16
1.5 to 1.99	7	14
2.0 to 2.99	16	18
3.0 to 5.99	39	24
6.0 or More	26	9
Total	100	100

Source: Congressional Budget Office based on annual data from the Census Bureau's Current Population Survey.

Note: Calculated using before-tax family cash income. Poverty thresholds vary with family size and composition. The definitions of income and of poverty thresholds are those used to determine the official poverty rate and are as defined by the Census Bureau. CBO projects that in 2016, the poverty threshold (in 2013 dollars) will be about \$18,700 for a family of three and \$24,100 for a family of four.

a. Low-wage workers are people who are projected, under current law in the second half of 2016, to be paid less than \$11.50 per hour.

would otherwise have been below the poverty threshold; increases totaling \$4 billion for families whose income would have been between one and six times the poverty threshold; and a decrease of about \$4 billion for families with higher income, as the declines in income for business owners and the loss of purchasing power would more than offset the increases in earnings for low-wage workers in that group. The agency estimates that average real family income would increase by about \$100, or 0.9 percent, for families whose income would have been below the poverty threshold, and that the number of people living in such families would decline by about 300,000, or two-thirds of one percent. That is one-third of the decline in the number of people in poverty that would occur under the \$10.10 option, CBO projects. For families whose income would otherwise have been six times the poverty threshold or more, average real family income would be lower by 0.1 percent.

The effects of the two options on average family income and on the number of people living in poverty are

difficult to project accurately. Those effects depend on many things, including the extent to which the higher minimum wage would reduce employment, the length of time that people are not working, and the rate at which wages will grow over time under current law. The larger the reduction in employment for a given increase in the minimum wage, the less effective the policy would be at raising families out of poverty. And if wages grew more quickly under current law than CBO projects, fewer workers would have their wages increased under the options, and the effect on poverty would be smaller. (If those wages grew less quickly than CBO projects, the effect would be larger.)

The Effect of an Increase in the Minimum Wage on the Federal Budget

An increase in the federal minimum wage would directly affect the federal budget by requiring the government to increase wages for a small number of hourly federal employees. A minimum-wage increase would also indirectly affect the budget by boosting the prices of some goods and services purchased by the government. Most of those added costs for wages, goods, and services would need to be covered by discretionary appropriations, which are capped through 2021 under current law. If the caps were not adjusted, federal budget deficits would not be affected by the higher costs, but the benefits and government services that could be provided under the existing caps would be reduced. If, instead, lawmakers adjusted the caps to cover the higher costs, and if future appropriations equaled those higher caps, then deficits would be larger.

In addition, an increase in the federal minimum wage would indirectly affect the federal budget by changing people's income—raising real income for some workers while reducing the real income of people who would be jobless because of the minimum-wage increase, of business owners, and of consumers facing higher prices. As a group, the workers receiving an earnings increase would pay more in taxes and receive less in benefits than they would have otherwise, reducing the federal budget deficit; however, the workers, business owners, and consumers with reduced income would pay less in taxes and receive more in benefits, increasing the deficit.

Table 4.**Estimated Effects on Average Real Family Income of an Increase in the Federal Minimum Wage, Second Half of 2016**

Ratio of Family Income to the Poverty Threshold	Average Real Family Income Before the Wage Change (2013 dollars, annualized)	Change in Average Real Family Income	
		2013 Dollars, Annualized	Percent
\$10.10 Option^a			
Less Than 1.0	10,700	300	2.8
1.0 to 1.49	26,300	300	1.1
1.5 to 1.99	36,300	200	0.6
2.0 to 2.99	51,400	200	0.4
3.0 to 5.99	86,600	*	**
6.0 or More	182,200	-700	-0.4
\$9.00 Option^b			
Less Than 1.0	10,700	100	0.9
1.0 to 1.49	26,300	100	0.4
1.5 to 1.99	36,300	100	0.3
2.0 to 2.99	51,400	100	0.2
3.0 to 5.99	86,600	*	**
6.0 or More	182,200	-200	-0.1

Source: Congressional Budget Office based on annual data from the Census Bureau's Current Population Survey.

Notes: Changes in real (inflation-adjusted) income include increases in earnings for workers who would receive a higher wage, decreases in earnings for workers who would be jobless because of the minimum-wage increase, losses in income for business owners, decreases in income because of increases in prices, and increases in income generated by higher demand for goods and services. Results are weighted by the number of people in the family; for example, when CBO calculated the averages, a family of three would be represented three times.

Calculated using before-tax family cash income. Poverty thresholds vary with family size and composition. The definitions of income and of poverty thresholds are those used to determine the official poverty rate and are as defined by the Census Bureau. CBO projects that in 2016, the poverty threshold (in 2013 dollars) will be about \$18,700 for a family of three and \$24,100 for a family of four.

* = between zero and \$50; ** = between zero and 0.05 percent.

- The minimum wage would rise (in three steps, starting in 2014) to \$10.10 by July 1, 2016, and then be indexed to inflation.
- The minimum wage would rise (in two steps, starting in 2015) to \$9.00 by July 1, 2016, and would not be subsequently indexed to inflation.

CBO anticipates that the increases in income would be larger than the decreases in income for a few years after an increase in the minimum wage but would be smaller thereafter, as discussed earlier in the report. Further, for reasons discussed below, CBO anticipates that the effective marginal tax rate—that is, the combination of increased taxes and decreased benefits for each additional dollar of income—for the increases in income would probably be slightly larger than the effective marginal tax rate for the decreases in income. Combining those factors, CBO concludes that the net effect on the federal budget of raising the minimum wage would probably be a small decrease in budget deficits for several years but a small increase in budget deficits thereafter. It is unclear whether the effect for the coming decade as a whole

would be a small increase or a small decrease in budget deficits.¹⁵

15. Cost estimates produced by CBO and the staff of the Joint Committee on Taxation (JCT) typically reflect the convention that macroeconomic variables, such as nominal output and the average price level, remain fixed at the values that they are projected to reach under current law. That is a long-standing convention—one that has been followed in the Congressional budget process since it was established in 1974 and by JCT since the early 1960s. Therefore, in producing a cost estimate for legislation that would increase the minimum wage, CBO and JCT would not incorporate some of the effects that such an increase would probably have on the economy. CBO was not able to assess how that approach might affect the estimated budgetary impact of increasing the minimum wage.

Box 1.**The Minimum Wage and the Earned Income Tax Credit**

The earned income tax credit (EITC) provides cash assistance through the federal income tax system to low- and moderate-income families on the basis of their earnings, adjusted gross income, and family structure.¹ At first, as family earnings rise above zero (the “phase-in” range), EITC benefits increase; when earnings reach a certain point, the benefits stop increasing; when earnings reach a higher point (the beginning of the “phase-out” range), the benefits decline; and when earnings are high enough, the benefits end.² The maximum credit in 2014 is \$5,460 for people with two qualifying children, for example. In 2016, the Congressional Budget Office (CBO) projects, the earnings level at which EITC benefits end will range from \$15,100 for an unmarried worker without children to \$54,300 for a married couple with three or more children.

Using the Minimum Wage or the EITC to Boost the Resources of Low-Income Families

To achieve any given increase in the resources of lower-income families would require a greater shift of resources in the economy if done by increasing the minimum wage than if done by increasing the EITC.³ The reason is that a minimum-wage increase would add to the resources of most families of low-wage workers regardless of those families’ income; for example, one-third of low-wage workers would be in families whose income was more than three times the federal poverty

threshold in 2016, and many of those workers would see their earnings rise if the minimum wage rose. By contrast, an increase in the EITC would go almost entirely to lower-income families.

The Interaction of the Minimum Wage and the EITC

An increase in the minimum wage would affect EITC benefits in different ways for different families. Many families whose income was initially within the phase-in range of the EITC schedule would find that increased earnings led to additional EITC benefits. But families whose income was initially in the phaseout range of the schedule would find that income gains from a higher minimum wage were partly offset by a reduction in EITC benefits. And families whose income was initially between the phase-in and phaseout ranges (a range in which EITC benefits do not change as earnings rise) and remained in that range after the minimum-wage increase would see no change in their EITC benefits. As for higher-income families with low-wage workers, they would not have been eligible for the EITC in the first place.

The EITC encourages more people in low-income families to work—particularly unmarried custodial parents, often mothers, for whom the EITC is larger than it is for people without children.⁴ That increase in the number of available workers tends to reduce workers’ wages, allowing some of the benefit of the EITC to accrue to employers, rather than to the workers themselves.⁵ An increase in the minimum wage would shift some of that benefit from employers to workers by requiring the former to pay the latter more.

1. Adjusted gross income is income from all sources not specifically excluded from the tax code, minus certain deductions.
2. For a more extensive description of the EITC, see Congressional Budget Office, *Refundable Tax Credits* (January 2013), www.cbo.gov/publication/43767.
3. In a 2007 analysis, CBO compared the cost to employers of a change in the minimum wage that increased the income of poor families by a given amount to the cost to the federal government of a change in the EITC that increased the income of poor families by roughly the same amount. The cost to employers of the change in the minimum wage was much larger than the cost to the federal government of the change in the EITC. See Congressional Budget Office, *Response to a Request by Senator Grassley About the Effects of Increasing the Federal Minimum Wage Versus Expanding the Earned Income Tax Credit* (attachment to a letter to the Honorable Charles E. Grassley, January 9, 2007), www.cbo.gov/publication/18281. Most of the budgetary effect of an increase in the EITC shows up as an increase in spending, rather than as a reduction in revenues, because the credit is refundable and most of the total benefits represent amounts that are paid out rather than amounts that are used to offset other tax liabilities.

4. See Bruce D. Meyer and Dan T. Rosenbaum, “Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers,” *Quarterly Journal of Economics*, vol. 116, no. 3 (August 2001), pp. 1063–1114, <http://www.jstor.org/stable/2696426>; and Nada Eissa and Jeffrey B. Liebman, “Labor Supply Response to the Earned Income Tax Credit,” *Quarterly Journal of Economics*, vol. 111, no. 2 (May 1996), pp. 605–637, <http://www.jstor.org/stable/2946689>.
5. See David Lee and Emmanuel Saez, “Optimal Minimum Wage Policy in Competitive Labor Markets,” *Journal of Public Economics*, vol. 96, no. 9 (October 2012), pp. 739–749, <http://dx.doi.org/10.1016/j.jpubeco.2012.06.001>; and Jesse Rothstein, “Is the EITC as Good as an NIT? Conditional Cash Transfers and Tax Incidence,” *American Economic Journal: Economic Policy*, vol. 2, no. 1 (February 2010), pp. 177–208, <http://www.jstor.org/stable/25760056>.

Effects for People Whose Income Would Rise

As a group, the workers whose income rose because of a minimum-wage increase would consequently pay more in taxes and receive less in benefits.¹⁶ CBO has previously estimated that the effective federal marginal tax rate on earnings for low- and moderate-income workers is 32 percent, on average; that is, the combination of increased taxes and decreased benefits equals, on average, about one-third of such a worker's added earnings.¹⁷ CBO expects that workers receiving an increase in earnings from a boost to the minimum wage would face a similar rate, on average. Therefore, CBO expects that the reduction in the deficit associated with people whose earnings would rise would be about 32 percent of the increase in earnings for those workers.

Part of that deficit reduction would result from increased tax payments for the workers who were earning more. The largest part of that increase would consist of payroll taxes assessed for Social Security and Medicare, which are paid at a combined rate of 15.3 percent by most employees and employers.¹⁸ The increase in earnings for some workers would also increase the amount that they owed in income taxes before refundable tax credits were taken

into account, although almost all of them would owe no tax or be in one of the two lowest federal income tax brackets. In addition, benefits from the EITC would fall for workers whose annual income was in the range where the credits decrease with income (see Box 1). (However, those benefits would rise for workers whose annual income remained in the income range where the credits increase with income, and some workers with increased earnings would qualify for a larger child tax credit.)

The rest of the deficit reduction would result from less federal spending (aside from the effects on refundable earned income and child tax credits) for the workers receiving an increase in earnings. Spending on cash and near-cash transfer programs (such as SNAP and Supplemental Security Income) would decline for those workers, because the amount of those benefits generally falls as income rises.¹⁹ In addition, spending for premium assistance tax credits and cost-sharing subsidies for health insurance purchased through exchanges would decline for people who will be receiving such support under current law, because the amount of that support also generally falls as income rises.²⁰

The estimated effective federal marginal tax rate of 32 percent does not include the budgetary effects of some people's moving out of Medicaid coverage or into subsidized insurance coverage through exchanges because their earnings had increased.²¹ Some of those effects would raise federal costs and others would lower them. In particular, some people who will be eligible for Medicaid

16. In the short term, some people would also see an increase in income because, as discussed earlier in the report, an increase in the minimum wage would boost economywide demand for goods and services and thereby generate an increase in the nation's total output and income. That additional income would raise federal taxes and lower benefits. By contrast, in the long term, and also as discussed earlier in the report, an increase in the minimum wage would generate a decrease in total output and income. That loss in income would lower federal taxes and raise benefits; those effects are incorporated in the discussion in the following section.

17. Congressional Budget Office, *Effective Marginal Tax Rates for Low- and Moderate-Income Workers* (November 2012), www.cbo.gov/publication/43709. Table 6 in that report shows an aggregate marginal rate for 2014 of 34.8 percent. Subtracting the marginal rate attributable to state income taxes yields a federal marginal rate of 32.2 percent. That rate includes the effects of federal income and payroll taxes and of refundable earned income, child, and premium assistance tax credits for health insurance purchased through exchanges. It also includes changes in benefits under SNAP and cost-sharing subsidies provided to some participants in health insurance exchanges. That report was published before the enactment of the American Taxpayer Relief Act of 2012, but CBO estimates that the average federal marginal rate for 2014 would remain at about 32 percent after incorporating the effects of that act.

18. The 12.4 percent Social Security portion of that tax is paid on earnings up to a threshold (\$117,000 in 2014).

19. Some researchers have examined the change in cash and near-cash transfer payments that would result from a minimum-wage increase. See Linda Giannarelli, Kye Lippold, and Michael Martinez-Schiferl, *Reducing Poverty in Wisconsin: Analysis of the Community Advocates Public Policy Institute Policy Package* (Urban Institute, June 2012), <http://tinyurl.com/q7jb8v6> (PDF, 2.1 MB); and Linda Giannarelli, Joyce Morton, and Laura Wheaton, *Estimating the Anti-Poverty Effects of Changes in Taxes and Benefits with the TRIM3 Microsimulation Model* (Urban Institute, April 2007), <http://tinyurl.com/p75lej> (PDF, 2.9 MB). The authors estimate that the reduction in transfer payments for those receiving an increase in earnings would be roughly 4 percent of that increase in earnings.

20. A small portion of the premium assistance tax credits represents a reduction in revenues.

21. There would also be budgetary effects of some people's moving between eligibility categories for Medicaid and some people's moving between Medicaid and the Children's Health Insurance Program.

under current law and would receive higher earnings because of a minimum-wage increase would lose eligibility for Medicaid. Some of those people would gain eligibility for subsidized coverage through exchanges and would choose to take up that coverage; for those people, federal costs would rise. However, some of the people who would lose eligibility for Medicaid would not gain eligibility for subsidized coverage through exchanges (because their income would still be too low) or would gain eligibility but would choose not to take up that coverage (in part because they would have to pay a portion of their premiums themselves); for those people, federal costs would fall. Moreover, some people who, under current law, will not be eligible either for Medicaid or for subsidized coverage through exchanges (because they live in a state that has not expanded Medicaid coverage under the Affordable Care Act but will have too little income to qualify for the subsidies) would gain eligibility for subsidized coverage through exchanges and would choose to take up that coverage; for those people, federal costs would rise. The net federal cost of those various shifts would be small, CBO expects.

Effects for People Whose Income Would Fall

Apart from the group of workers whose earnings rose because of a minimum-wage increase, other people would generally see a reduction in real income, CBO estimates. Some of the reduction would consist of lower earnings for workers who became jobless for at least part of a year because of the change in policy. Some would consist of lower profits for business owners. The remainder would come from higher prices, which would reduce real income. However, it is unclear how much of the total reduction in income would come from each of those sources, and that allocation would affect the impact of a minimum-wage increase on the federal budget. CBO has not estimated the effective federal marginal tax rate for that collection of reductions in income, but the agency anticipates that it would probably be slightly smaller than the effective federal marginal tax rate for the people who would receive higher income.

CBO estimates that workers who were jobless for at least part of a year because of the minimum-wage increase would suffer a loss of real income. As a result, those workers would pay less in taxes and receive more in benefits. The effective federal marginal tax rate for those workers would be similar in magnitude to the rate for workers whose earnings rose.

CBO estimates that profits would also be lower. The lower profits would mean less in personal and corporate income tax receipts. CBO expects that some of the reduction in profits would be for businesses subject to the corporate tax, which would lower corporate tax receipts; the reduction in profits would also indirectly reduce personal income tax receipts, because stockholders' dividend income and realized capital gains on corporate stock would be lower. For those firms, CBO estimates that the decline in corporate and personal tax payments would amount to roughly one-third of the decline in profits. However, some of the reduction in profits would be for firms not subject to the corporate tax, most of whose income is directly subject to the individual income tax. For those firms, the resulting reduction in individual income tax payments could be somewhat lower, as a share of the reduction in profits, than the estimated one-third decline for firms subject to the corporate tax.

Prices would rise as a result of a minimum-wage increase, according to CBO's analysis. That increase in prices would raise federal transfer payments, because some of those payments, such as Social Security, are automatically indexed to changes in the price level. An increase in prices would also reduce federal personal income taxes, because many parameters of the tax system change automatically when the price level rises. Federal spending that is not subject to statutory caps and is not indexed to changes in the price level might also increase, although the extent of that increase would depend on the concentration of minimum-wage workers in the sectors of the economy in which the federal government was doing such spending. CBO was not able to estimate the effective marginal tax rate from the collection of changes in taxes and spending that would take place because of price changes.



Appendix A: The Basis of CBO's Findings

This appendix describes the steps that the Congressional Budget Office (CBO) took to arrive at the estimates in this report—estimates of the number of low-wage workers affected by the two options for increasing the minimum wage; of the responsiveness of employment to changes in the minimum wage; of the options' total effects on employment; and of the options' effects on family income.

How CBO Estimated the Number of Workers Who Would Be Affected by the Options

CBO estimated the number of workers who would be directly affected by the two options for increasing the federal minimum wage. Directly affected workers are those whose wages would otherwise have been below the new federal minimum and who therefore would either receive a higher wage or become jobless if the new federal minimum was imposed. In 2016, CBO estimates, about 17.0 million workers would be directly affected by the \$10.10 option and 7.7 million by the \$9.00 option. CBO also estimated the number of workers whose wages would otherwise have been slightly above (as defined later in this section) the new federal minimum in 2016 and who would probably also be affected by a change in the minimum wage. Under the \$10.10 option, there would be 8.0 million such workers; under the \$9.00 option, 4.1 million. (The 33 million workers mentioned in the text—which refers to all workers who are projected to earn less than \$11.50 under current law—includes not only the 17.0 million directly affected workers under the \$10.10 option and the 8.0 million workers with wages slightly above \$10.10 but also some workers, generally at the low end of that range, who are not covered by minimum-wage laws and some workers, at the high end of that range, who live in states projected to have high

minimum wages in 2016 and who therefore would probably not be affected by a change in the federal minimum.)

Of the 17.0 million workers directly affected by the \$10.10 option, 16.5 million would end up with higher earnings during an average week in the second half of 2016, and 500,000 would end up jobless and therefore with lower earnings (as estimated using the approach described below). Of the 7.7 million workers directly affected by the \$9.00 option, 7.6 million would end up with higher earnings during an average week in the second half of 2016, and 100,000 would end up jobless and therefore with lower earnings, according to CBO's estimate.

Workers Who Would Be Directly Affected by Increases in the Minimum Wage

CBO estimated the number of directly affected workers in three main steps: calculating the distribution of hourly wages in 2013; projecting the wage distribution in 2016 under current law; and identifying the workers who would be directly affected by a change in the federal minimum wage in 2016.

In the first step, CBO calculated hourly wages for all workers in calendar year 2013, using monthly data from the Census Bureau's Current Population Survey (CPS), which collects information from about 60,000 households. The CPS is designed to be representative of the U.S. civilian population as a whole; each observation in the survey represents a number of people, and that number is the observation's "sample weight." CBO used those sample weights to estimate effects for the entire population on the basis of the people who were surveyed. When respondents to the survey did not report an hourly wage, their hourly wages were calculated as their usual earnings

per week divided by their usual hours worked per week.¹ Because calculated wages are subject to error, CBO adjusted those wages to be a weighted average of a worker's calculated wage and the average wage of workers with similar characteristics—increasing calculated wages that were below the group average and decreasing wages that were above it.²

In the second step, CBO applied forecasts of employment and wage growth to the hourly wages that it had calculated for 2013 to project the distribution of workers' hourly wages in 2016 under current law. CBO expects that very high-wage workers will experience faster wage growth in the next several years than will workers as a whole, so the forecast of wage growth for low-wage workers used in this analysis was smaller than the one in the agency's overall economic forecast. The forecast of wage growth also accounted for the penalties, imposed under the Affordable Care Act, that some employers will pay for not providing qualifying health insurance; those employers will probably pass along the cost of those penalties to their workers in the form of reduced wages.³ In addition, CBO accounted for prospective increases in some states' minimum wages, including both changes scheduled in current state laws and changes projected on the basis of how states have changed their minimum wages in the past. That adjustment boosted projected wage growth for workers in those states. Altogether, CBO projected that nominal wages of low-wage workers—for example, those at the 10th percentile of the wage distribution—would grow at an average annual rate of 2.9 percent between 2013 and 2016 under current law.

1. If the number of hours that the respondents usually worked per week varied, CBO used the number of hours that they reported having worked during the week prior to the survey. If that number was unavailable, CBO used the average hours of full-time or part-time workers, as appropriate. If the Census Bureau imputed an hourly wage for the worker, CBO used that wage.
2. That adjustment is based in part on findings from Thomas Lemieux, "Increasing Residual Wage Inequality: Composition Effects, Noisy Data, or Rising Demand for Skill?" *American Economic Review*, vol. 96, no. 3 (June 2006), pp. 461–498, <http://dx.doi.org/10.1257/aer.96.3.461>.
3. See Congressional Budget Office, *The Budget and Economic Outlook: 2014 to 2024* (February 2014), Appendix C, www.cbo.gov/publication/45010. That forecast of wage growth was made in December 2013 and does not account for subsequent developments.

In the third step, CBO identified workers who would be directly affected by a change in the federal minimum wage in 2016. That group includes most workers projected to have hourly wages lower than the new minimum. However, it does not include 2.6 million low-wage workers who, CBO projects, would not be covered or affected by the Fair Labor Standards Act (FLSA).⁴ The group of directly affected workers does include 3.5 million workers who, though they may not be covered by the FLSA, are expected by CBO to be affected by an increase in the federal minimum because their hourly wages tend to be as concentrated near the minimum as are the wages of workers covered by the FLSA; those 3.5 million workers consist of employees of small firms, workers in occupations generally exempt from the FLSA, and teenagers in their first 90 days of employment.⁵

CBO distinguished tipped from nontipped workers because a separate minimum cash hourly wage applies to workers who receive more than \$30 per month in tips. Under the FLSA and many state laws, employers may pay such workers a lower cash hourly wage if tips bring their total hourly earnings above the minimum hourly wage. To estimate the number of tipped workers, CBO applied the lower minimum cash wage to workers in 11 occupations (such as waiter, bartender, and hairdresser) whose compensation depends heavily on tips. They constitute about 10 percent of low-wage workers.

Other Workers Who Would Probably Be Affected by Increases in the Minimum Wage

CBO also considered the effects of a minimum-wage increase on the wages and employment of workers whose wages would otherwise have been higher than the new

4. To project the percentage of low-wage workers who would not be covered or affected by the FLSA in 2016, CBO estimated the share earning less than the federal minimum wage (or their state's minimum wage, if higher) in 2013, which was 12 percent. Because the agency concluded that nontipped workers who reported being paid up to 25 cents less, and tipped workers who reported being paid up to 13 cents less, than the federal minimum wage—or the state minimum, if it was higher—had probably misreported their wages, it did not count such workers as being paid less than the minimum wage. The analysis does not account for localities' minimum wages because it uses data from the CPS, which does not identify the localities in which respondents work.
5. Department of Labor, "Wages and Hours Worked: Minimum Wage and Overtime Pay" (accessed January 23, 2014), www.dol.gov/compliance/guide/minwage.htm.

federal minimum in 2016. Those effects could be positive or negative for any particular worker, depending on whether that worker's value to a firm would be higher or lower if lower-wage workers became more expensive to employ. Available research, however, suggests that the average effect on the wages of those workers would be positive. (See Appendix B for a list of studies that CBO reviewed.)

In its analysis, CBO assumed that such "ripple effects" would probably apply to workers whose projected wage in 2016 was up to the amount that would result from an increase that was 50 percent larger than the increase in their effective minimum wage (incorporating both their state minimum and the new federal minimum) under either option. Thus, in states where the current minimum wage is \$7.25, CBO anticipates that workers earning up to about \$11.50 per hour would probably be affected by the \$10.10 option. In states with a higher minimum wage, the ripple effect would be much smaller. For instance, under current California law, the minimum wage is scheduled to increase to \$10.00 in 2016, and in that state, only workers earning up to \$10.15 per hour would probably be affected by an increase to \$10.10 in the federal minimum, by CBO's estimate.

Ripple effects added 8.0 million potentially affected workers to CBO's analysis under the \$10.10 option and 4.1 million under the \$9.00 option. Although CBO estimates that wage increases under the options are much more likely for those workers than for workers with still higher wages, the agency does not expect that all of them would receive wage increases. CBO did not have a basis for estimating the total number of workers whose earnings would rise, although that number would be less than the total number of potentially affected workers.

Uncertainty in the Estimates

Estimates of the total number of potentially affected workers are uncertain for at least four reasons. The first and most important is that, if CBO's forecast of wage growth for low-wage workers between 2013 and 2016 is either too high or too low, the result will be an underestimate or an overestimate, respectively, of the number of workers who would be directly affected by a change in the federal minimum wage. Second, determining whether workers are covered by the FLSA on the basis of what they report to the CPS yields inaccuracies. For instance, some respondents undoubtedly misreported their wages, earnings, or hours worked, leading CBO to classify some

unaffected workers as affected and vice versa; similarly, the use of occupation to classify people as tipped workers results in inaccuracies.⁶ Third, changes in states' minimum wages could be different from what CBO projects. Fourth, the ripple effects could be smaller or larger than CBO projects.

How CBO Estimated the Responsiveness of Employment to Increases in Minimum Wages

CBO reviewed a large body of research to estimate how adopting either of the two options for increasing the minimum wage would affect employment. Such research typically calculates an employment elasticity—that is, the percentage change in employment induced by a percentage change in the minimum wage. Researchers have generally focused on the employment of workers with low average wages, such as teenagers, high school dropouts, and workers in low-wage industries. Initially focusing on estimates of the employment elasticity for teenagers (in part because they were the most commonly studied group), CBO arrived at a teen-employment elasticity for each of the options, after accounting for the fact that the \$10.10 option differed significantly from the scenarios explored by prior research. CBO then synthesized the teen elasticities with broader research to construct elasticities for adults. (See Appendix B for a bibliography of the research that CBO reviewed.)

The elasticities discussed in this section would apply only to directly affected workers and not to others whose wages would be higher than the new minimum wages under the options. For example, CBO concluded that the \$9.00 option probably would not affect the employment of workers who would earn more than \$9.00 in 2016 under current law (except by increasing overall demand for goods and services, an effect discussed below). That conclusion was the result of considering two opposing factors. On the one hand, wages would probably increase for some of those workers (such as the supervisors of minimum-wage workers), as firms sought to maintain a differential between their wages and those of employees earning the minimum wage—and that wage increase

6. For a discussion of mismeasured wage rates, see, for example, John Bound, Charles Brown, and Nancy Mathiowetz, "Measurement Error in Survey Data," in James J. Heckman and Edward Leamer, eds., *Handbook of Econometrics*, vol. 5 (Elsevier, 2001), pp. 3705–3843, [http://dx.doi.org/10.1016/S1573-4412\(01\)05012-7](http://dx.doi.org/10.1016/S1573-4412(01)05012-7).

would tend to lower employment. On the other hand, some firms would probably employ more workers with wages higher than the new minimum, because the productivity of those workers relative to their wages would be higher than that of workers whose wages had been pushed up by the minimum-wage increase.

Elasticities for Teenagers Under the \$9.00 Option

CBO reviewed the economic research to develop a range of estimates of the elasticity of teen employment with respect to a change in the minimum wage under the \$9.00 option. On the basis of that review, CBO selected a central estimate of that elasticity of -0.075 ; in other words, a 10 percent increase in the minimum wage would reduce employment among teenage workers by three-quarters of one percent. However, there is considerable uncertainty about that elasticity, and CBO developed a range of estimates to reflect that uncertainty. The high end of the likely range was -0.15 and the low end was zero. In CBO's assessment, there is about a two-thirds chance that the effect of the \$9.00 option on the employment of teenage workers would lie within that range. Some studies, however, have found that increases in the minimum wage raise employment slightly, while others have found much larger negative effects on employment than are reflected in CBO's range.

Several factors influenced CBO's conclusion about the range of elasticities for teenagers. First, CBO put more weight on studies using certain methodologies than on other studies. Several studies compare employment rates among states that have different minimum wages but otherwise similar labor markets; such analyses plausibly isolate the effects of minimum wages from the effects of national economic changes, such as fluctuations in the business cycle. Other studies try to isolate the employment effects of minimum-wage increases by comparing the national employment rate in years when the minimum wage was high to the rate in years when the minimum wage was low. CBO put the most weight on the studies of state-by-state differences, judging those studies to have estimated more accurately the effects of minimum wages on employment. Changes in state minimum wages are sometimes related to local economic conditions in ways that could lead elasticity estimates based on those changes to be higher or lower than the elasticity that would apply to similar changes in law in the future; CBO considered studies that took a variety of approaches to addressing that issue.

Second, CBO considered the role of publication bias in its analysis. Academic journals tend to publish studies whose reported effects can be distinguished from no effect with a sufficient degree of statistical precision. According to some analyses of the minimum-wage literature, an unexpectedly large number of studies report a negative effect on employment with a degree of precision just above conventional thresholds for publication. That would suggest that journals' failure to publish studies finding weak effects of minimum-wage changes on employment may have led to a published literature skewed toward stronger effects. CBO therefore located its range of plausible elasticities slightly closer to zero—that is, indicating a weaker effect on employment—than it would have otherwise.

Third, CBO considered whether economic conditions in 2016 could lead the responsiveness of employment to an increase in the minimum wage to be larger than it had been in the past. One recent study has found evidence that the employment elasticity is more negative when unemployment is high. However, CBO projects a national unemployment rate of about 6 percent for 2016—a rate similar to the average of unemployment rates during the periods studied in the literature from which CBO drew elasticity estimates.⁷ CBO therefore did not adjust its central elasticity estimates to account for economic conditions in 2016.

However, the extent to which employment would respond to changes in the minimum wage in 2016 in the same way that it has in past years is uncertain. For example, the relatively slow growth in the wages of low-wage workers observed in the past few decades has been partly attributed by many analysts to growth in information and other technologies, which have automated some of the tasks traditionally done by those workers. Continued improvements in such technology will probably lead to the automation of some other tasks that they still perform, such as payment collection at retail stores. The pace of technological innovation, though, is difficult to predict. Uncertainty about future developments in the labor market is reflected in CBO's range of estimates.

7. See Congressional Budget Office, *The Budget and Economic Outlook: 2014 to 2024* (February 2014), www.cbo.gov/publication/45010. For additional information about CBO's projections of future labor market conditions, see Congressional Budget Office, *The Slow Recovery of the Labor Market* (February 2014), www.cbo.gov/publication/45011.

Elasticities for Teenagers Under the \$10.10 Option

In analyzing the \$10.10 option, CBO used a central estimate of the elasticity of employment for teenagers of -0.10, with a likely range from a very slight negative amount to -0.20. Four main factors differentiate the \$10.10 option from the \$9.00 option and from policies studied in previous research, leading CBO to conclude that the elasticity would be larger (in absolute value) under the \$10.10 option.

First, the \$10.10 option would index the minimum wage to inflation and would therefore result in a higher minimum wage for many years in the future. The federal minimum wage has not been previously indexed to inflation, and some employers may have refrained from reducing employment in response to prior minimum-wage increases, realizing that inflation would soon erode the cost of those increases. Therefore, an indexed minimum wage would probably reduce employment more than a nonindexed minimum wage would—and neither the \$9.00 option nor most policies studied in past research are indexed.

Second, most studies measure changes in employment over a short term, typically a year or two. However, employment reductions after a minimum-wage increase are probably larger over a longer term, in part because those reductions may be less attributable to the elimination of existing low-wage jobs than to slower *growth* in the number of low-wage jobs, which is difficult to detect in short-term studies. CBO assessed the effects of both options in the second half of 2016—two years after the first step of the \$10.10 option, but only one year after the first step of the \$9.00 option. That longer lag between the initiation of the option and the evaluation date led CBO to estimate a larger elasticity for the \$10.10 option than for the \$9.00 option.

Third, raising the minimum wage from \$7.25 to \$10.10 represents a 39 percent increase, which would be larger than most of the increases that have been studied, and CBO expects that employment would be more responsive to a larger increase.⁸ Many employers incur adjustment costs when they reduce staffing (especially if that requires restructuring their operations), which may deter them from laying off low-wage workers in response to a small increase in the minimum wage. But the savings from not having those employees are more likely to exceed the adjustment costs when the minimum-wage increase is large.⁹

Fourth, the \$10.10 option would apply to a larger fraction of the workforce—one that accounts for about 10 percent of all hours worked, CBO projects—than many previous increases did. It would do so not only because the percentage increase is large, but also because the minimum wage before the increase would be higher in real (inflation-adjusted) terms than it was before many previous increases (see Figure 1 on page 5).¹⁰ For example, although the percentage increase in the federal minimum wage from 2007 to 2009 was similar to the one projected under the \$10.10 option, the fraction of the workforce affected under that option would be about five times as large (see Table A-1).¹¹ When a greater proportion of a firm's work hours are affected by the minimum wage, the adjustment cost per worker of reducing staffing (again, especially if the firm is restructuring its operations) is probably smaller, making the firm more likely to reduce employment.

Translating Elasticities From Previous Research for Use in CBO's Analysis

In order to project the change in employment that would result from the \$9.00 and \$10.10 options, CBO

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8. The last increase in the federal minimum wage, implemented between 2007 and 2009, constituted a 41 percent increase, but earlier percentage increases were typically lower. Some states have implemented large percentage increases in the minimum wage, however. New York, for example, increased its minimum wage from \$5.15 to \$7.15 per hour—a 39 percent increase—between 2005 and 2007.
 9. In addition, at the same time that the proposed increases in the minimum wage would take effect, the Affordable Care Act's requirement that many employers provide health insurance (or pay a penalty if they do not) will impose an additional cost on employers for some low-wage workers who do not currently have employment-based health insurance. CBO expects that the cost will ultimately be borne by workers through lower wages; but before that adjustment has fully taken effect, the cost further boosts the likelihood that employers' savings from reducing the size of their workforces would exceed their adjustment costs.
 10. The 10 percent of work hours affected in 2016 by the \$10.10 option is not directly comparable to the percentage of workers projected to make less than \$10.10 per hour in 2016 as reported in Figure 1. That percentage is based on a count of workers, rather than of hours worked, and it includes workers making less than \$10.10 who are not covered by the FLSA.
 11. The 10 percent of work hours affected in 2016 by the \$10.10 option reported above differs from the 11.4 percent in 2016 reported in Table A-1 mainly because of the different definition of directly affected workers used in Table A-1 to create a consistent series over time.

Table A-1.**Comparing Changes in the Federal Minimum Wage Since 1980 With Changes Under the Two Options**

Year of the Minimum-Wage Increase ^a	Percentage of Workers Earning Between the Old and New Minimum Wages	Percentage of Hours Worked by Workers Earning Between the Old and New Minimum Wages
Changes Since 1980		
1980	10.9	8.6
1981	11.7	9.2
1990	4.3	3.2
1991	5.2	4.0
1996	3.4	2.5
1997	5.8	4.3
2007	1.3	0.9
2008	1.9	1.4
2009	2.7	2.0
Average	5.3	4.0
Projected Changes Under the \$9.00 Option^b		
2015	3.9	2.3
2016	7.7	5.7
Average	5.8	4.0
Projected Changes Under the \$10.10 Option^c		
2014	6.3	4.7
2015	10.0	7.7
2016	14.1	11.4
Average	10.1	7.9

Source: Congressional Budget Office based on the Census Bureau's Current Population Survey and on data from the Department of Labor.

Note: For the analysis in this table, to create a consistent series over time, CBO focused on groups of workers earning between the old minimum wage and the new minimum wage that was scheduled to take effect within a year. To allow for some misreporting of wages, workers earning slightly below the old minimum wage were also included. The hours worked were those reported prior to the increase in the minimum wage. Those groups of workers differ from the groups of directly affected workers under the options discussed elsewhere in this report because they do not account for any wage growth, within the year prior to the new minimum wage's taking effect, that would have occurred if the minimum wage had not been raised, or for increases in state minimum wages that would have increased workers' wages during the period.

- The amendments to the Fair Labor Standards Act of 1938 mandating the minimum wage increases for these years were enacted in 1977, 1989, 1996, and 2007.
- The minimum wage would rise (in three steps, starting in 2014) to \$10.10 by July 1, 2016, and then be indexed to inflation.
- The minimum wage would rise (in two steps, starting in 2015) to \$9.00 by July 1, 2016, and would not be subsequently indexed to inflation.

converted the elasticity estimates that it drew from the literature on teenage workers to elasticity estimates for directly affected teenagers and adults.

Elasticities for Directly Affected Teenagers. The research discussed above typically defines employment elasticity (e) as the responsiveness in the employment ($\% \Delta E$) of a group of workers, such as teenagers, to a change in the applicable minimum wage ($\% \Delta MW$ —that is, the

change in the federal or state minimum, whichever is higher), as shown in the following equation:

$$e_{literature} = \frac{\% \Delta E}{\% \Delta MW}$$

The elasticity ranges reported earlier in this appendix are based on that approach so that they will be more easily comparable to the elasticities typically reported in the research literature. In its calculations, however, CBO used

elasticities that were modified in two ways to be more accurate estimates of the effect of the options.

The first modification that CBO made arose because the literature typically focuses on the historical employment response of *all* teenagers to a change in the minimum wage. Many of those teenagers initially had low wages and, when the minimum wage rose, received a wage increase (or were rendered jobless); but many other teenagers had wages that were higher than the new minimum and therefore were largely unaffected by the change. In contrast, CBO's approach examines the responsiveness of employment of only *directly affected* teenagers to a change in the minimum wage—that is, the responsiveness of employment of those who would otherwise earn less than the new minimum wage. When analyzing the \$10.10 option, for example, CBO's approach focuses on the responsiveness of teenage workers who would have earned less than \$10.10 per hour in 2016 if the option had not been implemented. The two approaches are similar, but they can yield different results when the fraction of teenagers with low wages varies over time and with policy changes. In CBO's view, an approach that focuses on the response of low-wage workers is more accurate.

The second modification that CBO made was to use elasticities that relate employment not to changes in the minimum wage itself but to average changes in workers' wages induced by a change in the minimum wage. (For instance, a worker who would otherwise have earned \$9.00 per hour would receive a 12 percent increase if the minimum wage rose to \$10.10. However, the minimum wage for that worker would rise from \$7.25 to \$10.10, an increase of 39 percent.) The elasticities that are typically reported in the literature are scaled to the increase in the minimum wage itself—but for two reasons, an approach relying on them is not as well suited for projecting the change in employment resulting from a future change in the minimum wage. First, that approach does not incorporate information about the distribution of workers' wages. For example, in a projection of the effect of the \$10.10 option, it would make no difference, under that approach, whether most workers would otherwise have earned \$7.25 or \$10.09. Second, that approach regards all directly affected workers as equally likely to lose their jobs after a minimum-wage increase, no matter what they would otherwise have been paid. In CBO's view, by contrast, workers whose wages are just below the new minimum wage are more likely to remain employed after

it increases than workers who are earning substantially less and are probably less valuable to the employer. CBO's approach accounts for the distribution of workers' wages and for the difference in the likelihood of losing one's job.

CBO calculated the responsiveness of employment among directly affected teenagers by dividing the elasticities drawn from the literature by the portion of employed teenagers who would earn less than the new minimum wage before its implementation (p_{direct}) and then multiplying by the ratio of the percentage change in the applicable minimum wage ($\% \Delta MW$) to the average percentage change in the wages of those teenagers ($\% \Delta W_{direct}$).¹² The following equation shows the calculation:

$$e_{direct} \equiv \frac{\% \Delta E_{direct}}{\% \Delta W_{direct}} = \frac{e_{literature}}{p_{direct}} \times \frac{\% \Delta MW}{\% \Delta W_{direct}}$$

CBO calculated those conversion factors using CPS data from 1979 through 2009. The CPS data indicate that past increases in the minimum wage typically affected about a third of employed teenagers and were typically about 50 percent higher than the average of the wage changes necessary for compliance with the new minimum. Thus, elasticities for directly affected teenagers are about 4.5 times higher, CBO estimates, than the teen-employment elasticities with respect to the change in the applicable minimum wage discussed in the previous section.

Elasticities for Directly Affected Adults. Much less research has been conducted on the responsiveness of adult employment to minimum-wage increases than on the responsiveness of teenage employment. Using the available information, CBO concluded that the elasticity for directly affected adults was about one-third of the elasticity for directly affected teenagers, and the agency

12. A similar conversion was used in Charles Brown, "Minimum Wages, Employment, and the Distribution of Income," in Orley Ashenfelter and David Card, eds., *Handbook of Labor Economics*, vol. 3B (Elsevier, 1999), pp. 2101–2163, <http://tinyurl.com/omxr3p7>, and in David Neumark and William L. Wascher, *Minimum Wages* (MIT Press, 2008), <http://mitpress.mit.edu/books/minimum-wages>. The conversion relies on the assertion that the increase in the minimum wage does not have a net effect on employment for workers earning more than the new minimum wage. As discussed earlier, CBO concluded that the research supports that assertion, with the exception of the increase in employment that would result from greater overall demand for goods and services. The adjustment made to account for that increase in employment is discussed in the section "How CBO Estimated the Total Effects of the Options on Employment."

applied that proportional adjustment to the central estimates and likely ranges of elasticities for teens discussed above.

Some studies have found large elasticities for particular groups of adults, such as high school dropouts or African Americans in their 20s, but most of the adults who would be affected by the \$9.00 and \$10.10 options would not fall into those categories. A study that tracked directly affected adults regardless of their education, age, or race suggests that their employment is less sensitive to increases in the minimum wage than that of directly affected teenagers. One explanation for that lower degree of responsiveness is that employers facing an excess of workers or of job applicants tend to favor adults over teenagers. Supporting that explanation is research suggesting that encouraging employment among low-wage parents reduces employment among younger, childless adults.

CBO also reviewed studies that examined the response of employment to changes in the minimum wage for other groups of workers, such as those in particular industries. Those results were broadly consistent with CBO's findings for teenagers and adults after being adjusted to avoid apples-to-oranges comparisons. For example, several studies of the food and drink industry measured elasticities in terms of the change in all employment in the industry stemming from a change in the applicable minimum wage. Many of the employees at those businesses did not have wages low enough to be directly affected by a minimum-wage change; that factor largely accounts for differences between the smaller elasticities typically reported in studies of the food-and-drink industry and CBO's estimates of elasticities for directly affected workers.

How CBO Estimated the Total Effects of the Options on Employment

CBO's central estimates that the \$10.10 and \$9.00 options would reduce employment by roughly 500,000 and 100,000 workers, respectively, were based on four main factors. Two were discussed above: the number of low-wage workers directly affected by the options and the responsiveness of the employment of low-wage workers to increases in minimum wages. The remaining two factors were the change in the wages of directly affected workers and the increase in demand for goods and services caused by each option. To calculate

the total effect on employment, CBO multiplied estimates of the first three factors together for teenagers; did the same for adults; added the results; and then added an amount to account for the fourth factor. To reflect the considerable uncertainty in estimating the total employment effect, CBO also reported a range within which, in the agency's assessment, there was about a two-thirds chance that the actual effect would lie.

The Increase in the Wages of Directly Affected Workers

CBO first projected wages for all workers in 2016 under current law; it then increased wages that would be below the new minimum wage under consideration to equal that new minimum. The difference between the directly affected workers' wages before and after that adjustment was used to calculate the average percentage changes in directly affected workers' wages (before accounting for job losses caused by the minimum-wage increase). Under the \$10.10 option, CBO projects average percentage changes of about 18 percent for teenagers and 14 percent for adults. The projected changes are smaller under the \$9.00 option—10 percent for teenagers and 8 percent for adults. All those percentage changes are lower than the percentage changes in the minimum wage itself because most low-wage workers in 2016 would earn more than \$7.25 under current law.

The Increase in Demand for Goods and Services

Raising the minimum wage would have four direct effects on the aggregate demand for goods and services. First, consumption would be reduced among people who became jobless because of the minimum-wage increase. In estimating that effect, CBO accounted for lower savings and some borrowing by people who would thereby avoid a sharp reduction in their standard of living. Second, additional spending by affected workers with earnings increases would boost demand. Third, demand would be reduced because business owners and shareholders would absorb part of the cost of the minimum-wage increase in the form of reduced profits and therefore would reduce their spending. Fourth, demand would also be reduced because affected employers would pass part of their increased costs on to consumers in the form of higher prices for goods and services; those higher prices would reduce the average consumer's purchasing power, resulting in less spending by consumers after adjusting for inflation. (For examples of the research that CBO reviewed on these topics, see Appendix B.)

On balance, according to CBO's analysis, raising the minimum wage would increase demand for goods and services because, taken together, the second, third, and fourth direct effects would shift income from business owners and consumers (as a whole) to low-wage workers. Low-wage workers generally spend a larger share of each dollar they receive than the average business owner or consumer does; thus, when a dollar from business owners or consumers is shifted to low-wage workers, overall spending increases. The increase in demand from that shifting of income would be larger than the decrease in demand from the reduced consumption of people who became jobless, CBO estimates.

Increasing the minimum wage would also have indirect effects on demand that could either enhance or reduce the direct effects. For instance, the greater demand for goods and services just described would prompt some companies to increase investment to bolster their future production, further boosting demand. But higher prices of goods and services sold by companies employing minimum-wage workers would cause consumers to shift their purchases to other companies, potentially creating bottlenecks until those companies adjusted to the increased demand. On net, the indirect effects would reduce demand, according to CBO's central estimates. (Under current conditions, the indirect effects would increase demand, CBO estimates, but they would reduce demand in 2016 because the economy will be stronger and the Federal Reserve would therefore be more active in offsetting the direct increase in demand by raising interest rates.)

The increased demand for goods and services that would result from an increase in the minimum wage would have a short-term impact, boosting employment by a few tens of thousands of workers in the second half of 2016 under the \$10.10 option, CBO estimates. The agency's estimation approach was similar to the one that it used to assess the effects of the American Recovery and Reinvestment Act of 2009 (ARRA) and of various policies designed to increase output and employment—but adjusted to account for the much stronger economy projected for late 2016.¹³ Specifically, CBO estimated the impact of both the \$10.10 option and the \$9.00 option on demand while accounting for both the direct and indirect effects. Then CBO estimated the effect of those changes in demand on productivity, hours worked per worker, and employment, using historical relationships as a guide. Changes in demand would affect employment gradually,

over several quarters, because part of a rise in output would initially result in higher productivity and hours worked per worker, rather than in increased employment.

The overall increase in demand from boosting the minimum wage, and the resulting increase in employment, are represented in the findings of most previous research only to a small extent. For example, a study of impacts on directly affected workers captures the macroeconomic effects only on those workers, not on all workers. Also, a study of a minimum-wage increase in a given state may capture its effects on demand for in-state goods but not for out-of-state goods. After analyzing the importance of such factors, CBO concluded that previous research incorporated roughly 10 percent of the overall effects on aggregate demand. CBO therefore reduced its estimate of the economywide demand effects of a minimum-wage increase by about 10 percent to avoid double-counting those effects.

Uncertainty in the Estimates

CBO produced a range of estimates of the effect of increasing the minimum wage on employment by analyzing various sources of uncertainty. The three most important were the growth in wages of affected workers under current law over the next three years, the responsiveness of employment to changes in wages, and the extent to which an increase in aggregate demand because of higher labor earnings would increase employment. CBO concluded that two further sources of uncertainty—sampling variability in the CPS and the level of state minimum wages in 2016—were relatively insignificant.

To estimate a range of values for wage growth, CBO examined the history of wage growth rates and the extent to which those rates varied over three-year periods. To estimate a range for the responsiveness of employment to changes in wages, CBO used the elasticity ranges developed for the two options that were discussed above. CBO measured uncertainty in aggregate demand effects by

13. See Congressional Budget Office, *Estimated Impact of the American Recovery and Reinvestment Act on Employment and Economic Output From October 2012 Through December 2012* (February 2013), www.cbo.gov/publication/43945; and testimony of Douglas W. Elmendorf, Director, Congressional Budget Office, before the Senate Committee on the Budget, *Policies for Increasing Economic Growth and Employment in 2012 and 2013* (November 15, 2011), www.cbo.gov/publication/42717.

using methods similar to those that it used in its analysis of ARRA.¹⁴

Building on those ranges of wage growth, elasticities, and aggregate demand effects, CBO generated simulations of effects on employment that incorporated the likelihood that wage growth could be higher or lower by a certain amount, the likelihood that elasticities could be larger or smaller to a certain extent, and other sources of uncertainty. CBO used the results of those estimates to form a range for the effect on employment of each policy option. There is a two-thirds chance, in CBO's assessment, that the actual effects would be within the ranges reported.

How CBO Estimated the Effects of the Options on Family Income

CBO analyzed the effects on family income of the two options for increasing the federal minimum wage by comparing a projected distribution of family income in 2016 under current law with the distribution that would prevail if the federal minimum wage was increased to either \$10.10 or \$9.00. The monthly data from the CPS that CBO used in its analysis of employment did not contain the information on family income necessary for this analysis, so CBO instead used data from the CPS Annual Social and Economic Supplement (ASEC) that was administered in March 2013, which reported family income and individuals' earnings for calendar year 2012.

Wages and Family Income Under Current Law

Before it could estimate the effect of the two options on family income in 2016, CBO needed to project family income under current law. CBO used a two-step process similar to the one that it used in its employment analysis—first calculating hourly wages and annual family income in 2012 and then using those calculations to project wages and family income in 2016.

Hourly Wages and Annual Family Income in 2012.

CBO estimated the hourly wages of workers and annual income of families in 2012 by using data from the 2013

ASEC. Workers' hourly wages were calculated as their annual earnings divided by the number of hours they worked during the year (calculated as the number of hours they usually worked per week times the number of weeks they worked during the year).¹⁵ As in its analysis of employment, CBO adjusted workers' calculated wages up or down to move their wage toward the average wage for workers with similar observable characteristics.

However, when CBO used those data to project workers' wages in 2016, it found far fewer workers who would be directly affected by the change in the minimum wage than it had in its analysis of employment.¹⁶ The discrepancy probably arose because of greater measurement error in the ASEC than in the monthly CPS, which reports wages according to people's responses to a direct question about how much they earn per hour. CBO therefore further adjusted the distribution of hourly wages calculated from the ASEC to match more closely the analogous distribution from the monthly CPS, mostly by adjusting some workers' wages up to the minimum wage projected to apply to them in 2016 under current law.¹⁷

CBO also used the ASEC to measure the distribution of before-tax family cash income in 2012, which is the measure that the Census Bureau uses to determine the official poverty rate. That measure of income includes labor earnings, capital and business income, and other private sources of income, as well as cash transfers from the government, such as Supplemental Security Income (SSI) and Social Security (both Old-Age and Survivors

15. CBO did not exclude observations for which the Census Bureau imputed annual earnings, the number of hours of work per week, or the number of weeks worked per year.

16. To be consistent with the analysis of the number of workers affected by an increase in the minimum wage, CBO identified nontipped workers who were paid up to 25 cents less and tipped workers who were paid up to 13 cents less than the federal minimum wage—or the state minimum if it was higher—as workers who would be affected by a change in the minimum wage.

17. As it did in estimating the number of affected workers, CBO identified tipped workers as those in 11 occupations (such as waiter, bartender, and hairdresser) whose compensation depends heavily on tips. Throughout its analysis, CBO applied to those workers the lower minimum wage for tipped workers.

14. Felix Reichling and Charles Whalen, *Assessing the Short-Term Effects on Output of Changes in Federal Fiscal Policies*, Working Paper 2012-08 (Congressional Budget Office, May 2012), www.cbo.gov/publication/43278.

Insurance and Disability Insurance payments).¹⁸ It does not include noncash government transfers, such as benefits provided through the Supplemental Nutrition Assistance Program (SNAP), Medicaid, or Medicare, nor does it reflect the taxes people pay or the tax credits they receive, such as the earned income tax credit (EITC).

Projecting Hourly Wages and Annual Family Income in 2016. CBO used the calculations described above and its forecasts of growth in wages and other income to project the distribution of hourly wages and annual family income in 2016.¹⁹ As in the employment analysis, the forecast of wage growth used for this analysis was smaller than the agency's overall forecast of wage growth because CBO expects that very high-wage workers will experience faster wage growth in the next several years than other workers will.²⁰ In addition, CBO accounted for prospective increases in some states' minimum wages, including changes scheduled in current state laws and changes projected on the basis of how states have changed their minimum wages in the past.

To project family income in 2016, CBO used its forecasts of growth in the components of income when they were available—as they were for interest and dividends, for example. CBO projected that the other components of income will grow at the same rate that the price index for personal consumption expenditures does in CBO's forecast. CBO estimated that the number of workers will increase according to the agency's forecast of employment growth between 2013 and 2016. The rate of growth in the number of nonworking family members was similarly matched to the agency's forecasts of growth in the nonworking population.

18. Specifically, before-tax family cash income includes wage and salary earnings; pension or retirement income; income from self-employment, Temporary Assistance for Needy Families (TANF), Supplemental Security Income, Social Security, child support, unemployment compensation, workers' compensation, disability benefits, educational assistance, and financial assistance from outside the household; and other cash income.

19. See Congressional Budget Office, *The Budget and Economic Outlook: 2014 to 2024* (February 2014), www.cbo.gov/publication/45010.

20. In addition, the Affordable Care Act's requirement that many employers provide health insurance (or pay a penalty if they do not) will impose an additional cost on employers for some low-wage workers who do not currently have employment-based health insurance. CBO expects that the cost will ultimately be borne by workers through lower wages.

Estimating the Effects of Increases in the Minimum Wage on Family Income

The steps described above show how CBO formed an estimate of the distribution of hourly wages and family income in 2016 under current law. CBO then estimated how a higher minimum wage would affect family income in 2016. To do that, CBO first estimated the effect of an increase in the minimum wage on workers' annual earnings. CBO then projected how that change in earnings, along with several other factors, would change family income.

Changes in the Annual Earnings of Workers. CBO estimated the effect of increases in the minimum wage on the annual earnings of low-wage workers using methods similar to those used in its analysis of employment. The higher wages of two groups were multiplied by the workers' projected 2016 annual hours of work to estimate their annual earnings under the options. The first group consisted of workers who were projected to have wages lower than the new minimum in 2016 under current law. The second group consisted of workers whose projected wages in 2016 would be up to as much as \$11.50; as in its analysis of the number of affected workers, CBO estimated that wages would rise for people in that category, on average.

The wages of the first group were initially adjusted up to the new minimum, and then further adjustments for ripple effects were made in both groups. Specifically, those ripple effects were projected to extend up to the amount that would result from an increase that was 50 percent larger than the increase in their applicable federal or state minimum wage under either option. Ripple effects were included for workers whose wages under current law were projected to be slightly less and slightly more than the minimum wages under each option, respectively. The ripple effects were the largest for workers who, under current law, would have earned precisely the minimum wage that would be set under the option. On average, the ripple effects were substantially smaller than the increases in wages needed to bring workers up to the new minimum.

CBO's analysis of annual earnings also accounted for reductions in employment—and therefore in some workers' earnings—that would result from the increases in the minimum wage. Here, CBO used the same employment elasticities that it used in its analysis of the options' effects on employment. Employment reductions were restricted to workers who would have had, under current law, an

hourly wage less than the new minimum. Workers who would have had wages between the new minimum and \$11.50 were not considered to be at risk of losing employment as a result of the minimum-wage increase, as discussed above.

The reductions in employment would be concentrated more among teenage workers than among older workers, CBO expects, both because they tend to have lower wages and because their employment typically responds more sharply to changes in the minimum wage (as discussed above). Among workers at least 20 years old, CBO anticipates that the reductions in employment would be disproportionately concentrated among those who would have had the lowest wages under current law (apart from those to whom the minimum wage would not apply). Because many low-wage workers move in and out of employment within a year, CBO estimated the effects of the employment loss among low-wage workers by assuming that the affected people worked, on average, about half as many weeks as they otherwise would have; CBO therefore lowered projected earnings by 50 percent for twice as many workers as the projected number of people who would become jobless (rather than lowering earnings by 100 percent for a number of workers equal to the number of people who would become jobless).

Changes in the Annual Income of Families. An increase in the minimum wage would not only affect family income by changing workers' earnings. It would also result in losses in income for business owners, decreases in real income for many people because of increases in prices, and increases in some people's income generated by higher demand for goods and services. To determine the economywide effect on total income, CBO subtracted the output lost because of the decline in employment from the output gained because of the increase in the aggregate demand for goods and services. On balance, the total amount of real income in the economy would increase by \$2 billion in 2016 under the \$10.10 option, CBO projects, and by \$1 billion under the \$9.00 option.

In CBO's estimation, overall real income would increase for families with income less than six times the poverty threshold but would decrease for higher-income families, because both the income losses for business owners and the increase in prices would have the greatest effects on those higher-income families. In CBO's estimation, about 1 percent of the reduction in real income from those two factors would fall on people living in families

whose income was below the poverty threshold, whereas about 70 percent would fall on people living in families whose income was more than six times the poverty threshold.

CBO used those estimates of the change in income for families to project how many families would move into and out of poverty.²¹ Following the official definition of poverty, CBO did not consider the effects of a minimum-wage increase on taxes, tax credits, or noncash transfer payments in its calculations. (CBO has not analyzed the effects of minimum-wage increases on a measure of income that accounts for taxes, tax credits, or noncash transfers.) Some of those effects would partly offset the gain to families from a higher minimum wage. For example, workers who received higher wages because of an increase in the minimum wage would pay more payroll taxes (though they would later be eligible for more Social Security benefits), and some of their families would be eligible to receive less in noncash means-tested benefits, such as those provided by SNAP. The amount of the EITC received by workers in poor families would increase in some cases and decrease in others, depending on each worker's earnings and family income.

Uncertainty in the Estimates

There is considerable uncertainty about the effects of minimum-wage increases on family income. Some of the sources of uncertainty are the same as those in CBO's analysis of employment; they involve wage growth, the elasticity of employment with respect to the change in the minimum wage, and the magnitude of the macroeconomic response that would result from the redistribution of income. However, there are some additional sources of uncertainty in the analysis of the options' effects on family income. They include the following:

- The effect on total income and on the income of families with different amounts of income is uncertain because of various factors, including how much spending varies by family income, the extent to which people avoid sharp changes in consumption when their income changes, the relative magnitudes of

21. The Census Bureau's poverty thresholds, which identify the income level below which families are classified as being in poverty, were projected to grow at the same rate that CBO forecast for growth in the consumer price index for urban consumers, or CPI-U. That approach is consistent with the fact that poverty thresholds are updated annually for inflation with the CPI-U.

profit reductions and price increases by firms paying increased wages, and the magnitude of indirect effects on demand.

- It is uncertain how the reduction in employment resulting from a minimum-wage increase would be distributed among families during 2016. In its analysis, CBO distributed that employment reduction among families on the basis of the age and the wages under current law of the workers who live in those families. Alternative distributions would produce different effects on family income and poverty.
- The effect of a higher minimum wage on the behavior of other people who live in low-wage workers' families is uncertain. For example, someone in that situation might work fewer hours in response to a spouse's higher earnings—or more hours, if the spouse lost employment as a result of the higher minimum wage. In general, such responses would probably offset to some extent the effects of the options on low-wage workers' family income.

Comparing CBO's Approach With Other Approaches

CBO's estimates of the effect of increasing the minimum wage on family income are based on a "simulation" approach.²² That is, CBO estimated what the distribution of family income was likely to be in 2016 under current law and then projected how a higher minimum wage would alter that distribution by projecting

22. Also, CBO's analysis of income focuses on *family* income, in part because that is how official poverty measures are determined. Some analysts, however, have focused on *households* as the unit over which income is shared. CBO expects that the results using that alternative measure would yield qualitatively similar results, in this instance.

wages and employment (and then earnings and family income). CBO then projected the effect on the poverty rate by comparing each family's poverty status under current law with its poverty status under the two options.

An alternative approach to forecasting the effect of a minimum-wage increase on poverty rates is to estimate the historical correlation between the poverty rate and the minimum wage and to use that correlation to project a change in the poverty rate for a given change in the minimum wage. Some of the estimates produced by studies taking that approach would imply that the \$10.10 policy would reduce poverty by more than CBO has estimated. (See Appendix B for examples of such studies.)

There are several reasons that the two approaches may yield different results. It might be, for example, that CBO's analysis underestimates the increase in income that would accrue to poor families if the minimum wage was increased. That underestimate might occur if the minimum wage raised earnings for workers projected to have wages above the new minimum by more than CBO has estimated. It might also be that an increase in the minimum wage would alter family structure—through increased marriage rates, for example—in ways that reduced the number of families whose income was below the poverty threshold; such effects would be captured in the historical correlation approach but not in CBO's simulation approach. Alternatively, the effect on poverty of a minimum-wage increase might vary over time—for example, if the number of low-wage workers in families with income near the poverty threshold varied over time. If that was true, the correlation analysis might be less informative than CBO's simulation method, which uses more current data.



Appendix B: Research About the Effects of Minimum-Wage Increases

To develop its estimates of the effects of minimum-wage increases on employment and family income, the Congressional Budget Office (CBO) drew on the following research.

Reviews of Research About Employment Effects

For studies that analyze the central tendency of other studies' estimates of employment effects, accounting for journals' tendency to publish studies that find significant effects, see Dale Belman and Paul Wolfson, "Does Employment Respond to the Minimum Wage? A Meta-Analysis of Recent Studies From the New Minimum Wage Research," in *What Does the Minimum Wage Do?* (Upjohn Institute, forthcoming), <http://tinyurl.com/p475ahg> (PDF, 224 KB);

Hristos Doucouliagos and T. D. Stanley, "Publication Selection Bias in Minimum-Wage Research? A Meta-Regression Analysis," *British Journal of Industrial Relations*, vol. 47, no. 2 (June 2009), pp. 406–428, <http://dx.doi.org/10.1111/j.1467-8543.2009.00723.x>; and

David Card and Alan B. Krueger, "Time-Series Minimum-Wage Studies: A Meta-Analysis," *American Economic Review: Papers and Proceedings*, vol. 85, no. 2 (May 1995), pp. 238–243, www.jstor.org/stable/2117925.

For reviews that examine the methods and data used in the research literature that estimates employment effects of the minimum wage, see Sylvia Allegretto and others, *Credible Research Designs for Minimum Wage Studies*,

Discussion Paper 7638 (Institute for the Study of Labor, September 2013), <http://tinyurl.com/ld9rwmg>; and

David Neumark and William L. Wascher, "Minimum Wages and Employment," *Foundations and Trends in Microeconomics*, vol. 3, no. 1–2 (March 2007), pp. 1–182, <http://tinyurl.com/o7cngec>.

For a review of the literature on the effect of Britain's minimum wage (which was introduced in 1999), see Low Pay Commission, *National Minimum Wage*, Report 2013 (April 2013), Chapter 2, pp. 19–74, <http://tinyurl.com/m6bbe93>.

For a review of the literature on mechanisms that might explain small employment effects, see John Schmitt, *Why Does the Minimum Wage Have No Discernible Effect on Employment?* (Center for Economic and Policy Research, February 2013), <http://tinyurl.com/b54lk8m>.

For a literature review that covers a variety of effects, including the effects found in other countries, see David Neumark and William L. Wascher, *Minimum Wages* (MIT Press, 2008), <http://mitpress.mit.edu/books/minimum-wages>.

For reviews of that book, see Arindrajit Dube, "Minimum Wages. By David Neumark and William L. Wascher," *Journal of Economic Literature*, vol. 49, no. 3 (September 2011), <http://dx.doi.org/10.1257/jel.49.3.719.r18>; and

Richard V. Burkhauser, "Minimum Wages. By David Neumark and William L. Wascher," *Industrial and Labor Relations Review*, vol. 64, no. 1 (September 2010), pp. 202–203, <http://tinyurl.com/o3gy5bg>.

For a review of the research literature before 1999, see Charles Brown, “Minimum Wages, Employment, and the Distribution of Income,” in Orley C. Ashenfelter and David Card, eds., *Handbook of Labor Economics*, vol. 3, part B (Elsevier, 1999), pp. 2101–2163, <http://tinyurl.com/mmkdrme>.

For an early review of the literature from an international perspective, see Organisation for Economic Co-operation and Development, “Making the Most of the Minimum: Statutory Minimum Wages, Employment and Poverty,” in *OECD Employment Outlook 1998—Towards an Employment-Centred Social Policy* (OECD Directorate for Labour and Social Affairs, June 1998), Chapter 2, <http://tinyurl.com/q6rs9a2>.

For an early review of the new minimum-wage research from the first half of the 1990s, see David Card and Alan B. Krueger, *Myth and Measurement: The New Economics of the Minimum Wage* (Princeton University Press, 1995), <http://press.princeton.edu/titles/5632.html>.

For a very early review of the literature, see Charles Brown, Curtis Gilroy, and Andrew Kohen, “The Effect of the Minimum Wage on Employment and Unemployment,” *Journal of Economic Literature*, vol. 20, no. 2 (June 1982), pp. 487–528, www.jstor.org/stable/2724487.

Other Research About Employment Effects

CBO also considered the following studies, which are generally too recent to have been covered by the reviews listed above.

For studies focused on employment among teenagers, see Laura Giuliano, “Minimum Wage Effects on Employment, Substitution, and the Teenage Labor Supply: Evidence From Personnel Data,” *Journal of Labor Economics*, vol. 31, no. 1 (January 2013), pp. 155–194, <http://dx.doi.org/10.1086/666921>;

Yusuf Soner Baskaya and Yona Rubinstein, “Using Federal Minimum Wages to Identify the Impact of Minimum Wages on Employment and Earnings Across the U.S. States” (draft, Department of Economics Workshop, University of Chicago, December 2012), <http://tinyurl.com/lmjohgl> (PDF, 580 KB); and

Sylvia A. Allegretto, Arindrajit Dube, and Michael Reich, “Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data,” *Industrial Relations*, vol. 50, no. 2 (April 2011), pp. 205–240, <http://dx.doi.org/10.1111/j.1468-232X.2011.00634.x>.

For a recent study that focuses on how the effects of minimum-wage increases vary with economic conditions, see John T. Addison, McKinley L. Blackburn, and Chad D. Cotti, “Minimum Wage Increases in a Recessionary Environment,” *Labour Economics*, vol. 23 (August 2013), pp. 30–39, <http://dx.doi.org/10.1016/j.labeco.2013.02.004>.

For analyses of changes in employment in industries where low wages are prevalent, see William E. Even and David A. Macpherson, “The Effect of the Tipped Minimum Wage on Employees in the U.S. Restaurant Industry,” *Southern Economic Journal*, vol. 80, no. 3 (January 2014), pp. 633–655, <http://tinyurl.com/kv6fz6c>;

Jonathan Meer and Jeremy West, *Effects of the Minimum Wage on Employment Dynamics* (draft, Texas A&M University, December 2013), <http://tinyurl.com/cllro5p> (PDF, 2.9 MB).

Arindrajit Dube, *Minimum Wages and Aggregate Job Growth: Causal Effect or Statistical Artifact?* Discussion Paper 7674 (Institute for the Study of Labor, October 2013), <http://tinyurl.com/kx6t2yz>;

David Neumark, J. M. Ian Salas, and William Wascher, *Revisiting the Minimum Wage-Employment Debate: Throwing Out the Baby With the Bathwater?* Working Paper 18681 (National Bureau of Economic Research, January 2013), www.nber.org/papers/w18681;

John T. Addison, McKinley L. Blackburn, and Chad D. Cotti, “The Effect of Minimum Wages on Labour Market Outcomes: County-Level Estimates from the Restaurant-and-Bar Sector,” *British Journal of Industrial Relations*, vol. 50, no. 3 (September 2012), pp. 412–435, <http://tinyurl.com/ot9apya>; and

Arindrajit Dube, T. William Lester, and Michael Reich, “Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties,” *Review of Economics and Statistics*, vol. 92, no. 4 (November 2010), pp. 945–964, http://dx.doi.org/10.1162/rest_a_00039.

For recent studies that examine changes in employment among a variety of groups that earn low wages, on average, see Arindrajit Dube, T. William Lester, and Michael Reich, *Minimum Wage Shocks, Employment Flows and Labor Market Frictions*, Working Paper 149-13 (Institute for Research on Labor and Employment, June 2013), www.irlle.berkeley.edu/workingpapers/149-13.pdf (3.4 MB); and

Joseph J. Sabia, Richard V. Burkhauser, and Benjamin Hansen, “Are the Effects of Minimum Wage Increases Always Small? New Evidence From a Case Study of New York State,” *Industrial and Labor Relations Review*, vol. 65, no. 2 (April 2012), pp. 350–376, <http://tinyurl.com/mn566b3>.

For examples of earlier studies about effects on adults with low wages, see David Neumark, Mark Schweitzer, and William Wascher, “Minimum Wage Effects Throughout the Wage Distribution,” *Journal of Human Resources*, vol. 39, no. 2 (Spring 2004), pp. 425–450, <http://tinyurl.com/ncgswlg>;

David Neumark, “The Employment Effects of Minimum Wages: Evidence From a Prespecified Research Design,” *Industrial Relations*, vol. 40, no. 1 (January 2001), pp. 121–144, <http://dx.doi.org/10.1111/0019-8676.00199>;

Richard V. Burkhauser, Kenneth A. Couch, and David C. Wittenburg, “Who Minimum Wage Increases Bite: An Analysis Using Monthly Data From the SIPP and the CPS,” *Southern Economic Journal*, vol. 67, no. 1 (July 2000), pp. 16–40, www.jstor.org/stable/1061611; and

Donald Deere, Kevin M. Murphy, and Finis Welch, “Employment and the 1990–1991 Minimum-Wage Hike,” *American Economic Review*, vol. 85, no. 2 (May 1995), pp. 232–237, www.jstor.org/stable/2117924.

For examples of research on the long-term effects of changes in minimum wages, see Isaac Sorkin, “Are There Long-Run Effects of the Minimum Wage?” (draft, University of Michigan, October 2013), <https://sites.google.com/site/isaacsorkin/papers>;

Dale L. Belman and Paul Wolfson, “The Effect of Legislated Minimum Wage Increases on Employment and Hours: A Dynamic Analysis,” *Labour*, vol. 24, no. 1 (March 2010), pp. 1–25, <http://tinyurl.com/nhp7mth>; and

Michael Baker, Dwayne Benjamin, and Shuchita Stanger, “The Highs and Lows of the Minimum Wage Effect: A Time-Series Cross-Section Study of the Canadian Law,” *Journal of Labor Economics*, vol. 17, no. 2 (April 1999), pp. 318–350, <http://dx.doi.org/10.1086/209923>.

For a reexamination of earlier research using time series methods, see Nicolas Williams and Jeffrey A. Mills, “The Minimum Wage and Teenage Employment: Evidence From Time Series,” *Applied Economics*, vol. 33, no. 3 (February 2001), pp. 285–300, <http://dx.doi.org/10.1080/00036840122088>.

Research About Family Income Effects

For analysis of the effects of minimum-wage changes on family income and the poverty rate, see Joseph J. Sabia and Robert B. Nielsen, “Minimum Wages, Poverty, and Material Hardship: New Evidence From the SIPP,” *Review of Economics of the Household* (January 2013), <http://dx.doi.org/10.1007/s11150-012-9171-8>;

Arindrajit Dube, “Minimum Wages and the Distribution of Family Incomes” (draft, University of Massachusetts, Amherst, December 2013), <http://tinyurl.com/muab8nc> (PDF, 1.52 MB);

David Neumark and William Wascher, “Does a Higher Minimum Wage Enhance the Effectiveness of the Earned Income Tax Credit?” *Industrial and Labor Relations Review*, vol. 64, no. 4 (July 2011), pp. 712–746, <http://tinyurl.com/looy95w>;

Joseph J. Sabia and Richard V. Burkhauser, “Minimum Wages and Poverty: Will a \$9.50 Federal Minimum Wage Really Help the Working Poor?” *Southern Economic Journal*, vol. 76, no. 3 (January 2010), pp. 592–623, <http://dx.doi.org/10.4284/sej.2010.76.3.592>;

Joseph J. Sabia, “Minimum Wages and the Economic Well-Being of Single Mothers,” *Journal of Policy Analysis and Management*, vol. 27, no. 4 (Autumn 2008), pp. 848–866, <http://dx.doi.org/10.1002/pam.20379>;

Robert H. DeFina, “The Impact of State Minimum Wages on Child Poverty in Female-Headed Families,” *Journal of Poverty*, vol. 12, no. 2 (October 2008), pp. 155–174, <http://dx.doi.org/10.1080/10875540801973542>;

Richard V. Burkhauser and Joseph J. Sabia, "The Effectiveness of Minimum-Wage Increases in Reducing Poverty: Past, Present, and Future," *Contemporary Economic Policy*, vol. 25, no. 2 (April 2007), pp. 262–281, <http://dx.doi.org/10.1111/j.1465-7287.2006.00045.x>;

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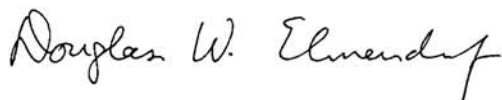
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About This Document

This Congressional Budget Office (CBO) report was prepared in response to interest expressed by a number of Members of Congress. In keeping with CBO's mandate to provide objective, impartial analysis, the report contains no recommendations.

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Director

February 2014



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

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February 2013

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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 report¹ and six additional volumes of related research papers.² In their independent summary of the research reviewed in the MWSC, Brown, Gilroy, and Kohen, three economists involved in producing the report, distinguished between employment effects on: teenagers (ages 16-19), where they concluded that a 10 percent increase in the minimum wage reduced teen employment, most plausibly, from between zero and 1.5 percent; young adults (ages 20-24), where they believed the employment impact is “negative and smaller than that for teenagers”; and adults, where the “direction of the effect...is uncertain in the empirical work as it is in the theory.”^{3,4} Their summary of the theoretical and empirical research through the late 1970s suggested that any “disemployment” effects of the minimum wage were small and almost exclusively limited to teenagers and possibly other younger workers.

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

1 Minimum Wage Study Commission (1981)

2 For an overview of the workings of MWSC and a review of its main findings, see Eccles and Freeman (1982). For a lengthy review of the MWSC's finding, prepared by three economists involved in preparation of the MWSC report, see Brown, Gilroy, and Kohen (1982).

3 Brown, Gilroy, and Kohen (1982), p. 524.

4 The employment impact on adults is uncertain in theory because an increase in the minimum wage might encourage employers to replace some (presumably lower productivity) teenagers with more (presumably higher productivity) adults.

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

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

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

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

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

5 Other important studies along these lines include Card's (1992a) analysis of the impact of the 1988 increase in California's state minimum wage and Katz and Krueger's (1992) study of the impact of the 1990 and 1991 increases in the federal minimum wage.

6 Card and Krueger (1994), p. 792.

7 Economists David Neumark and William Wascher (2000) criticized Card and Krueger's study, arguing that the survey was poorly designed and implemented. Card and Krueger (2000) responded by confirming their original results using payroll records from a virtual census of fast-food restaurants in New Jersey and eastern Pennsylvania.

8 Card (1992b), p. 36.

9 Card and Krueger (1995), pp. 389-390.

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

Since the early 2000s

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

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

Meta-studies

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

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

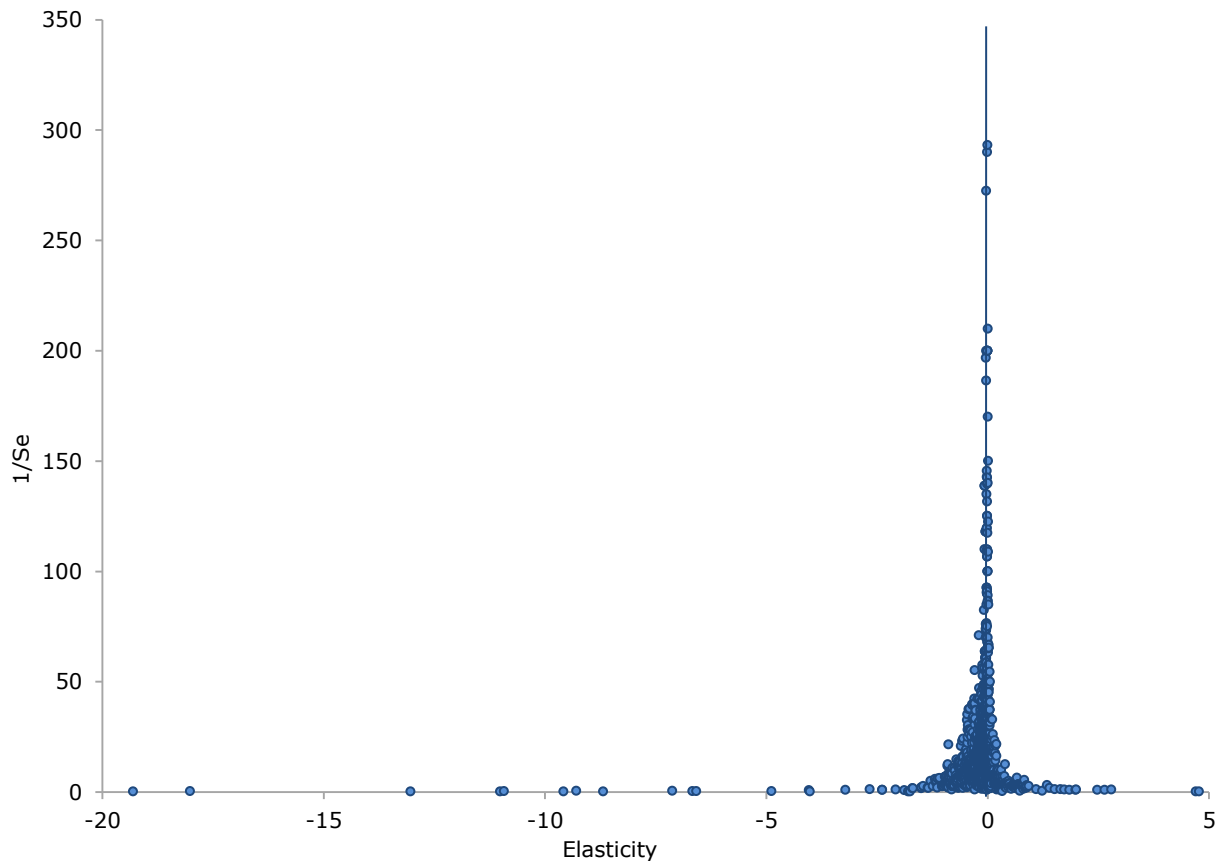
10 Neumark and Wascher (2008), p. 104.

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

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

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

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



Source: Doucouliagos and Stanley (2009).

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

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

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

Reviews

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

"What may be most striking to the reader who has managed to wade through our lengthy review of the new minimum wage research is the wide range of estimates of the effects of the minimum wage on employment, especially when compared to the review of the earlier literature by Brown et al. in 1982 [for the Minimum Wage Study Commission]. For example, few of the studies in the Brown et al. survey were outside of the consensus range of -0.1 to -0.3 for the elasticity of teenage employment with respect to the minimum wage. In contrast, even limiting the sample of studies to those focused on the effects of the minimum wage of teenagers in the United States, the range of studies comprising the new minimum wage research extends from well below -1 to well above zero."¹⁶

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

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

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

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

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

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

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

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

18 Neumark and Wascher (2006).

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

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

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

A closer look at several key recent studies

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

Dube, Lester, and Reich (2010)

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

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

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

21 The paper first circulated in 2007.

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

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

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

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

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

23 Dube, Lester, and Reich (2010), p. 961.

24 Dube, Lester, and Reich (2010), p. 962.

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

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

Allegretto, Dube, and Reich (2011)

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

Allegretto, Dube, and Reich analyzed data on teenagers taken from the Current Population Survey (CPS) for the years 1990 through 2009.²⁸ Because the CPS sample is smaller than the QCEW data used in the county-analysis, Allegretto, Dube, and Reich instead tracked teen employment at the state level. When they produced standard statistical analyses of the kind used in much of the research since the mid-1990s on teen employment, the three economists found results similar to those found in that earlier research (a 10 percent increase in the minimum wage reduces teen employment slightly more than 1 percent). But, once they controlled for different regional trends, the estimated employment effects of the minimum wage disappeared, turning slightly positive, but not statistically significantly different from zero.

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

Hirsch, Kaufman, and Zelenska (2011)

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

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

26 Addison, Blackburn, and Cotti (2012), p. 412. This research first circulated in 2008, at about the same time that Dube, Lester, and Reich's work first appeared.

27 Of course, Dube, Lester, and Reich (2010) included data covering the 1990-91 and 2001 recessions.

28 The detailed data on restaurant employment that Dube, Lester, and Reich (2010) used in their study do not contain information on workers' characteristics such as age, so Allegretto, Dube, and Reich (2011) used the smaller CPS data set.

29 Allegretto, Dube, and Reich (2011), p. 238.

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

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

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

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

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

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

Sabia, Burkhauser, and Hansen (2012)

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

31 Hirsch, Kaufman, and Zelenska (2011), p. 32.

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

33 Hirsch, Kaufman, and Zelenska (2011), p. 33.

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

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

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

Adjustment Channels

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

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

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

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

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

Competitive model

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

Institutional model

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

This institutional approach to the labor market allows for several additional channels of adjustment to a minimum-wage increase. Probably the most important of these concern productivity. Employers may respond to a minimum-wage increase by exerting greater managerial effort on productivity-enhancing activities, including the reorganization of work, setting higher performance standards, or demanding greater work intensity. In the competitive model, firms are assumed already to be operating at peak efficiency, but in the institutional framework, firms are assumed to often operate below their peak efficiency because it is costly to managers and to workers to identify, implement, and maintain practices that continuously maximize efficiency.³⁸ In this context, a minimum-wage increase gives new incentives to employers to undertake additional productivity-improving practices. Alternatively, a higher minimum wage may also boost productivity through "efficiency wage" effects. A strong theoretical and empirical basis exists for the idea that wages set above the competitive market rate can induce workers to work harder,³⁹ either to ensure that they keep their job⁴⁰ or in reciprocity for the higher wages paid.^{41, 42}

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

37 Hirsch, Kaufman, and Zelenska (2011), p. 5. For an excellent discussion of the institutional framework as it relates to the minimum wage, see Kaufmann (2010).

38 Kaufman (1999, 2010).

39 Katz (1986).

40 Shapiro and Stiglitz (1984).

41 Akerlof (1982).

42 See Hirsch, Kaufman, and Zelenska (2011), pp. 5-7 for additional possible channels of adjustment under the institutional model.

43 See Hall and Cooper (2012).

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

Dynamic monopsony model

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

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

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

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

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 increase 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 Kaestner'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.

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:

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

⁷⁵ See, for example, DiNardo, Fortin, and Lemieux (1996), and Autor, Manning, 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

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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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 \$11 per hour in 2015 and to \$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 \$11/hour in April 2015 and to \$13/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 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 in our analysis. 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 focusing on 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 assume that the impact of a minimum wage policy on wages is equal to the statutory increase in the 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 conventionally calculated elasticities 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 estimates around -1. The subsequent increase to as much as \$13 yielded more substantial disemployment effects, with net elasticity 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/hour than the rise from \$9.47 to \$11/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

¹Because 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 -2.8 to 0.3.

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. Because of limitations of our data, smaller single-site employers are over-represented in our sample, and these businesses may react differently than larger multi-site employers – though survey evidence, discussed below, indicates that multi-site employers were if anything more likely to report staffing reductions in the wake of the minimum wage increase. 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.

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 previously reported longitudinal analysis of experienced workers suggests that their earnings have held steady or slightly increased over the time period examined here (The Seattle Minimum Wage Study Team, 2016). A pattern of gains for experienced workers coupled with losses for new entrants would be consistent with qualitative evidence indicating employers' focus on hiring employees who do not require on-the-job training. In future work we anticipate studying effect heterogeneity in detail by linking administrative payroll data to other administrative data with more socioeconomic and demographic information on individual workers.

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, the possibility that higher wages intensify job search and thus improve employee-employer match quality, 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. 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?

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 then-new minimum wage ordinance. The authors argue that fast-food restaurants

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).”

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, Lester and Reich, 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, 1996, 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 analysis below is 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.

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.

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 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 bias employment elasticity estimations to be more negative than they are in reality.

To account for this issue, researchers have argued for a variety of different 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; Allegetto, 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

A final strand of estimation has used linear factor estimation and interactive fixed effects. 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

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.

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 \$13.00 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 most 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 who 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.

4. Data

We study the impact of the 2015 and 2016 minimum wage hikes 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 Washington is one of four states in the US that collects not only data on earnings, but also on hours worked during the quarter. 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

This unique dataset allows us to measure the average wage paid to each worker in each quarter.¹¹ We measure hourly wage rate as total quarterly earnings divided by quarterly hours worked, which corresponds to average hourly earnings, or realized hourly wage rate. As such, we can identify jobs that would appear to be affected by an increase in the minimum wage, and track

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.

⁹ Most papers 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 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.

trends in both employment counts and calculated average hourly wages.¹² As a result, 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. We can estimate effects solely for low-wage jobs within all industries.

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.¹³ We therefore exclude multi-site single-account businesses from the analysis, referring henceforth to the remaining firms as “single-site” businesses. As shown in Table 2, in Washington State as a whole, single-site businesses comprise 89% of firms and employ 62% of the entire workforce (which includes 2.7 million employees in an average quarter).

Multi-location firms may respond differently to local minimum wage laws. On the one hand, firms with establishments inside and outside of the affected jurisdiction could more easily absorb the added labor costs from their affected locations, and thus would have less incentive to respond by changing their labor demand. On the other hand, such firms would have an easier time relocating work to their existing sites outside of the affected jurisdiction, and thus might reduce labor demand more than single-location businesses. Survey evidence collected in Seattle at the time of the first minimum wage increase, and again one year later, increase suggests that

¹² 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.

¹³ 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 PUMAs within Washington State.

multi-location firms were in fact more likely to plan and implement staff reductions.¹⁴ Our employment results may therefore be biased towards zero.

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.

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).¹⁵

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.¹⁶ 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.

¹⁴ The Seattle Minimum Wage Study surveyed 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).

¹⁵ Specifically, we examine employment and wages in the 3-digit NAICS code 722 “Food and Drinking Places”.

¹⁶ 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.

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 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 – that is, the point where this reaction function strikes the 45-degree line. Directly estimating a reaction function would require a longitudinal analysis of wages paid before and after a wage increase, complicated in this application by the high turnover rates common in the low-wage job market.¹⁷ We instead present results of a repeat-cross-section analogue in Figure 1. This exercise estimates a series of difference-in-differences and synthetic control models to estimate the impact of Seattle’s minimum wage increases on the number of hours worked in jobs with average wages in bins of width \$1, up to the \$39-40/hour level.¹⁸ We expect minimum wage increases to result in negative estimates at the lowest wage rates, positive estimates at slightly

¹⁷ Longitudinal analysis of ESD data reveals that more than one-third of low-wage employees observed in one calendar quarter are no longer employed *anywhere* in Washington state six quarters later.

¹⁸ This exercise strongly resembles the “bunching” analysis presented in Cengiz et al. (2017).

higher wage rates, and negligible estimates at the highest wage rates. The empirical question is where, exactly, the transitions lie.

The upper panels of Figure 1 show large declines in the hours worked in jobs paying less than \$11/hour by the 4th quarter of 2015 relative to the baseline 2nd quarter of 2014, when the Ordinance was passed. This result is to be expected given the intervening minimum wage increase from \$9.47 to \$11/hour. Both difference-in-differences and synthetic control methods show evidence of an increase in jobs paying between \$11 and \$12 per hour. Across these two specifications, there is not consistent evidence of systematic increases in the number of hours worked in jobs paid between \$12 and any other threshold.¹⁹ The synthetic control estimates, which, as we explain below, we consider more reliable, suggest some increase in work paid at wage rates up to roughly \$18/hour. Above that level, point estimates are generally small, with the majority unable to reject the hypothesis of no effect even at the highly conservative 50% confidence level.²⁰

The lower panels repeat this analysis examining transitions from the baseline quarter to the 3rd quarter of 2016, at which time the minimum wage had reached as high as \$13/hour. Predictably, the data show marked drops in the number of hours worked for wages under \$13. The preferred synthetic control estimates show remarkably little evidence of significant increases in hours worked at wages above that level.²¹

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

¹⁹ This pattern exemplifies the “bunching” referenced in Cengiz et al. (2017), and can be interpreted as evidence that significant numbers of lower-paid workers had their wages increased to comply with the law.

²⁰ The 50% confidence level is employed here as the goal is to assess the hypothesis that the true effect is zero, rather than the traditional alternate hypothesis that the true effect is not zero.

²¹ Again referencing Cengiz et al. (2017), and foreshadowing results to be presented, this pattern suggests that the second minimum wage increase led to a proportionately stronger reduction in employment opportunities.

promoted to higher wage levels, in our preferred specifications we report findings based on a relatively conservative \$19 threshold. The \$19 threshold is roughly twice the initial value of the minimum wage, a level beyond which cascading effects are less likely to occur (Neumark, Schwizer, and Wascher, 2004).²²

5.2 Causal identification strategy

We estimate 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 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. The minimum wage reached as high as \$11/hour in the fourth through sixth quarters after baseline and as high as \$13/hour in the remaining quarters. The “pre-treatment” period includes quarterly observations beginning in 2005.

Though we are interested in the cumulative effect of the minimum wage, we analyze variation in year-over-year changes in each outcome. This approach differences out seasonal fluctuations, and conforms to a standard time-series approach used in the prior literature. We define the year-over-year change in outcome Y as follows:

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

where r denotes region (e.g. Seattle or comparison region), and t denotes quarter (with t ranging from -33 to 9, and $t = 0$ corresponding to the quarter during which the Ordinance was passed).

²² 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.

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 (abbreviated to SKP), which surround King County but do not share a border with Seattle (see Figure 2). 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.²³

In both cases, we estimate the following difference-in-differences specification:

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

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.

²³ Our companion paper (Jardim et al., 2017) examines this possibility of spillover and mechanisms for estimating spillovers in greater detail.

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, $\beta_q^{cum} = (1 + \beta_q)(1 + \beta_{q-4}) - 1$; 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 clustering the 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.

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 composition of K unobserved linear factors μ_{tk} :

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

where μ_{tk} is an unobserved factor, common across all regions in each year-quarter, and λ_{rk} is a region-specific factor loading, constant across time.

The unobserved factors can be thought of as common economic shocks which affect all regions at the same time, such as an exchange rate shock, common demand shock, or changes in weather. However, regions are allowed to have different sensitivity in response to these shocks. As a result, 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 $r = 2, \dots, R$ all potential control regions. Then the weights for synthetic control can be found by minimizing forecasting error in the pre-policy period:

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

subject to the constraints $\sum_r w_r = 1$ and $\forall r w_r \geq 0$.²⁴ Given a set of weights \widehat{w}_r , the impact of the Ordinance in quarter q is estimated as follows:

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

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).²⁵ 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.

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.²⁶ We exclude King County PUMAs from analysis because of potential spillover effects. The remainder of Washington includes 40 PUMAs (see Figure 3).

Appendix Table 2 shows the estimated weights chosen by the synthetic control estimator by outcome and lists PUMAs with positive weights. Appendix Figures 1-4 show the sensitivity of the interactive fixed effects estimates as a function of the number of factors used, as well as

²⁴ We implement synthetic control estimator using the R programs provided by Gobillon and Magnac (2016).

²⁵ The coefficients, β_q , can be identified as 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 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.

²⁶ 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.

showing the choice of the optimal number of factors using the criterion developed in Bai and Ng (2002).²⁷

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.²⁸ As in Gobillon and Magnac (2016), we implement 10,000 draws to obtain the standard errors. We then take the standard deviation of these estimated placebo impacts to be our estimate of the standard error.²⁹

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 ($t = 1, 2, \text{ or } 3$, July 2014-March 2015), and

²⁷ 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.

²⁸ Note that Seattle spans 5 PUMAs, thus our placebo treatment region replicates Seattle’s size.

²⁹ We have also estimated the standard errors based on “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 an estimate of the standard error. Standard errors using “placebo in space” approach prove to be more conservative (i.e., larger) than the standard errors using “placebo in time” approach, so we report “placebo in space” standard errors in our baseline estimates.

the first six quarters after the law was in force ($t = 4, 5, 6, 7, 8, \text{ or } 9$, 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%).³⁰ The decline is consistent with 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.³¹

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). 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 reduction at wages under \$19. Though it would be premature to make causal inferences on the basis of this single-differenced data, both headcount and hours data suggest that reduced low-wage employment can be apportioned primarily, but less than entirely, to wage increases.

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. The impression of skewed growth – driven in

³⁰ 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.

³¹ 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.

part by rapid growth in the technology sector – extends to wage data.³² 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).³³

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. 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 30%, 23%, and 11% of jobs in Seattle’s single-location establishments 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.

In contrast to overall low-wage labor market, low-wage employment in the restaurant industry increased slightly (by 0.1% in terms of hours) between second quarter of 2014 and the second quarter of 2016. At all wages, industry employment expanded by 12.0%, only slightly more slowly than the labor market as a whole. As in the full economy, growth in hours at jobs paying above \$19 per hour exceeded growth in lower-paying jobs. Relative to the full labor market, wage increases in the restaurant sector are distributed more evenly across the initial wage distribution.

³² 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.

³³ 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.

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 more generally 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 (June, 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 June 2012, as shown in columns 2 and 4. In both columns, all of the estimated pseudo-effects 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. 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 Causal effect estimates

Table 5 presents our first estimates of the causal impact of the Ordinance for workers earning less than \$19 per hour. Looking at results using both the synthetic control and interactive fixed effects methods, 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 a \$0.44 per hour relative to the base average wage of \$14.14.³⁴ 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

³⁴ 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.

phase-in and 6.0% during the second. These estimates suggest that wages increased in the control region as well.³⁵ 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. If we were to presume that our estimate reflects some sizable impact on jobs directly impacted by the increase and no effect on other jobs under \$19, the sizable impact works out to 7.9%, a level in line with existing literature.³⁶ 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 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

³⁵ 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.

³⁶ 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.

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 single-location firms.³⁷ Scaled up linearly to account for multi-location firms, job losses would amount to roughly 10,000.³⁸

Figure 4 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 estimated impacts become insignificant once the threshold rises to around \$17. It appears that any “loss” in hours at lower thresholds is likely to reflect 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 5 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 is maintained as we increase the threshold to \$25 per hour.

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

³⁷ 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.

³⁸ We cannot ascertain whether the effect on single-location firms should extrapolate to multi-location 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.

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 single-location Seattle businesses by roughly \$120 million on an annual basis.³⁹

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.⁴⁰

6.4 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 estimate of the effect on hours to our estimate of the effect on wages, using the synthetic control method, for the six quarters after the Ordinance was enforced.⁴¹ We also compute measures of statistical uncertainty for these elasticities since

³⁹ 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/hour, equate to a loss of \$132 million – and a net loss of \$116 million – on an annual basis.

⁴⁰ 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.

⁴¹ 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

they reflect the ratio of two estimates.⁴² 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 6, 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 2016.1, 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 6, the estimated elasticities are very close to -3 when the threshold is set anywhere between \$17 and \$25 per hour.⁴³

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.

⁴² Standard errors for the estimates elasticities have been computed using the delta method and take into account the correlation between estimated effect of the minimum wage on employment and wages.

⁴³ 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.

The larger elasticities in the second phase-in period relative to the first suggest that the sum total of 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.

6.5 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 1).⁴⁴ 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

⁴⁴ 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.

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 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 comparable to those shown in Column 1 for all industries – if anything, the results show larger decreases in hours, particularly when the minimum wage was raised to \$11, suggesting roughly a 7% decline, although two of these three estimates are insignificant. 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 workers in Food and Drinking Places is coming from a reduction in jobs rather than a reduction in hours worked by those who have such jobs.

In Column 4, we shift from examining low-wage jobs to all jobs in the restaurant industry. Here we see a *dramatic change*: the effect on all jobs is insignificant in all quarters and averages precisely 0.0% in the last three quarters when the wage increased to \$13 per hour. 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. Finally, column 5 returns to evaluating effects on total hours, but now for all

jobs in NAICS 722. While the estimates continue to be insignificant, they are now more negative, averaging -3.3% in the last three quarters. This result is consistent with Neumark and Wascher's (2000) critique of Card and Krueger (1994).

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 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 attempted to analyze 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 \$19.⁴⁵ A 3.1% increase in wages in jobs that paid less than \$19 coupled with a 9.4% 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 from 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

⁴⁵ 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.

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 substantially 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 is restricted to 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 single-site firms is larger than that of multi-site firms who do not report employment at specific locations. Yet, as discussed above, multi-site firms who we surveyed were more likely to self-report cuts in employment than smaller firms.⁴⁶

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

⁴⁶ 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).

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).

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

Effective Date	Large Employers ^a		Small Employers	
	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%
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 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.

Table 3: Employment Statistics for Seattle Single-Site Establishments

Quarter	Quarters After Passage/ Enforcement	<u>Number of Jobs</u>			<u>Total Hours (thousands)</u>			<u>Average Wage</u>			<u>Total Payroll (\$mlns.)</u>		
		Hourly wage rates:			Hourly wage rates:			Hourly wage rates:			Hourly wage rates:		
		Under \$13	Under \$19	All	Under \$13	Under \$19	All	Under \$13	Under \$19	All	Under \$13	Under \$19	All
<i>Panel A: All Industries</i>													
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
<i>Panel B: Food and Drinking Places (NAICS 722)</i>													
2014.2	0	11,980	21,800	32,648	4,310	8,198	11,938	10.99	13.10	17.77	47	107	212
2014.3	1	12,114	22,614	34,356	4,382	8,685	12,787	10.98	13.21	18.00	48	115	230
2014.4	2	10,997	22,392	34,811	3,749	8,276	12,514	11.10	13.48	18.76	41	112	235
2015.1	3	10,896	22,530	34,893	3,523	7,912	12,006	11.13	13.55	18.91	39	107	227
2015.2	4/1	10,123	22,228	35,072	3,534	8,380	12,758	11.42	13.77	18.74	40	115	239
2015.3	5/2	9,451	22,749	36,577	3,339	8,806	13,668	11.54	14.01	19.13	39	123	261
2015.4	6/3	8,464	22,672	37,177	2,830	8,561	13,577	11.60	14.26	19.83	33	122	269
2016.1	7/4	6,422	21,679	36,120	1,935	7,635	12,373	11.86	14.61	20.33	23	112	252
2016.2	8/5	6,728	21,556	36,618	2,213	8,209	13,368	11.96	14.63	19.99	26	120	267
2016.3	9/6	6,480	21,647	37,283	2,212	8,780	14,440	11.89	14.70	20.21	26	129	292

Note: Data derived from administrative employment records obtained from the Washington Employment Security Department. Multi-site employers excluded.

Table 4: Falsification Test: Pseudo-Effect of Placebo Law Passed 2012

Quarter	Quarters after (pseudo) Passage/ Enforcement	Difference-in-Differences between Seattle and:				Synthetic Control		Interactive Fixed Effects	
		Outlying King County		Snohomish, Kitsap, and Pierce Counties		Washington excluding King County		Washington excluding King County	
		Wage	Hours	Wage	Hours	Wage	Hours	Wage	Hours
2012.3	1	0.001* (0.001)	-0.044*** (0.004)	-0.003** (0.002)	-0.014*** (0.006)	0.001 (0.003)	-0.014 (0.015)	-0.002 (0.003)	-0.012 (0.013)
2012.4	2	-0.002*** (0.001)	-0.033*** (0.004)	-0.003* (0.002)	-0.038*** (0.006)	0.001 (0.003)	-0.018 (0.021)	-0.001 (0.003)	-0.022 (0.014)
2013.1	3	0.002*** (0.001)	-0.034*** (0.004)	0.001 (0.002)	-0.028*** (0.006)	0.001 (0.003)	-0.002 (0.020)	0.000 (0.003)	-0.017 (0.038)
2013.2	4/1	0.003*** (0.001)	-0.022*** (0.004)	0.005*** (0.002)	-0.036*** (0.006)	0.001 (0.003)	0.004 (0.026)	0.001 (0.003)	-0.016 (0.038)
2013.3	5/2	0.003*** (0.001)	-0.063*** (0.007)	-0.002 (0.003)	-0.063*** (0.012)	0.004 (0.005)	-0.006 (0.022)	-0.002 (0.004)	-0.024 (0.041)
2013.4	6/3	0.003** (0.001)	-0.069*** (0.007)	-0.006* (0.003)	-0.095*** (0.012)	0.006 (0.004)	-0.009 (0.033)	0.000 (0.004)	-0.034 (0.049)
2014.1	7/4	0.003** (0.001)	-0.031*** (0.007)	0.001 (0.003)	-0.047*** (0.012)	0.005 (0.004)	0.028 (0.029)	-0.001 (0.004)	-0.008 (0.053)
2014.2	8/5	0.006*** (0.001)	-0.031*** (0.007)	0.004 (0.003)	-0.059*** (0.012)	0.008*** (0.004)	0.014 (0.031)	0.003 (0.004)	-0.024 (0.055)
2014.3	9/6	0.004** (0.002)	-0.046*** (0.011)	-0.001 (0.005)	-0.073*** (0.017)	0.010* (0.005)	0.013 (0.031)	0.000 (0.005)	-0.019 (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: Clustered standard errors in parentheses. 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 are assigned zero weight in the synthetic control results.

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

Table 5: Main Results: Effect on Wages

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. Clustered standard errors in parentheses. 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 are assigned zero weight in the synthetic control results.

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

Table 6: Main Results: Effect on 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: 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 are assigned zero weight in the synthetic control results.

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

Table 7: Main Results: Effect on Payroll

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. Clustered standard errors in parentheses. 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 are assigned zero weight in the synthetic control results.

***, **, and * denote statistical 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: 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.

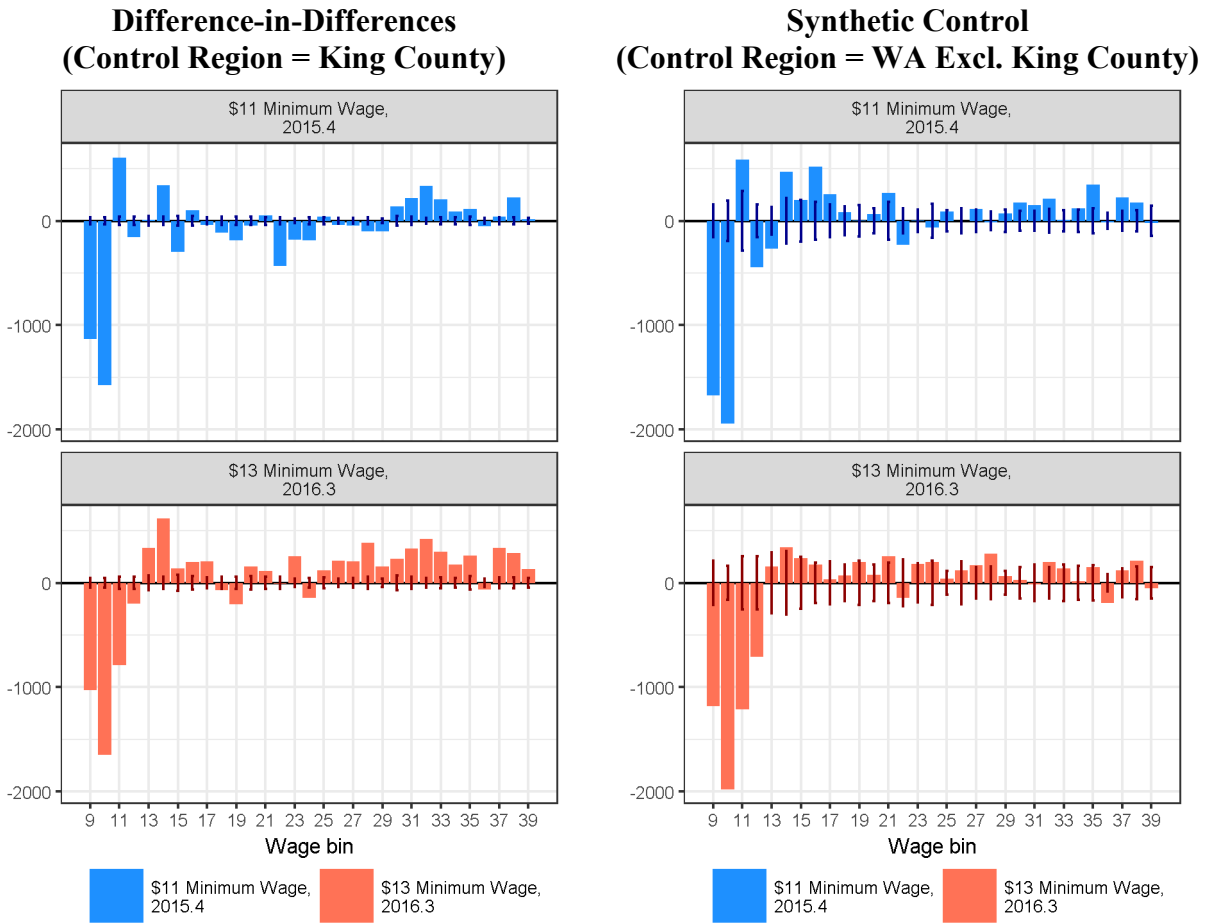
Table 9: Effect of Restricting Analysis to Food Service and Drinking Places

Quarter	Quarters since Passage/ Enforcement	All industries	Restaurant Industry (NAICS 722)			
		Wages under \$19 Hours	Wages under \$19		All wage levels	
			Hours	Jobs	Jobs	Hours
2014.3	1	0.008 (0.018)	-0.014 (0.031)	0.023 (0.031)	0.036 (0.027)	-0.004 (0.028)
2014.4	2	0.003 (0.018)	0.023 (0.033)	-0.005 (0.036)	0.032 (0.033)	0.037 (0.031)
2015.1	3	-0.023 (0.018)	-0.035 (0.038)	0.008 (0.039)	0.019 (0.037)	-0.014 (0.036)
2015.2	4/1	-0.013 (0.019)	-0.065* (0.038)	-0.055 (0.035)	-0.010 (0.033)	-0.041 (0.036)
2015.3	5/2	-0.034 (0.025)	-0.071 (0.049)	-0.025 (0.046)	0.013 (0.043)	-0.042 (0.046)
2015.4	6/3	-0.021 (0.033)	-0.074 (0.050)	-0.087** (0.043)	-0.013 (0.046)	-0.018 (0.052)
2016.1	7/4	-0.106*** (0.031)	-0.120*** (0.047)	-0.117*** (0.05)	-0.010 (0.056)	-0.046 (0.053)
2016.2	8/5	-0.087*** (0.031)	-0.110** (0.055)	-0.133*** (0.053)	0.000 (0.063)	-0.031 (0.064)
2016.3	9/6	-0.102*** (0.042)	-0.099 (0.062)	-0.090 (0.056)	0.010 (0.069)	-0.022 (0.069)

Notes: n=1,890. Clustered standard errors in parentheses. 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 are assigned zero weight in the synthetic control results.

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

Figure 1: Finding a Reasonable Threshold – Effect on Quarterly Hours Worked (000s) Relative to Baseline Quarter (2014.2) for Those Paid Within Each Wage Bin



Notes: Point estimates (i.e., bars) and 50% confidence intervals centered around zero are shown.

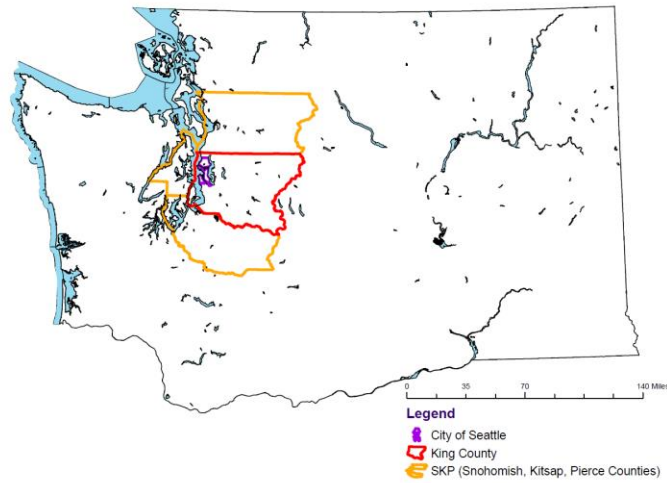
Figure 2: Geography of Seattle and King, Snohomish, Kitsap, and Pierce Counties

Panel A: Seattle's Water Boundaries

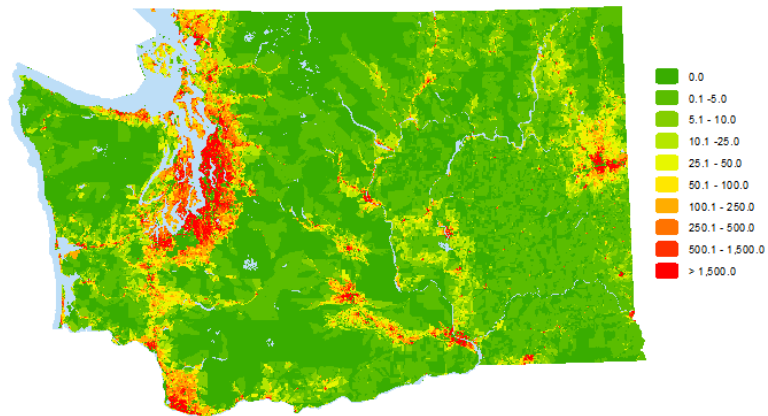


Source: <https://www.google.com/maps/>

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 3: Geography of Washington's PUMAs

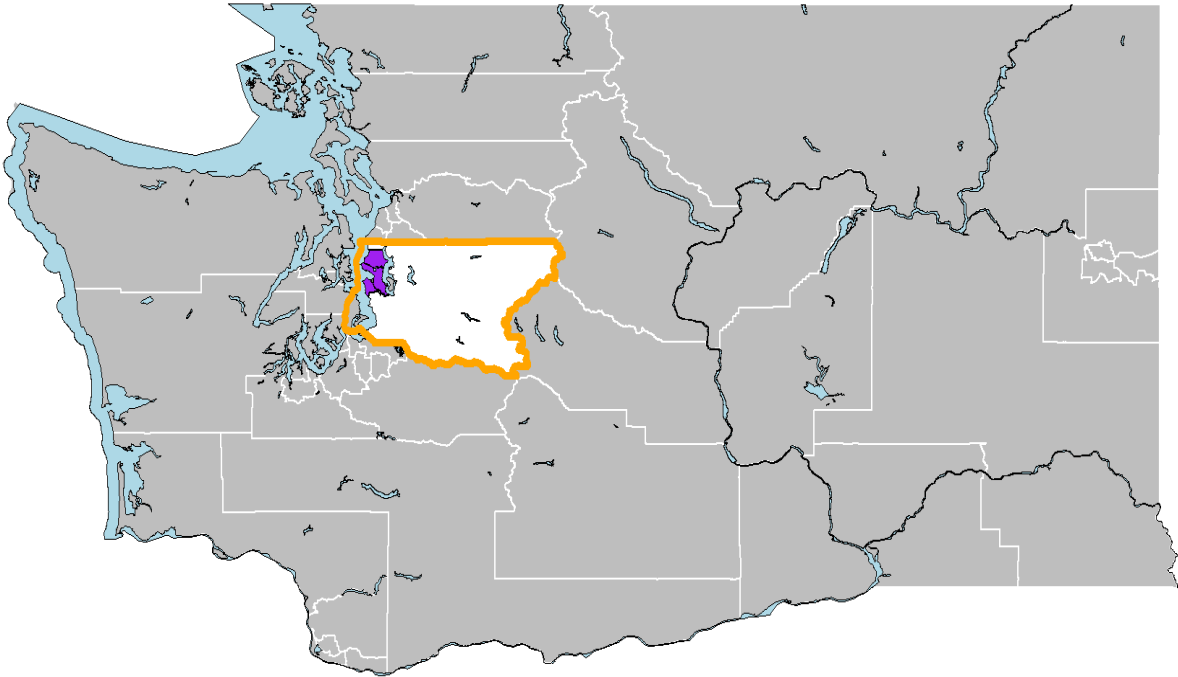
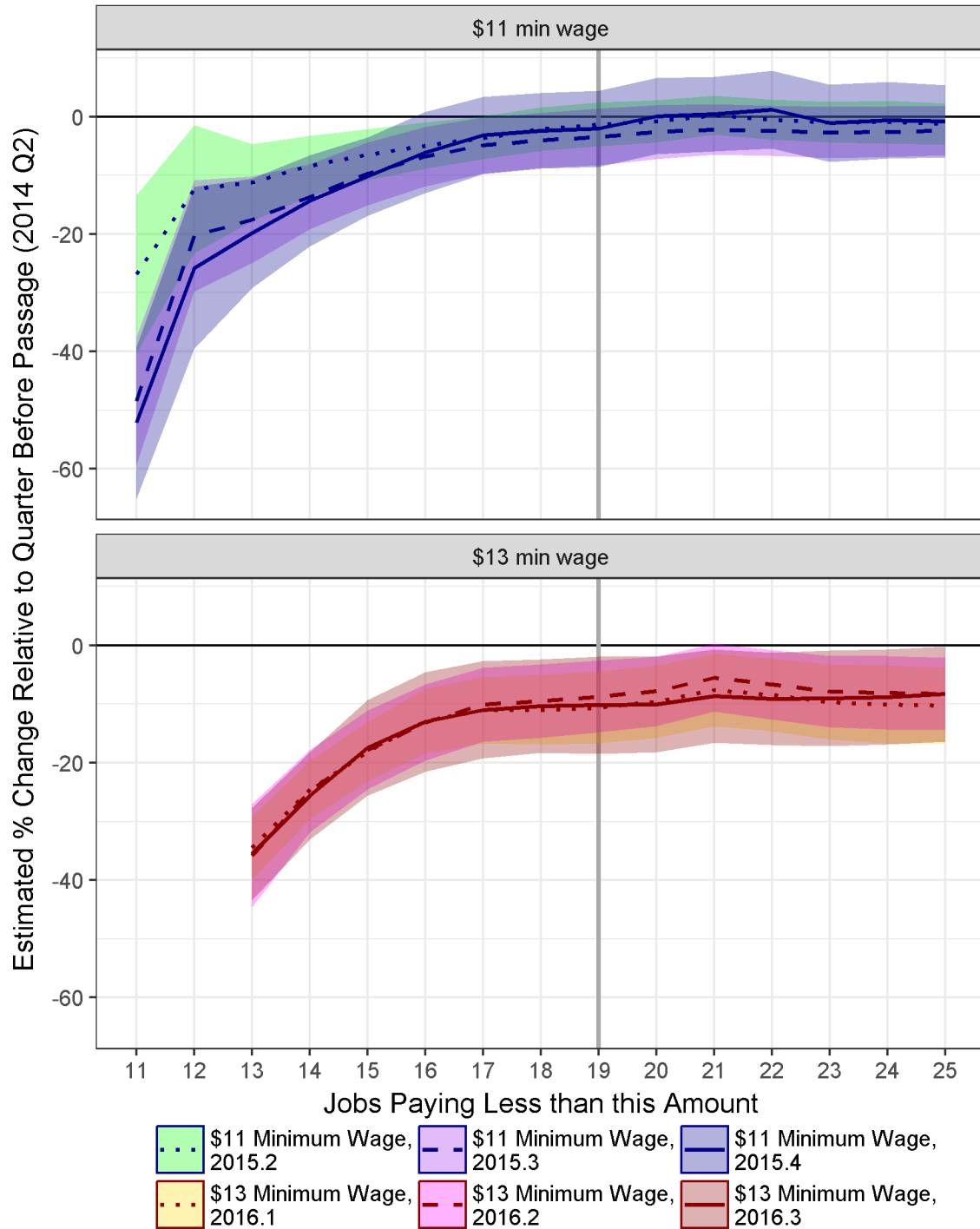
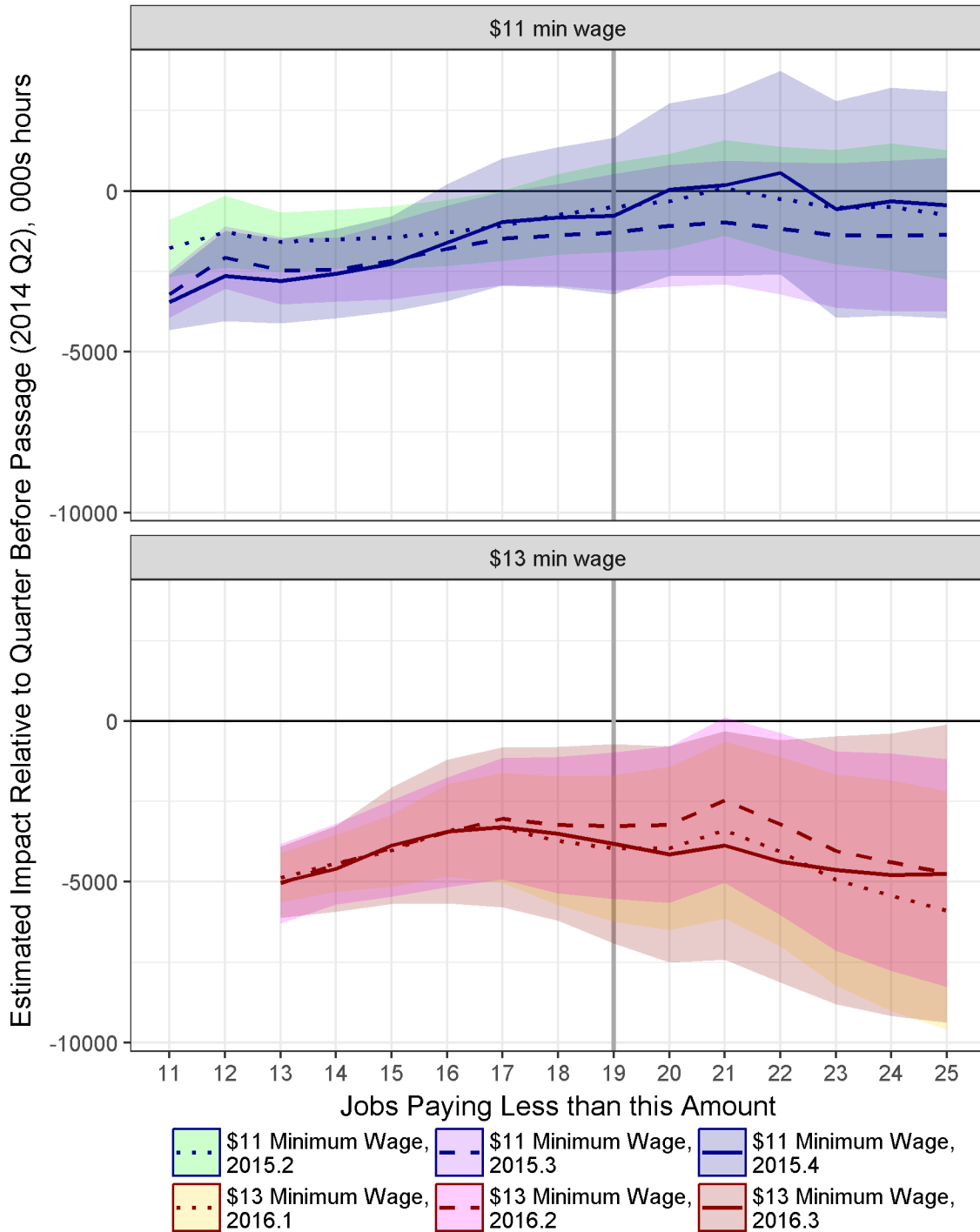


Figure 4: Sensitivity of the Estimated Effects on Percentage Change in Hours Worked Using Different Thresholds



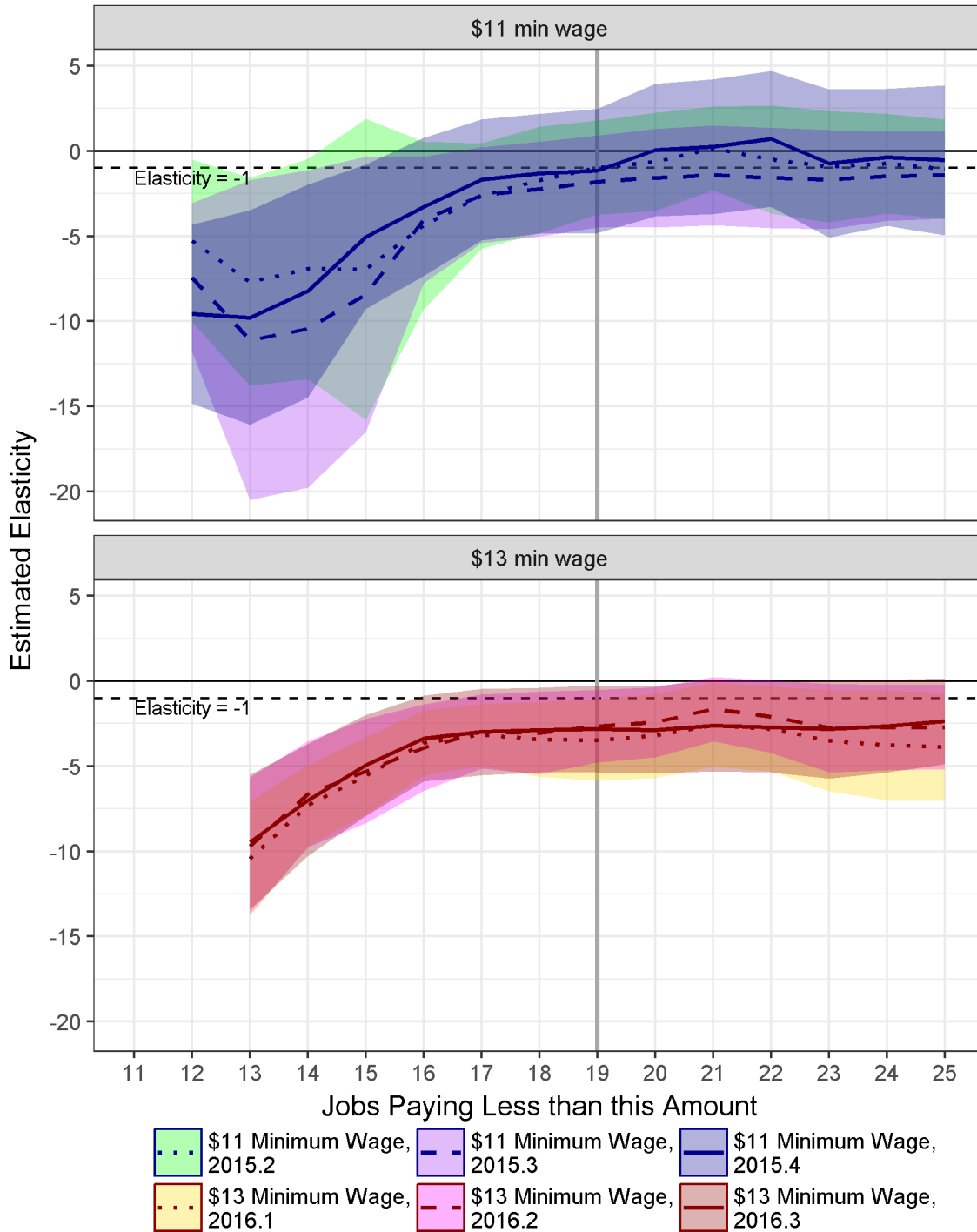
Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

Figure 5: 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 6: 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.

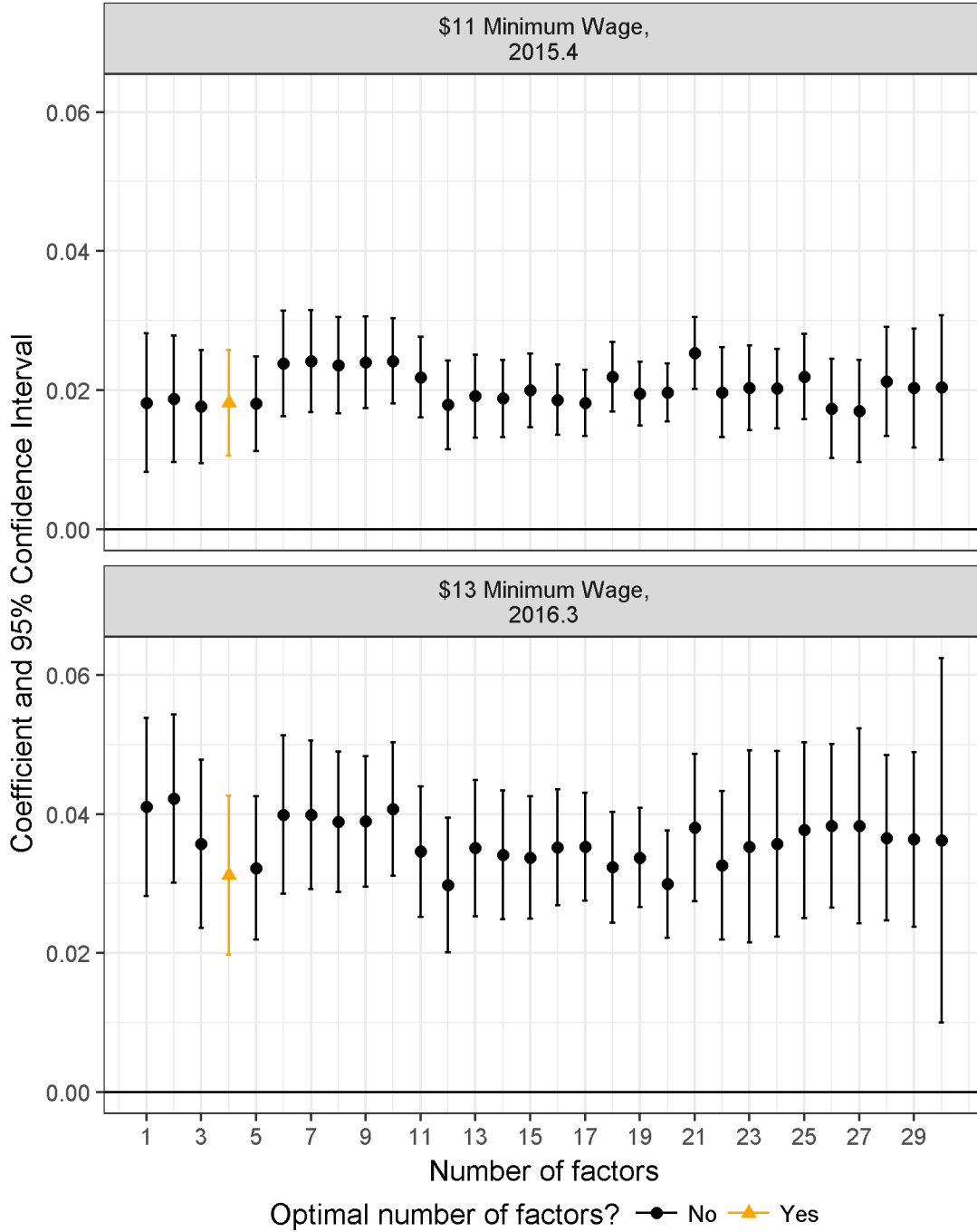
Appendix Table 1: Elasticity Estimates from Selected Literature

Paper	Level of Government	Industry and Outcome	Years	Method	Elasticity
Totty, 2015	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
NSW, 2014	State	Restaurant Employment All Jobs	2000- 2011	DnD (State and Time FE)	-0.12
				Synthetic Matching Estimator	-0.06
DLR, 2010	State	Restaurant Employment All Jobs	1990- 2006	DnD (Census division-by-period fixed effects and County FE) + State linear trend	-0.02 -0.04
				Contiguous Border County Pair Sample (County and Quarter FE)	-0.11
				Contiguous Border County Pair Sample (County-pair \times period FE)	0.02
DLR, 2016	State	Restaurant Employment All Jobs	2000- 2011	DnD (County and Quarter FE)	-0.07
				DnD (Contiguous County-Pair Quarter FE + County FE)	-0.02
ABC, 2014	State	Restaurant Employment All Jobs	1990- 2005	DnD (County and Quarter Fixed Effects) + Linear County Trends	-0.10 -0.01
				+ Quadratic County Trends	-0.05
				+ Cubic County Trends	-0.04
				+ Quartic County Trends	-0.06
				+ Fifth-order County Trends	-0.05
				DnD (County and Quarter FE) + Linear County Trends	0.00 -0.04
				1990- 2012	+ Quadratic County Trends
+ Cubic County Trends	-0.04				
+ Quartic County Trends	-0.02				
+ Fifth-order County Trends	-0.01				
ALDRZ, 2016	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 \times Quarter FE)	0.02
Unweighted Average					-0.05
Unweighted Standard Deviation					0.06

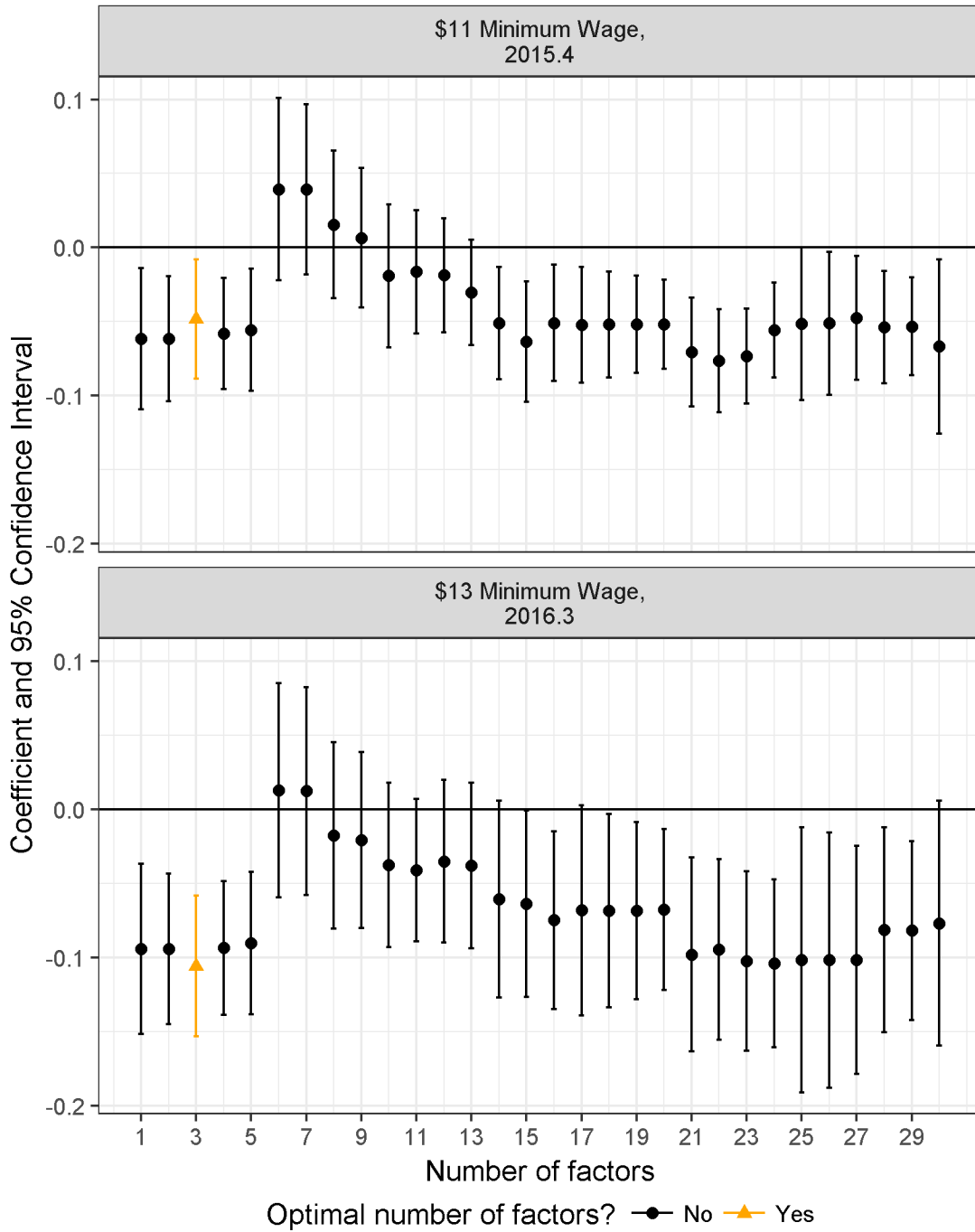
Appendix Table 2: PUMAs with positive weights chosen by Synthetic Control Estimator.

	PUMA ID	PUMA Name	Weight in Synthetic Control, %
A. Average Wages			
1	10503	Spokane County (East Central)--Greater Spokane Valley City PUMA	25.39
2	11702	Snohomish County (West Central)--Mukilteo & Everett (Southwest) Cities PUMA	19.29
3	11701	Snohomish County (Southwest)--Edmonds, Lynnwood & Mountlake Terrace Cities PUMA	15.22
4	11402	Thurston County (Outer) PUMA	10.08
5	10300	Chelan & Douglas Counties PUMA	9.86
6	10702	Benton County (East Central)--Kennewick & Richland (South) Cities PUMA	9.34
7	11502	Pierce County (Northwest)--Peninsula Region & Tacoma City (West) PUMA	5.30
8	11801	Kitsap County (North)--Bainbridge Island City & Silverdale PUMA	4.82
9	11505	Pierce County (North Central)--Tacoma (Port) & Bonney Lake (Northwest) Cities PUMA	0.69
B. Number of Jobs			
1	11401	Thurston County (Central)--Olympia, Lacey & Tumwater Cities PUMA	21.95
2	11706	Snohomish County (North)--Marysville & Arlington Cities PUMA	21.92
3	11101	Clark County (Southwest)--Vancouver City (West & Central) PUMA	13.35
4	11701	Snohomish County (Southwest)--Edmonds, Lynnwood & Mountlake Terrace Cities PUMA	11.81
5	11702	Snohomish County (West Central)--Mukilteo & Everett (Southwest) Cities PUMA	9.78
6	10100	Whatcom County--Bellingham City PUMA	6.45
7	10503	Spokane County (East Central)--Greater Spokane Valley City PUMA	6.26
8	11102	Clark County (West Central)--Salmon Creek & Hazel Dell PUMA	4.65
9	11704	Snohomish County (South Central)--Bothell (North), Mill Creek Cities & Silver Firs PUMA	2.31
10	11402	Thurston County (Outer) PUMA	0.91
11	10701	Benton & Franklin Counties--Pasco, Richland (North) & West Richland Cities PUMA	0.61
C. Quarterly Hours Worked			
1	11706	Snohomish County (North)--Marysville & Arlington Cities PUMA	23.55
2	11401	Thurston County (Central)--Olympia, Lacey & Tumwater Cities PUMA	23.10
3	11402	Thurston County (Outer) PUMA	14.86
4	11701	Snohomish County (Southwest)--Edmonds, Lynnwood & Mountlake Terrace Cities PUMA	10.75
5	11101	Clark County (Southwest)--Vancouver City (West & Central) PUMA	9.66
6	11702	Snohomish County (West Central)--Mukilteo & Everett (Southwest) Cities PUMA	7.00
7	11102	Clark County (West Central)--Salmon Creek & Hazel Dell PUMA	6.08
8	10503	Spokane County (East Central)--Greater Spokane Valley City PUMA	3.23
9	10800	Grant & Kittitas Counties PUMA	1.78
D. Quarterly Payroll			
1	11401	Thurston County (Central)--Olympia, Lacey & Tumwater Cities PUMA	20.46
2	11706	Snohomish County (North)--Marysville & Arlington Cities PUMA	16.05
3	11101	Clark County (Southwest)--Vancouver City (West & Central) PUMA	12.95
4	11402	Thurston County (Outer) PUMA	12.94
5	11102	Clark County (West Central)--Salmon Creek & Hazel Dell PUMA	12.39
6	11702	Snohomish County (West Central)--Mukilteo & Everett (Southwest) Cities PUMA	8.22
7	11701	Snohomish County (Southwest)--Edmonds, Lynnwood & Mountlake Terrace Cities PUMA	7.32
8	10702	Benton County (East Central)--Kennewick & Richland (South) Cities PUMA	3.66
9	10503	Spokane County (East Central)--Greater Spokane Valley City PUMA	3.21
10	10800	Grant & Kittitas Counties PUMA	2.81

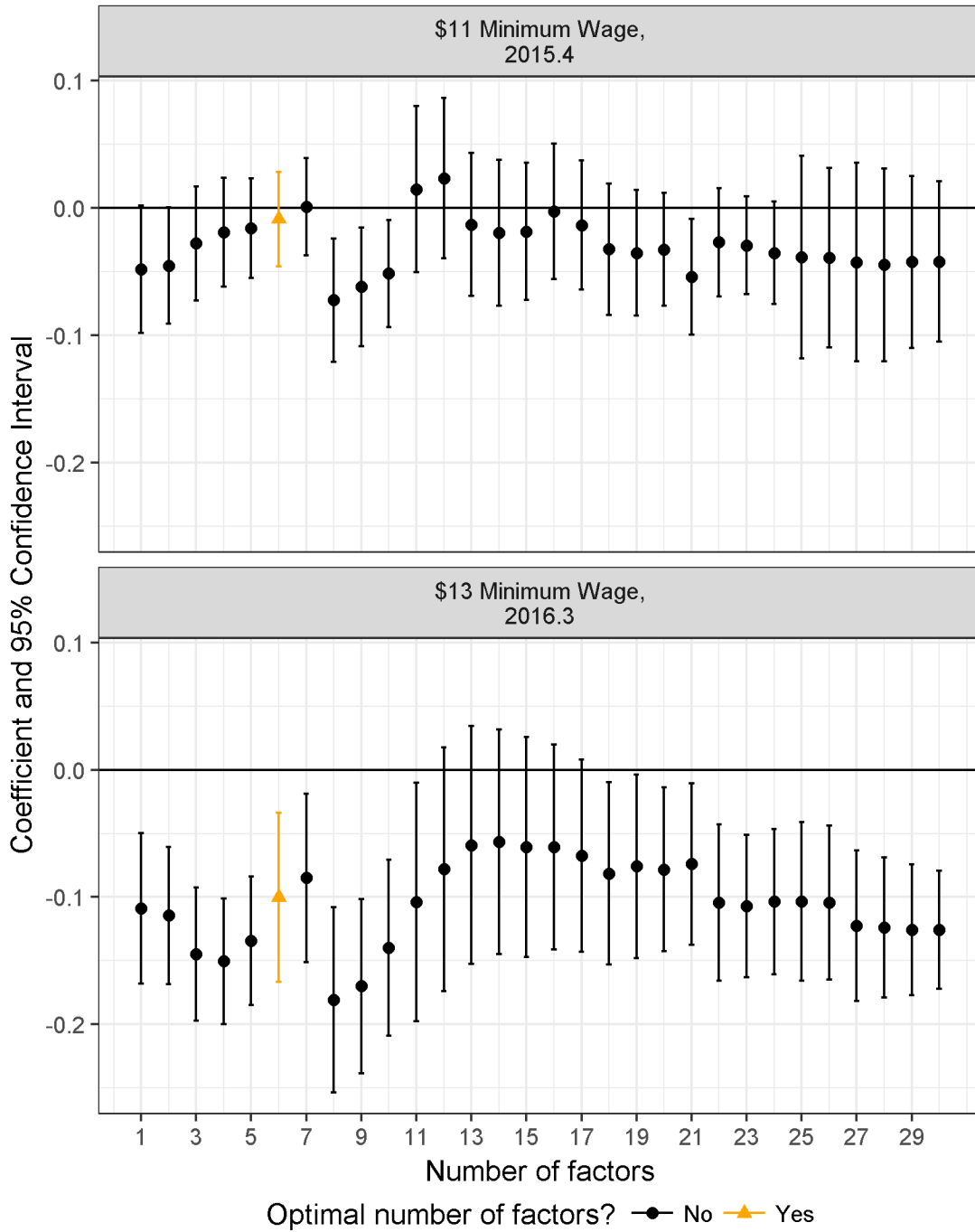
Appendx Figure 1: Estimated impact of the minimum wage on the average wages in jobs paying <\$19 per hour, all industries.



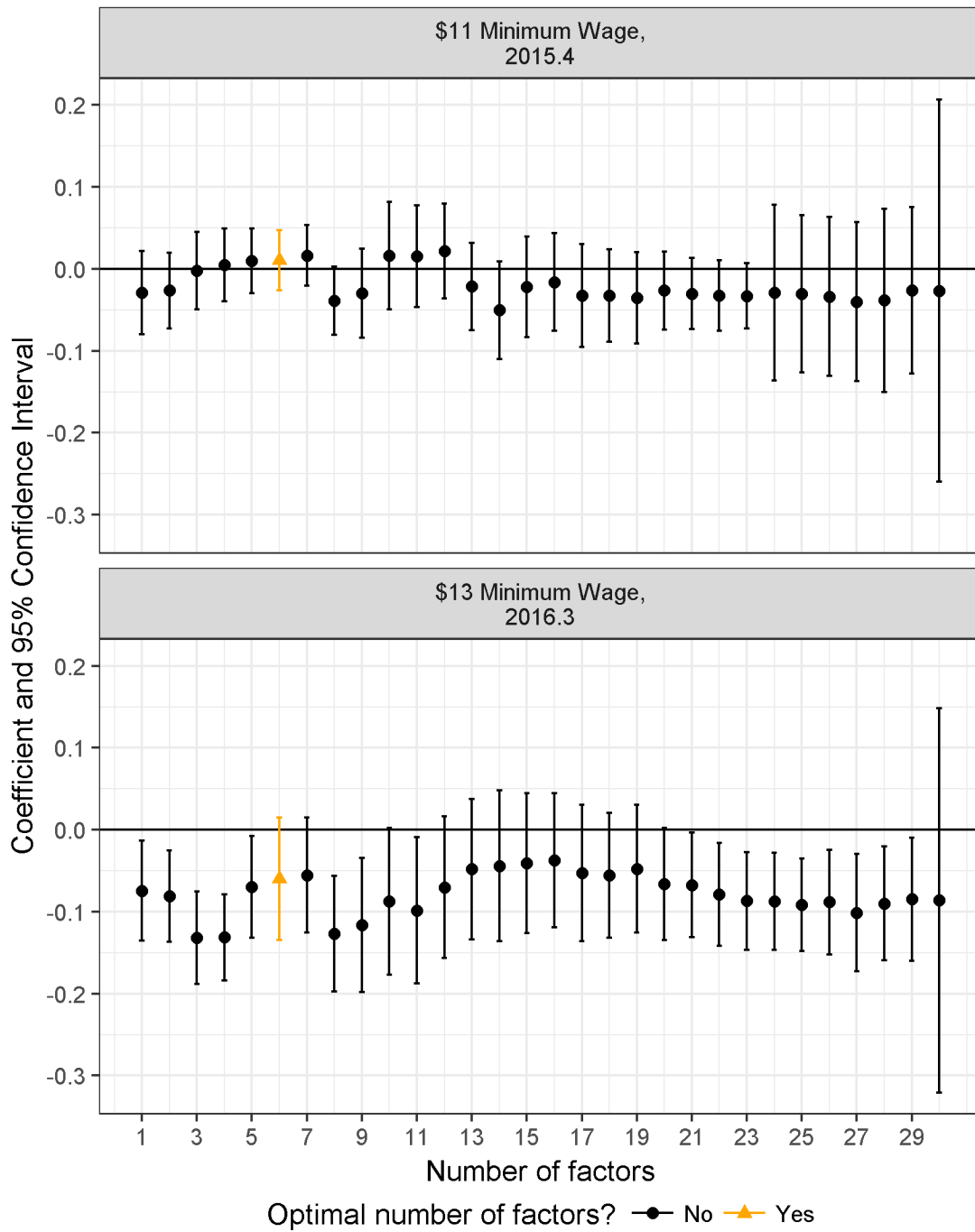
Appendix Figure 2: Estimated impact of the minimum wage on the number of jobs paying <\$19 per hour, all industries.



Appendix Figure 3: Estimated impact of the minimum wage on the quarterly hours worked in jobs paying <\$19 per hour, all industries.



Appendix Figure 4: Estimated impact of the minimum wage on the quarterly payroll to jobs paying <\$19 per hour, all industries.





Center on Wage and Employment Dynamics

Appendix 8

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CWED Policy Brief

Institute for Research on Labor and Employment

University of California, Berkeley

Seattle's Minimum Wage Experience 2015-16

By Michael Reich, Sylvia Allegretto, and Anna Godoey

June 2017

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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

Date	Large firms (500+)		Small firms (500 or fewer)	
	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.

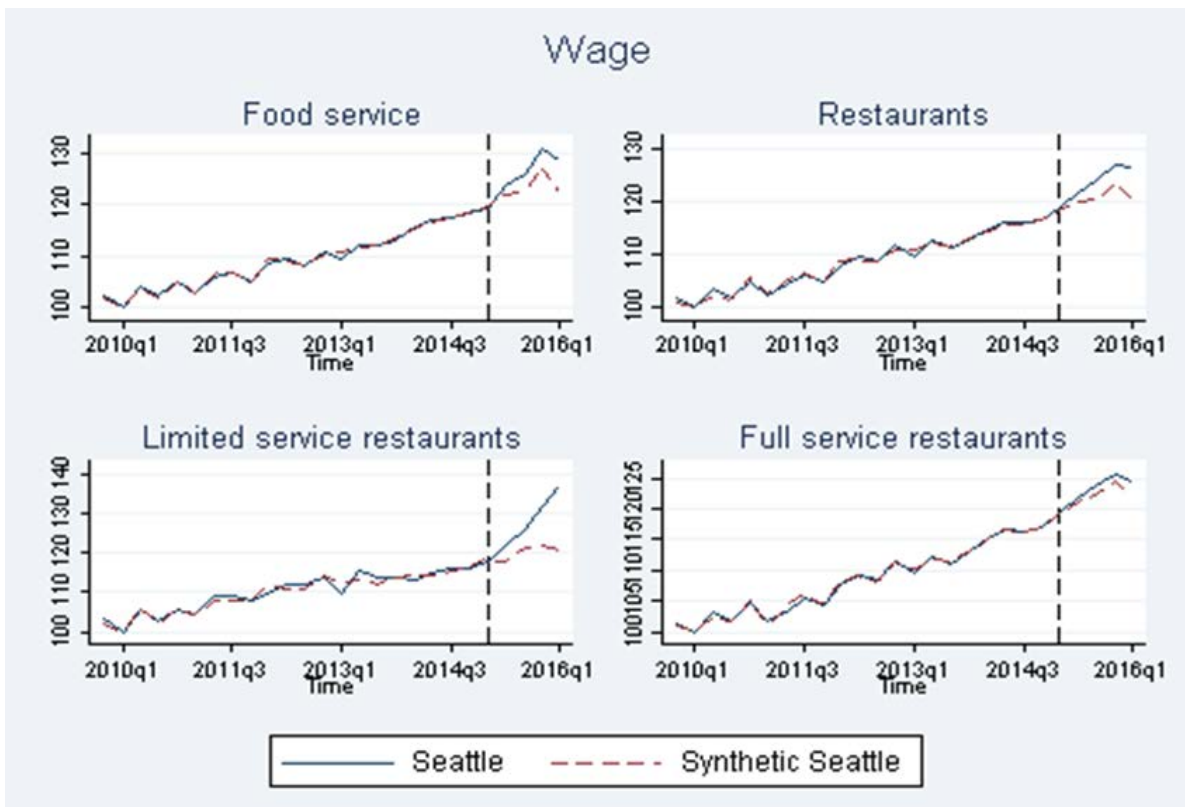
As the figure shows, wages in Synthetic Seattle track wages in Seattle remarkably well, and over the entire pre-treatment period.⁷ This finding indicates that our application of the synthetic control method strongly passes the parallel trends requirement. These results thereby satisfy the first of the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

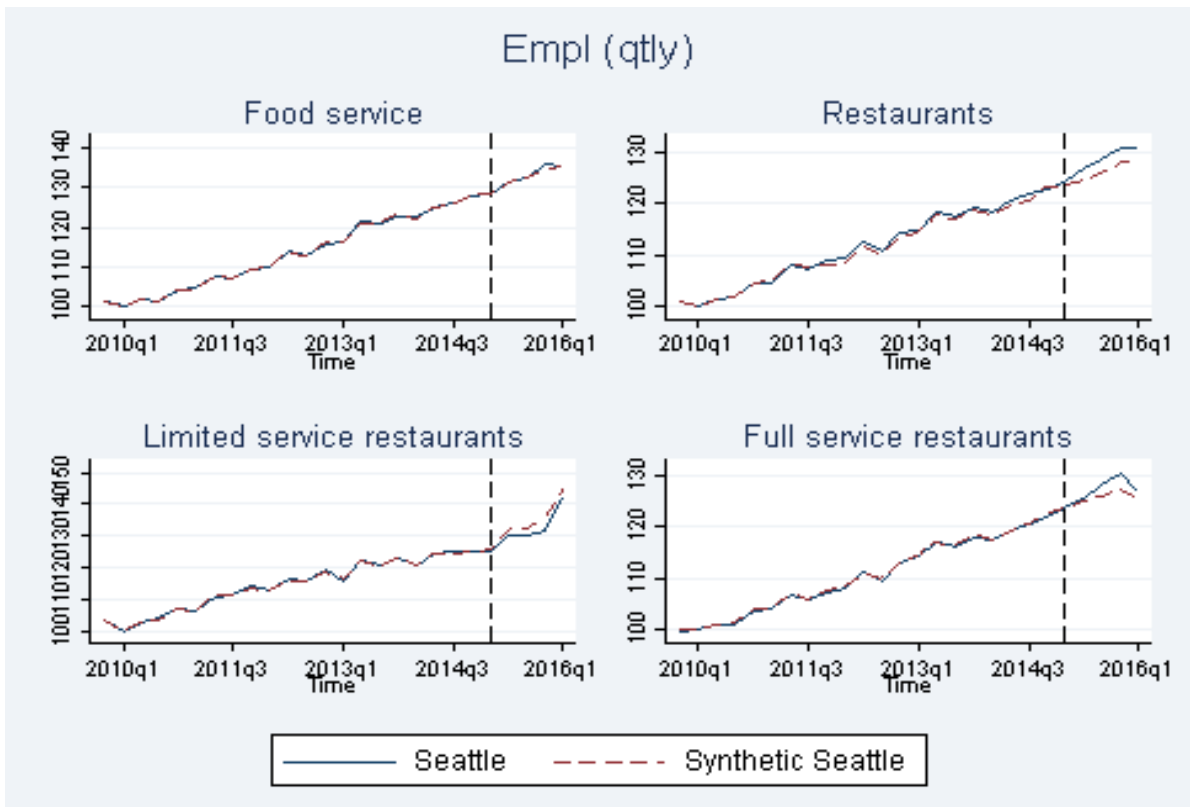


Notes: City-level QCEW data for Seattle. County-level QCEW data for the donors that make up Synthetic Seattle. See Appendix B for a list of donors. The vertical dashed line refers to April 1, 2015, the implementation date of the first phase. The second increase occurred on January 1, 2016.

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 envelope 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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Working Paper 17-088



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Survival of the Fittest:
The Impact of the Minimum Wage on Firm Exit^{*}

Dara Lee Luca[†] and Michael Luca[‡]

April 2017

Abstract

We study the impact of the minimum wage on firm exit in the restaurant industry, exploiting recent changes in the minimum wage at the city level. The evidence suggests that higher minimum wages increase overall exit rates for restaurants. However, lower quality restaurants, which are already closer to the margin of exit, are disproportionately impacted by increases to the minimum wage. Our point estimates suggest that a one dollar increase in the minimum wage leads to a 14 percent increase in the likelihood of exit for a 3.5-star restaurant (which is the median rating), but has no discernible impact for a 5-star restaurant (on a 1 to 5 star scale).

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I. Introduction

The minimum wage has recently re-entered the forefront of policy discourse as federal proposals range from leaving it as is, or increasing it to \$10.10 or even higher. Some proposals include raising the federal minimum to uncharted territory of \$15 per hour.¹ While the federal minimum wage has remained stagnant since 2009, states – and more recently, cities – have increasingly set local minimum wages above the federal mandate of \$7.25. In the San Francisco Bay Area alone, there have been twenty-one local minimum wage changes over the past decade.

In this paper, we investigate the impact of the minimum wage on restaurant closures using data from the San Francisco Bay Area. We find suggestive evidence that an increase in the minimum wage leads to an overall increase in the rate of exit. However, this masks important heterogeneity. At any minimum wage level, lower quality restaurants, as proxied by their ratings on the review platform Yelp are more likely to exit. Moreover, lower quality restaurants are disproportionately affected by minimum wage increases. In other words, the impact of the minimum wage on exit is more pronounced among lower-rated restaurants.

The restaurant industry in the Bay Area makes a compelling setting to investigate the impact of the minimum wage on small businesses. First, the restaurant industry is the most intensive employer of minimum wage workers (U.S. Bureau of Labor Statistics, 2016b). Second, there is high turnover within the restaurant industry. In our sample – which covers restaurants in the Bay Area from 2008 through 2016 – roughly 5 percent of restaurants go out of business each year. Hence, the exit margin is economically meaningful. Additionally, there is no tip credit in

¹ While his exact stance on the minimum wage is not clear, President Trump has intimated that he would prefer to eliminate the federal minimum wage and let states determine their own minimum wages (<http://www.politico.com/blogs/2016-presidential-debate-fact-check/2016/10/trump-kaine-minimum-wage-229149>). Bernie Sanders proposed a \$15 federal minimum wage as part of his presidential campaign in 2016 (<https://berniesanders.com/issues/a-living-wage/>).

California. Hence, tips do not count toward the official wage and wait staff are covered by the same minimum wage as other employees, so the minimum wage is more likely to be binding. Finally, there has been a substantial number of city-level minimum wage increases in the area since 2008, with a number of cities implementing minimum wages upwards of \$12.

Our analysis proceeds in three stages. First, we provide evidence that lower quality businesses are, on average, closer to the margin of exit and fail at higher rates than higher quality restaurants irrespective of the minimum wage level. A one-star increase in rating is associated with more than a 50% decrease in the likelihood of going out of business. This qualitative relationship holds both with and without restaurant effects.

We then exploit the multiple city-level minimum wage changes in recent years across the Bay Area to implement a difference-in-differences design to investigate the effects of the minimum wage. We find suggestive evidence that a higher minimum wage leads to overall increases in restaurant exit rates – depending on the specification, we find that a \$1 increase in the minimum wage leads to approximately a 4 to 10 percent increase in the likelihood of exit, although the estimate is only statistically significant in certain specifications.

Next, we present robust evidence that the impact of the minimum wage varies with the rating of the business. Our point estimates suggest that a \$1 increase in the minimum wage leads to an approximate 14 percent increase in the likelihood of exit for the median 3.5-star restaurant but the impact falls to zero for five-star restaurants. These effects are robust to a number of different specifications, including controlling for time-varying county characteristics that may influence both minimum wage policies and restaurant demand, city-specific time trends to account for preexisting trends, as well as county-year fixed effects to control for spatial heterogeneity in exit trends.

Our results contribute to the existing literature in several ways. First, our findings relate to a large literature seeking to estimate the impact of the minimum wage, most of which has focused on identifying employment effects. While some studies find no detrimental effects on employment (Card and Krueger 1994, 1998; Dube, Lester & Reich, 2010), others show that higher minimum wage reduces employment, especially among low-skilled workers (see Neumark & Wascher, 2007 for a review). However, even studies that identify negative impacts find fairly modest effects overall, suggesting that firms adjust to higher labor costs in other ways. For example, several studies have documented price increases as a response to the minimum wage hikes (Aaronson, 2001; Aaronson, French, & MacDonald, 2008; Allegretto & Reich, 2016). Horton (2017) find that firms reduce employment at the intensive margin rather than on the extensive margin, choosing to cut employees hours rather than counts. Draca et al. (2011) document lower profitability among firms for which the minimum wage may be more binding.

Our study contributes to the existing literature by examining one channel of adjustment to the minimum wage that has received relatively little attention – firms could exit the market altogether. We provide suggestive evidence that the minimum wage increases overall restaurant exit. This finding is consistent with Aaronson et al. (forthcoming), who use a border discontinuity approach to show that restaurant exit increases after the minimum wage increases.

However, our results reveal that the average treatment effect can be substantially different from the impact on sets of businesses that are predictably closer to the margin. While lower rated restaurants are driven to exit by increases to the minimum wage, higher rated restaurants tend to be more insulated from such shocks. This helps to shed light on the likely impact of minimum wage increases on existing businesses. .

Our analysis also highlights how digital data can be used to better understand labor policy and the economy. Historically, datasets from the US Census Bureau and the Bureau of Labor Statistics (BLS) have formed the backbone of analyses looking to estimate the impact of the minimum wage in the US (e.g. Dube, Lester & Reich, 2010, Aaronson et al., forthcoming). Other analyses consist mainly of researcher-administered surveys (e.g. Katz & Krueger, 1992; Card & Krueger, 1994).

While administrative datasets are critical to our understanding of the minimum wage and the economy more generally, the effects we identify in this paper would have been difficult to observe using standard datasets. The growth of online review platforms such as Yelp allows for unique insights into the economy. First, we can use each restaurant's rating as a proxy for its quality, a measure that is not captured by conventional datasets. This lets us to evaluate whether the minimum wage differentially impacts lower quality businesses. Second, we are able to use exit data in close to real time, whereas BLS and Census data only become publicly available after a lag. This allows researchers and policymakers to more quickly understand the impacts of different economic policies. Third, we are able to observe granular data on businesses, whereas the public versions of the Census and BLS data are aggregated to coarser geographic levels, such as by county (depending on the variable the researcher is interested in). In principle, researchers can access restricted business-level data via an extensive application process, but the current waiting period for access even among approved applications is estimated to be two years. For example, a researcher trying to understand the impact of a policy change in 2017 would not be able to examine firm-level microdata from the Census until at least 2020. By using digital data, researchers can measure the impacts in close to real time.

The rest of the paper proceeds as follows. We discuss the landscape of minimum wages across the United States in recent years in Section II. Section III discusses the data and empirical evidence, as well as graphical evidence. Section IV reports the main results, and Section V concludes.

II. The Minimum Wage in Recent Years

The current federal minimum wage of \$7.25 is binding for roughly 2.6 million hourly workers (U.S. Bureau of Labor Statistics, 2016a), with the restaurant industry having the highest percent of employees at the minimum (U.S. Bureau of Labor Statistics, 2016b). In addition to the \$7.25 federal minimum wage rate, 29 states and 41 cities have introduced higher than federal minimum wage. For example, San Francisco is set to increase its minimum wage to \$15 in July 2018 from its current wage of \$12.25.

We focus our analysis on the Bay Area, a region comprising of 101 cities surrounding the San Francisco Bay. The Bay Area is home to more than 7.5 million people, and includes the major cities and metropolitan areas of San Jose, San Francisco, and Oakland. Among the 41 cities and counties that have changed their minimum wage ordinances at the local level since 2012, 15 were in the Bay Area.² We document 21 total local changes during our sample period from 2008 through 2016, with four additional cities set to increase their minimum beginning in 2017. Beyond the wide variation in minimum wage, focusing on a single region potentially allows us to better control for macroeconomic trends and attitudes towards labor standards.

² See <http://laborcenter.berkeley.edu/minimum-wage-living-wage-resources/inventory-of-us-city-and-county-minimum-wage-ordinances/>

[Figure 1](#) depicts the changes for the state of California and 11 cities in the state of California that have increased their minimum wage since 2008.³ In cities with separate minimum wages for large (usually defined as over 500 employees) and small companies, we use the minimum wage for small companies. This is because the majority of full-service and limited-service restaurants have fewer than 500 employees (U.S. Census Bureau, 2014). At the state level, the minimum wage was set at \$8 in the beginning of the sample, increased to \$9 in 2014, and then to \$10 in 2016.

III. Data and Empirical Strategy

A. Restaurant Data

Our underlying restaurant data are obtained from Yelp, as part of an ongoing economic research initiative done in collaboration with the company. Yelp was founded in 2004 in San Francisco and is now the dominant review platform in the US. On Yelp, users can leave text reviews and ratings (from 1 to 5) for individual businesses, ranging from dry cleaners to dentists. However, it is perhaps best known as a review platform for restaurants.

We start with the universe of all Yelp reviews for the Bay Area since 2008, and limit the dataset to only reviews for full-service and limited-service restaurants. Based on the review-level data, we form an unbalanced panel dataset at the restaurant-month level, where a restaurant enters the panel when it becomes active on Yelp (either by the owner registering the business, a reviewer registering the business, or receiving the first review), and leaves the panel after it has been marked as having been closed on Yelp.

³ Four additional cities (San Leandro, Cupertino, Los Altos, San Mateo) are slated to increase their minimum wage above the state level in 2017.

The indicator for restaurant exit is crowdsourced. On each restaurant’s Yelp page, users have the option of updating the restaurant’s business details, including tagging it as having closed or moved. Any suggested changes are then verified by Yelp moderators before being marked as such on the restaurant’s profile page. In practice, timing of exit through Yelp may also be more accurate than official administrative data, which contains nontrivial reporting lags and errors. In the review data, we exclude filtered reviews, which are deemed by Yelp’s algorithm as more likely to be fake or untrustworthy (Luca & Zervas, 2016). The dataset contains basic information about the restaurant, including the type of cuisine (e.g., “New American”, “Chinese”), the price category of the restaurant (denoted by dollar signs ranging from \$ to \$\$\$\$), with four dollar signs being the most expensive)⁴, the exact location, and also time-varying characteristics such as the running average rating, the number of reviews, and exit status.⁵

Yelp coverage of restaurants is close to universal in the Bay Area. Comparing Yelp data to administrative data obtained for the city of San Francisco,⁶ the number of restaurants active at the end of 2016 is 6,087 and 5,808 based on the San Francisco administrative and Yelp data, respectively. Exit statistics generated from the two datasets are similar and consistent with previous research. For example, a common statistic that the restaurant industry focuses on is the rate of closure within one year of entry. Based on the administrative data, 19.8 percent of restaurants exit within one year of entry, whereas Yelp data indicates 20.9 percent. Other research on the restaurant industry has demonstrated similar numbers ranging from 23 percent in

⁴ Price category is a crowd sourced element. Upon reviewing a restaurant, users are able to designate dollar signs based on the following criteria: \$= *under \$10*, \$\$= *11–30*, \$\$\$= *31–60*, \$\$\$\$= *over \$61*.

⁵ We constructed these variables such that they capture the measure at the end of the month, for example, the running average of the restaurant at the end of the month, or the displayed rating at the end of the month.

⁶ SF OpenData is the central clearinghouse for data published by the City and County of San Francisco, and includes a database of registered businesses that pay taxes, including their date of entry and exit. We restricted to the NAICS code of 722 (full-service restaurants and limited-service restaurants).

Dallas, Texas (Cline Group, 2003) to around 26 percent in Columbus, Ohio (Parsa, Self, Njite, & King, 2005).

We present two descriptive statistics of the data. The first set of statistics provides a snapshot of the restaurants' last appearance in the panel, i.e., at the end of 2016 or at the time of exit ([Table 1](#) Panel A). There are 35,173 unique restaurants in our dataset, with a mean number of 184 reviews per restaurant and an average rating of 3.6.⁷ Among the entire universe of restaurants, around 30 percent have closed. Restaurants remain in the panel for an average of 70 months⁸ and have an average price sign of 1.6 “dollar signs”.

The second set of statistics shows a summary at the monthly panel level ([Table 1](#) Panel B). A restaurant receives on average 2.5 new reviews each month with an average rating of 3.5. The likelihood of exit in any month is 0.4 percent.

B. Graphical Evidence

We first present graphical evidence of the relationship between a restaurant's operational status and its rating. [Figure 2a](#) depicts a snapshot of the overall distribution of restaurant ratings when last observed in the dataset. The modal rating is 3.5, and ratings are generally more positive than negative; there are fewer than 5 percent of restaurants with ratings 2 and below, whereas 40 percent of restaurants have an average rating of 4 or above. [Figure 2b](#) overlays the distribution by whether the restaurant has closed. The mass of ratings for closed restaurants is concentrated towards lower ratings relative to operating restaurants, suggesting that a restaurant's rating is correlated with closure

⁷ While Yelp displays ratings rounded to the nearest 0.5 on their website, we use unrounded version in the analysis. (Whether we use the rounded or unrounded version of ratings does not affect the conclusions of our analysis.)

⁸ Note that this statistic may not accurately represent average lifespan of a restaurant since when the restaurant becomes active on Yelp may not necessarily be the same as when the restaurant began operations.

We further explore this by plotting the simple means of the monthly likelihood of exit by displayed rating (which is the average rating rounded to the nearest 0.5). [Figure 3](#) depicts a clear negative relationship between the likelihood of exit and rating, again implying that restaurants with lower ratings are closer to the margin of exit.

Next we explore the cross-sectional relationship between the likelihood of exit and the minimum wage. [Figure 4](#) plots the mean likelihood of exit by minimum wage, which shows a distinctly positive correlation. However, it is possible that larger or wealthier cities implement the minimum wage, and exit rates are systematically different (higher) in those cities as well. To investigate this, we obtain the residuals from regressing the likelihood of exit on city dummies, and plot the mean residuals against the minimum wage ([Figure 5](#)). While the slope is less pronounced, there still remains a positive relationship between the minimum wage and the likelihood of exit.

[Figure 6](#) examines the likelihood of exit by restaurant rating and minimum wage. The figure synthesizes our empirical strategy and our main result: at any rating level, the likelihood of exit is higher when the minimum wage is higher. However, the increase in the likelihood of exit is greater for lower rated restaurants, and there does not appear to be any penalty for the highest rated restaurants. We confirm this finding using a regression framework in Section 4.

C. Empirical Strategy

The graphical evidence presented in Section 3.B suggests three things. First, restaurants with lower ratings are more likely to exit. Second, higher minimum wages are correlated with higher probabilities of exit. Third, the increase in the likelihood of exit is greater for lower rated restaurants.

, We then use a difference-in-difference framework to empirically analyze the impact of the minimum wage on restaurant exit decisions, in which exploit the temporal and spatial variation in minimum wage increases at the city level across the Bay Area. The basic regression model, estimated as a linear probability model, is as follow:

$$Exit_{ijt} = \alpha_i + \phi'_t \lambda + \beta MW_{jt} + X'_{ijt} \delta + Z'_{jt} \rho + \varepsilon_{ijt} \quad (1)$$

where $Exit_{ijt}$ is a binary variable denoting whether restaurant i in city j has exited by time t . MW_{jt} is the minimum wage (measured in dollar amounts) in that city, α_i are restaurant fixed effects, ϕ'_t is a vector of time controls, including year and quarter dummies to capture variation in macroeconomic conditions and seasonal variation in restaurant demand. X'_{ijt} are time-varying restaurant measures, such as the number of ratings and lagged running average rating.⁹ Z'_{jt} includes a host of county-level time-varying characteristics that may influence both restaurant demand and minimum wage policies, including the percent of young workers between ages 15 to 24, percent black, percent under the poverty line, the unemployment rate, and logged per capita income. ε_{ijt} is the error term. In some specifications, we include city-specific time trends to account for preexisting trends in local exit rates. We also include county-year fixed effects in certain specifications to control for spatial heterogeneity in exit trends that are unrelated to minimum wage policies. The estimated impact of a \$1 increase in the minimum wage is then given by $\hat{\beta}$. Standard errors are clustered by city to allow for serial correlation within locale.

⁹ Restaurant characteristics that are constant over time, such as the price category, location, type of cuisine, are controlled implicitly by restaurant fixed effects.

We also enter the city-level minimum wage as the proportional increase over the state mandate, Gap_{jt} . As an example, if the state minimum wage is \$8 and the city minimum wage is \$9, the Gap measure would be 12.5. This measure reflects both increases in minimum wage within the city as well as relative to the state mandate.

We then estimate the heterogeneous effects of the minimum wage by including an interaction term of the minimum wage with the restaurant's rating. More specifically, our estimating equation becomes:

$$Exit_{ijt} = \alpha_i + \phi'_t \lambda + \beta MW_{jt} + \gamma Rating_{ijt} + \theta MW_{jt} * Rating_{ijt} + X'_{ijt} \delta + Z'_{jt} \rho + \varepsilon_{ijt} \quad (2)$$

where $\hat{\theta}$ would provide an estimate of how the minimum wage affects exit by the restaurant's quality, as measured by its rating.

IV. Main Results

As in our graphical evidence, we first examine the relationship between a restaurant's likelihood of exit and its Yelp rating ([Table 2](#)). Cross-sectionally, a one-star increase in rating is associated with a 0.09 percentage point decrease in the likelihood of exit in any given month (column 1), which is consistent with Figure 3. After controlling for restaurant fixed and calendar fixed effects, the coefficient increases to approximately -0.29 percentage point (Column 2). The relationship remains stable when we include time-varying county characteristics, city-specific time trends and county-year fixed effects (Columns 3-5). Our results imply a one-star increase in rating is associated with a decline in the likelihood of exit of around 70 percent. This is not necessarily a causal relationship – it is certainly possible that poor quality restaurants are both more likely to exit and receive worse ratings. It could also be that lower ratings directly contribute to restaurants exiting; as Luca (2011) shows, a one-star increase in Yelp rating leads

to a 5 to 9 percent increase in restaurant revenue. Our objective is to test whether restaurants with lower ratings tend to be closer to the margin of exit.

We find suggestive evidence that higher minimum wage increases restaurant exit ([Table 3](#)). Panel A reports the coefficients on the minimum wage entered as a dollar measure in the regression model, whereas Panel B reports those on the *Gap* variable as defined in Section III.C, which is a measure of how much the city minimum wage is above the state mandate. Cross-sectionally, a one-dollar increase in the minimum wage is associated with a 0.09 percentage point increase in the probability of exit, which represents a 22 percent increase (Panel A Column 1). However, the estimate falls to 0.04 percentage points and loses statistical significance when we layer on restaurant and calendar fixed effects (Panel A Column 2). The estimate becomes even more imprecise when we include time-varying county characteristics that may influence both minimum wage policy and restaurant demand, city-specific time trends, and county-year fixed effects (Panel A Columns 3-5)

We find similar results when we examine the impact of the minimum wage as the percent increase over the state mandate, which may give a better measure of the “bite” of the minimum wage. Depending on the specification, our estimates suggest that a 10-percent increase of the local minimum wage over the state mandate would increase the overall exit rate ranges from 0.016 to 0.04 percentage points, which corresponds to an increase in the likelihood of exit of 4 to 10 percent. While the estimates are generally more precise than in Panel A, they only reach statistical significance in certain specifications.

Overall impacts could mask underlying heterogeneous effects if the minimum wage differentially affects restaurants of varying quality. To examine this, we include the interaction effect between a restaurant’s rating and the minimum wage, as specified in Equation (2). [Table 4](#)

reports the main results of our paper: the minimum wage increases the likelihood of exit, but the impact falls for higher-rated restaurants. The estimates remain similar across the different specifications. Based on the estimates in Column (2), the results would suggest that the impact of a \$1 rise in the minimum wage would increase the likelihood of exit for the median restaurant on Yelp (i.e., a 3.5 star restaurant) by around 0.055 percentage points, which is approximately 14 percent. For a 5-star restaurant, this impacts falls to close to zero.

The results are consistent when we enter the minimum wage in the model as the percent above the state mandate ([Table 5](#)). A one-star increase in Yelp rating is associated with a 0.26 percentage point decline in the likelihood of exit for a restaurant in a city with the minimum wage equal to the state mandate, which is consistent with the results from Table 2. Further, the impact of the minimum wage varies by restaurant quality: a 10 percent increase in the minimum wage above the state mandate increases the likelihood of exit for a 3.5-star restaurant by 0.05 percentage points, translating into a 13.75 percent increase. The impact falls roughly by 0.09 percentage points for each star increase. The estimates are similar and statistically significant with city-specific time trends and county-year fixed effects. Finally, [Figure 7](#) plots the predicted likelihood of exit by rating for different minimum wages from the specification in Table 4 Column 5, and echoes [Figure 6](#). The figure shows that the predicted likelihood of exit is generally higher across ratings when the minimum wage is higher, but the impact, as well as the difference in impact across the three lines, shrinks as rating increases.

V. Further Investigation

A. Are Results Driven by Restaurant Prices?

If ratings are systematically correlated with prices – e.g., if cheap restaurants tend to receive low ratings, and expensive restaurants high ratings – then our results in Tables 4 and 5

may be confounded. Further, it could be that more expensive restaurants already pay wages above the minimum, and hence are less affected by minimum wage hikes. Are the heterogeneous effects we observe driven by how expensive the restaurant is rather than its quality?

We empirically examine this question by replacing $MW_{jt} * Rating_{ijt}$ in Equation 2 with the interaction term of the restaurant's price category (represented by dollar signs on Yelp) and the minimum wage $MW_{jt} * Price_i$ ([Table 6](#)). The coefficient on the interaction term is small and statistically insignificant, suggesting that the effects of the minimum wage along the price dimension are not significantly different (Column 1). When we include $MW_{jt} * Rating_{ijt}$ in the model as well, the coefficient on $MW_{jt} * Price_i$ remains insignificant, whereas the coefficient on $MW_{jt} * Rating_{ijt}$ are statistically significant and similar in magnitude to those in Table 5, providing evidence that the heterogeneous effects observed earlier are driven by quality rather than by the restaurant prices.

B. Impact on Entry

A natural follow-up question to our results on exit is the impact of the minimum wage on entry. Dates on restaurant entry only became regularly recorded by Yelp at the end of 2009, hence we restrict our entry analysis to the post-2010 period. To examine entry, we generate a city-level panel dataset based on our restaurant-level dataset and estimate the analogous version of Equation (1) using the entry rate as the dependent variable, weighted by the number of restaurants on Yelp in that city.

[Table 7](#) reports the results of this exercise. First, we find similar overall impacts of the minimum wage on exit as our restaurant-level analysis (Columns 1-3). Next, we find that the entry rate in fact declines with minimum wage increases – depending on the specification, the

entry rate declines by 0.025 to 0.045 percentage points from a base of 0.6 percent from a \$1 increase in the minimum wage, corresponding to an approximate 4 to 6 percent reduction. The number of restaurants per capita falls as expected, but the estimates are not statistically significant (Columns 7-9).

Our results suggest that higher minimum wages deter entry. Previous research on entry has produced mixed findings. Using a border discontinuity approach and data from Dun and Bradstreet Marketplace files, Rohlin (2011) finds that minimum wage hikes implemented between 2003 and 2006 discouraged firm entry – a \$1 increase in the minimum wage decreased the share of new establishments in an area relative to its comparison area by approximately 6 percent. Draca and Machin (2011) find some suggestive evidence that net entry rates decline after the imposition of a national minimum wage in the United Kingdom. In contrast, Aaronson et al. (forthcoming) finds that a 10 percent increase in the minimum wage increases the entry rate by roughly 14 percent from a mean of 8.7 percent using a similar border discontinuity approach and QCEW data.

C. Impact on Survival

In addition to the overall monthly likelihood of exit, we examine the effect of the minimum wage on restaurant time to exit. Since this relies on accurate coding of entry dates, we also restrict the analysis to after 2010. We estimate a survival model where the dependent variable is time to exit using a Weibull distribution (Table 8). The coefficients indicate that overall, the minimum wage increases the hazard rate, but the estimates are not statistically significant (Columns 1 and 3). However, when we interact the minimum wage with the restaurant's rating, we can see that the coefficient on the interaction term of minimum wage (or

gap) with rating is negative and statistically significant, suggesting that the speed to exit is accelerated for poorly rated restaurants (Columns 2 and 4).

V. Discussion

This paper presents several new findings. First, we provide suggestive evidence that higher minimum wage increases overall exit rates among restaurants, where a \$1 increase in the minimum wage leads to approximately a 4 to 10 percent increase in the likelihood of exit, although statistical significance falls with the inclusion of time-varying county-level characteristics and city-specific time trends. This is qualitatively consistent but smaller than what Aaronson et al. (forthcoming) find; they show that a 10 percent raise in the minimum wage increases firm exit by approximately 24 percent from a base of 5.7 percent. Differences in sample and specifications may account for the differences between our study and theirs.

Next, we examine heterogeneous impacts of the minimum wage on restaurant exit by restaurant quality. The textbook competitive labor market model assumes identical workers and firms who therefore are equally likely to share in the minimum-wage generated employment and profit losses. However, models that depart from the standard competitive model to allow for heterogeneous workers and firms suggest that a minimum wage increase would cause the lowest productivity firms to exit the market (Albrecht & Axell, 1984; Eckstein & Wolpin, 1990; Flinn, 2006). We show that there is, in fact, considerable and predictable heterogeneity in the effects of the minimum wage, and that the impact on exit is concentrated among lower quality restaurants, which are already closer to the margin of exit. This suggests that the ability of firms to adjust to minimum wage changes could differ depending on firm quality. Finally, we provide evidence that higher minimum wages deter entry, and hastens the time to exit among poorly rated restaurants.

Our findings suggest directions for future research. First, because most minimum wage changes in our sample are relatively new, our results should be considered short-term impacts. Second, while we find that the minimum wage reduces net entry slightly, it is unclear how employment would be affected given that the scale of entering or incumbent restaurants could change.¹⁰ Third, our results raise the possibility that higher rated restaurants may adjust to higher minimum wages through other channels, such as substituting toward higher productivity workers when faced with a minimum wage (Horton, 2017), especially if higher quality restaurants are able to assortatively match with more productive workers (Eeckhout & Kircher, 2011; Mendes et al., 2010).

Our results also demonstrate the potential for digital exhaust from online platforms to complement standard data sources to provide unique insight in policy evaluations. Glaeser et al (forthcoming) hypothesize that data from online platforms might provide dependent variables that are more granular and closer to real time, as well as independent variables that provide insight into dimensions of markets that were previously unobservable. Our analysis provides a case study in this, showing how digital exhaust from Yelp can further our understanding of the impact of the minimum wage.

¹⁰ The limited existing evidence on the interaction effect of firm dynamics and employment has been mixed. Anderson et al. (forthcoming) find the minimum wage increases exit (and entry) but do not find any impacts on employment. Draca and Machin (2011) find some evidence that minimum wages decreases net entry but no significant effects on employment.

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Figure 1. Minimum wage increases in the San Francisco Bay Area

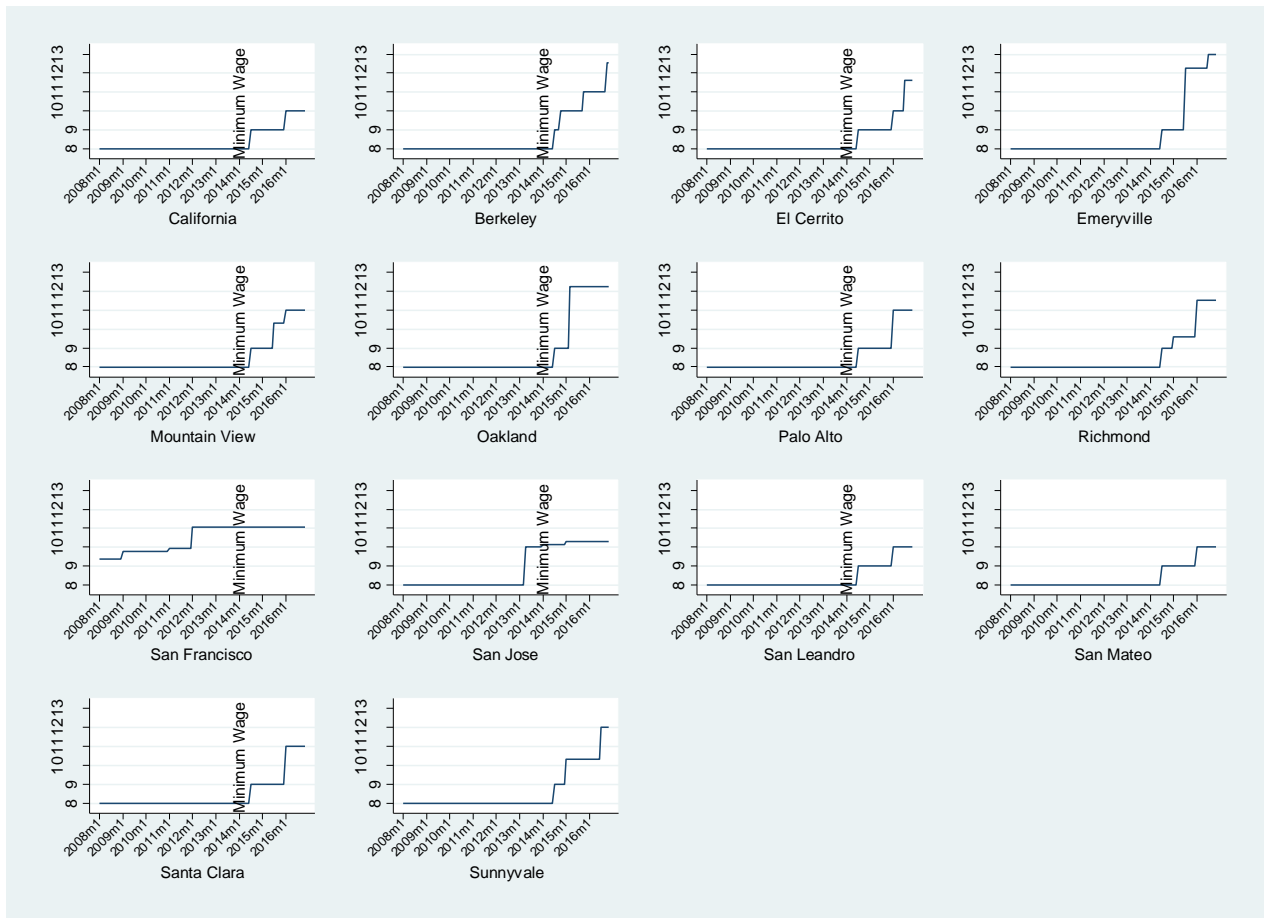


Figure 2a. Overall distribution of Yelp ratings

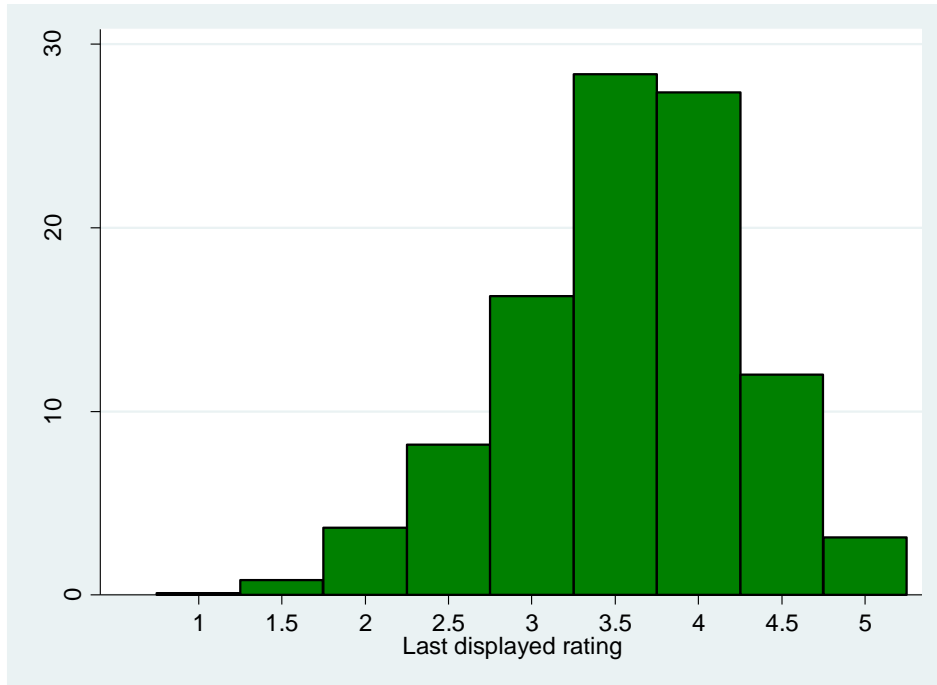


Figure 2b. Closed restaurants have lower ratings

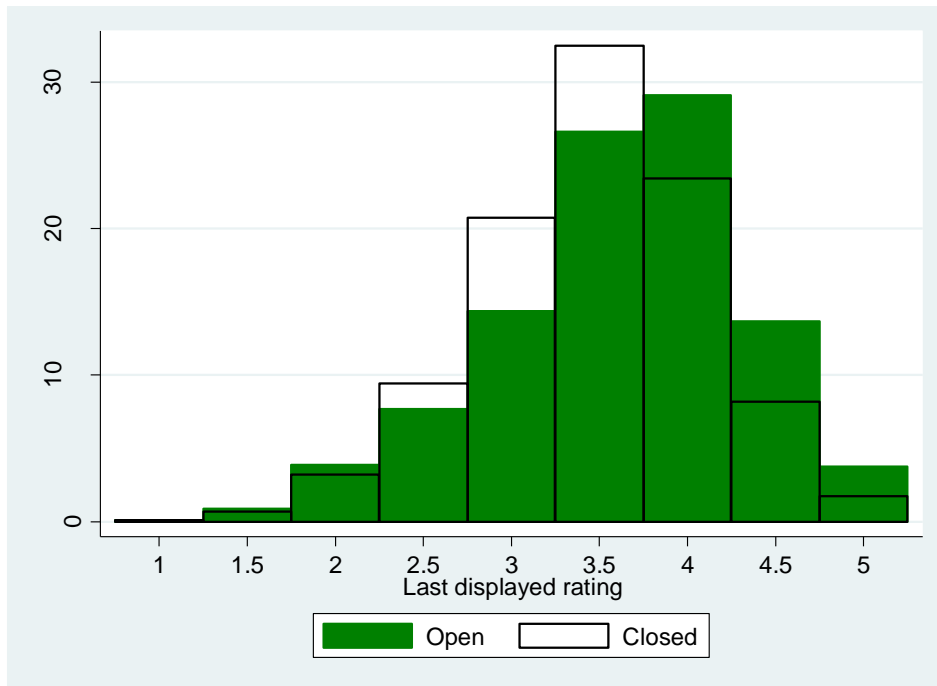
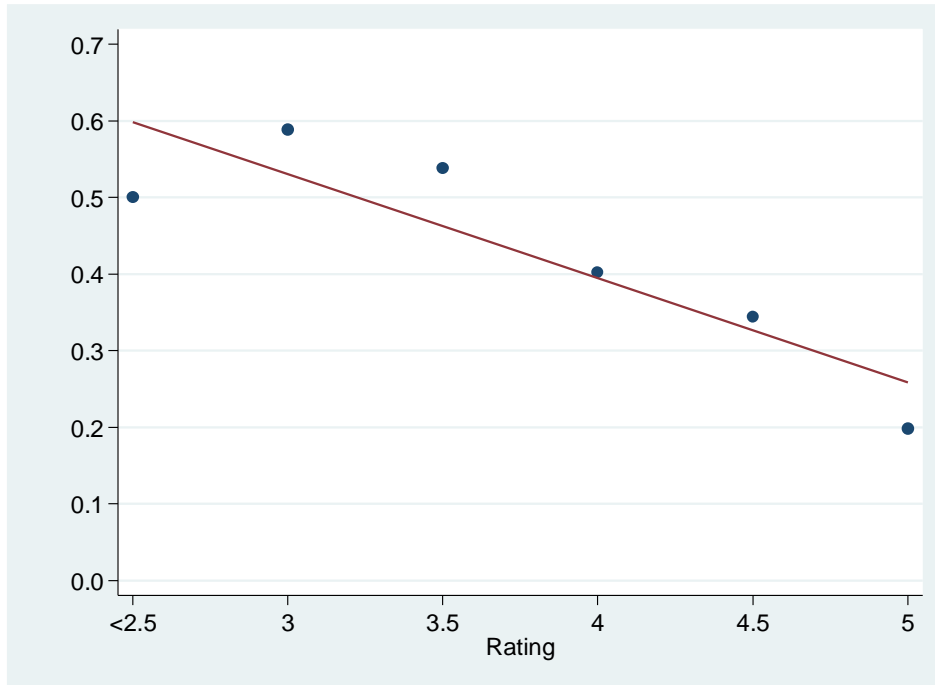
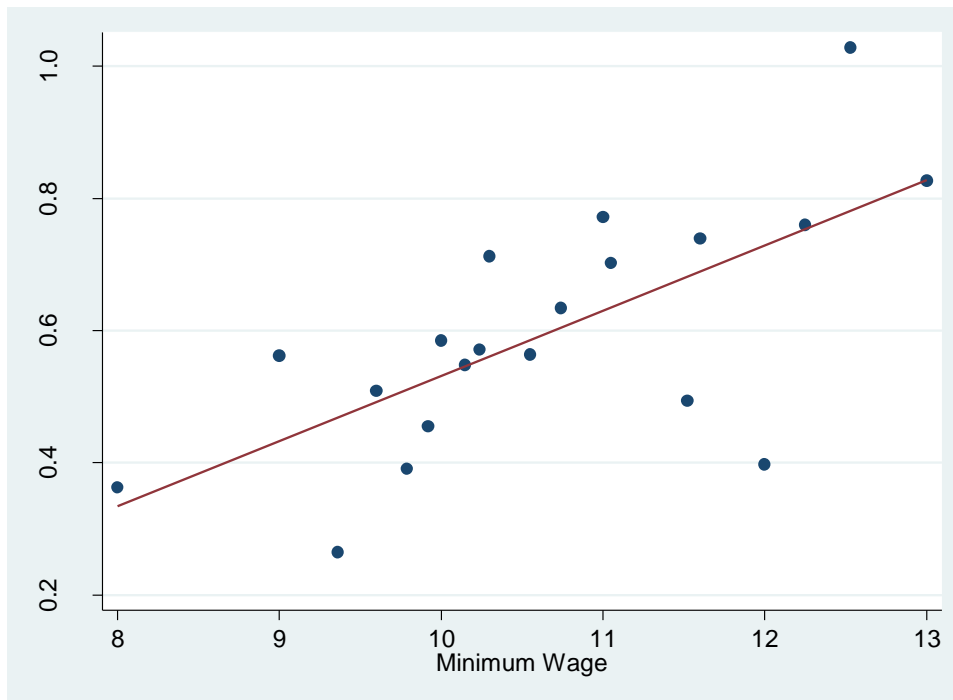


Figure 3. Lower rated restaurants are more likely to exit



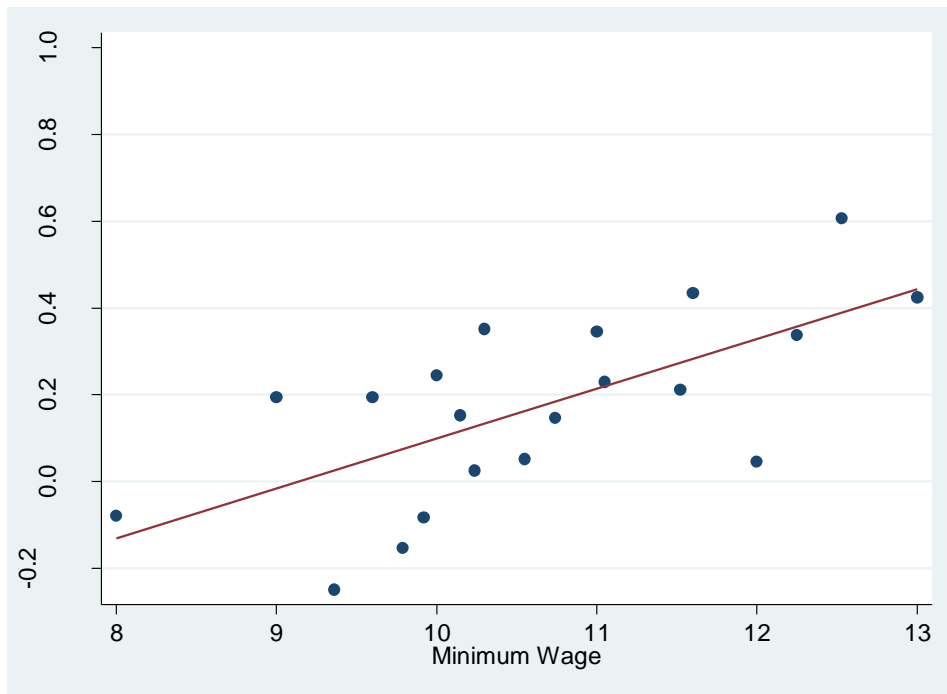
Note: This figure plots the monthly likelihood of exit at each Yelp rating.

Figure 4. Minimum wage and likelihood of exit



Note: This figure plots the simple means of the likelihood of exit at each minimum wage.

Figure 5. Minimum wage and likelihood of exit (within city)



Note: This figure plots the simple means of the residuals of regressing the likelihood of exit on city fixed effects at each minimum wage.

Figure 6. Minimum wage increases exit, but more so for worse restaurants

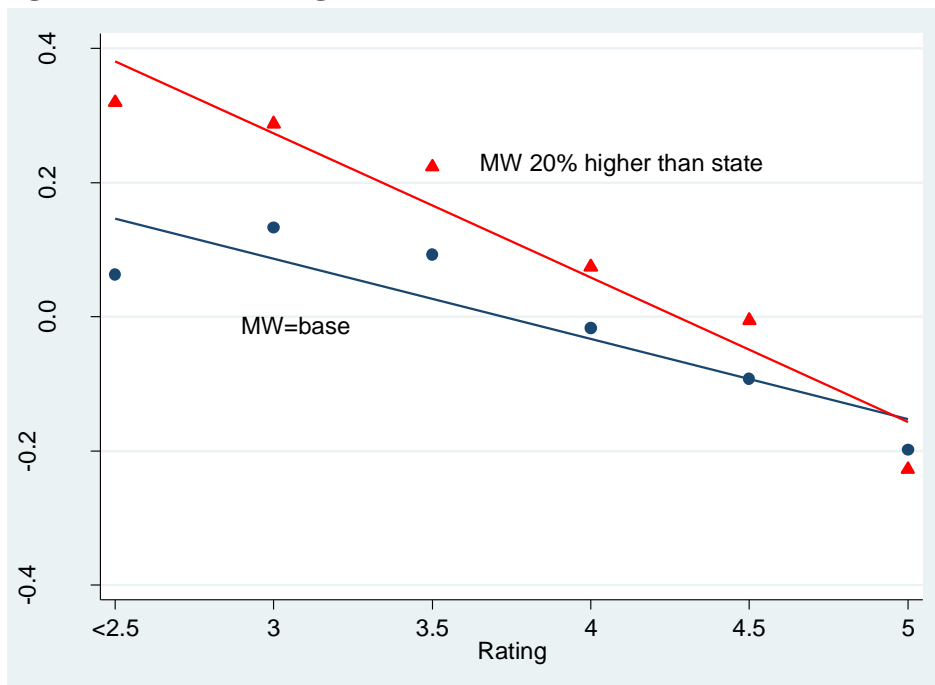
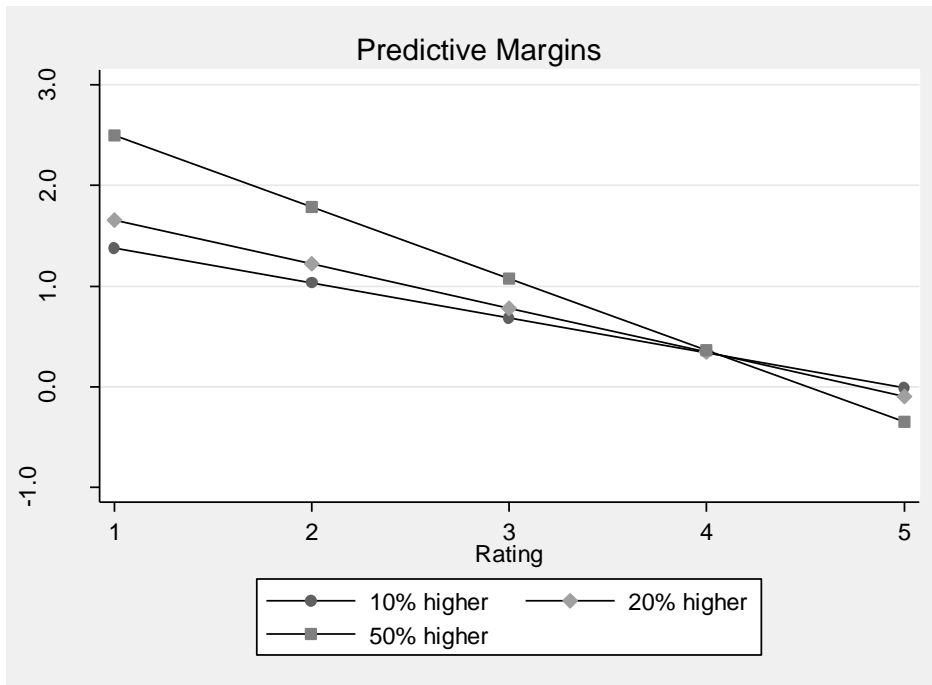


Figure 7. Predicted likelihoods of exit by minimum wage and rating



Note: This figure plots the predicted likelihood of exit by rating and the percent increase of local minimum wage above the state mandate based on the estimates from Table 4 Column 5.

Table 1. Descriptive statistics

Number of restaurants 35,173
Number of ratings 2,392,766

Panel A: Summary statistics at the restaurant level (at time of last appearance in panel)

Variable	Mean	Std Dev	Min	Max	Obs.
Total number of ratings	184.4	327.1	10	9781	35,173
Rating	3.564	0.691	1	5	35,173
Closed	0.301	0.459	0	1	35,173
Price category	1.588	0.603	1	4	35,173
Age of restaurants (months)	67.73	36.52	1	107	35,173
Minimum wage (\$)	10.49	1.534	8	13	35,173
Percent higher than state mw (%)	9.839	12.84	0	36	35,173

Panel B: Summary statistics at the restaurant-month level

Variable	Mean	Std Dev	Min	Max	Obs.
Incoming ratings	3.535	1.105	1	5	1,430,061
Number of incoming ratings	2.454	4.790	0	690	2,383,558
Average running rating	3.570	0.707	1	5	2,376,580
Exited (%)	0.464	6.792	0	100	2,392,766
Minimum wage (\$)	9.033	1.291	8	13	2,392,766
Percent higher than state mw (%)	7.346	11.77	0	36.1	2,392,766

Table 2. Are lower rated restaurants more likely to exit?

	Likelihood of Exit (Mean = 0.4%)				
	(1)	(2)	(3)	(4)	(5)
Rating	-0.0938*** (0.0116)	-0.2893*** (0.0277)	-0.2910*** (0.0287)	-0.2935*** (0.0280)	-0.2917*** (0.0290)
Restaurant FE		x	x	x	x
Calendar FE		x	x	x	x
Time-varying county characteristics			x	x	x
City-specific time trend				x	
County-year FE					x

Standard errors are clustered at the city level

Number of observations = 2,392,766

Calendar fixed effects = dummies for season and year

Table 3. Overall minimum wage effects on restaurant exit

	Likelihood of Exit (Mean = 0.4%)				
	(1)	(2)	(3)	(4)	(5)
Panel A: Minimum Wage	0.0929*** (0.0079)	0.0444 (0.0284)	0.0174 (0.0197)	-0.0132 (0.0134)	0.0263 (0.0181)
Panel B: Gap	0.0062*** (0.0006)	0.0045* (0.0024)	0.0026 (0.0019)	0.0016 (0.0017)	0.0040** (0.0018)
Restaurant FE		x	x	x	x
Calendar FE		x	x	x	x
Time-varying county characteristics			x	x	x
City-specific time trend				x	
County-year FE					x

Standard errors are clustered at the city level

Gap is a measure of the proportional increase of the city minimum wage over the state mandate

Number of observations = 2,392,766

Calendar fixed effects = dummies for season and year

Table 4. Heterogeneous effects of the minimum wage as a dollar measure

	Likelihood of Exit (%) (Mean = 0.4%)				
	(1)	(2)	(3)	(4)	(5)
Minimum wage	0.1639*** (0.0208)	0.2336*** (0.0786)	0.2047*** (0.0730)	0.1746** (0.0696)	0.2148*** (0.0732)
Rating	0.0653 (0.0547)	0.1598 (0.1341)	0.1521 (0.1387)	0.1514 (0.1406)	0.1526 (0.1403)
Minimum Wage * Rating	-0.0190*** (0.0064)	-0.0527*** (0.0165)	-0.0520*** (0.0171)	-0.0522*** (0.0172)	-0.0521*** (0.0174)
Restaurant FE		x	x	x	x
Calendar FE		x	x	x	x
Time-varying county characteristics			x	x	x
City-specific time trend				x	
County-year FE					x

Standard errors are clustered at the city level

Number of observations = 2,370,963

Calendar fixed effects = dummies for season and year

Table 5. Heterogeneous effects of the minimum wage measured as the proportional increase above the state mandate

	Likelihood of Exit (%) (Mean = 0.4%)				
	(1)	(2)	(3)	(4)	(5)
Gap	0.0170*** (0.0016)	0.0371*** (0.0096)	0.0338*** (0.0084)	0.0325*** (0.0081)	0.0349*** (0.0082)
Rating	-0.0831*** (0.0101)	-0.2557*** (0.0218)	-0.2589*** (0.0208)	-0.2615*** (0.0207)	-0.2597*** (0.0206)
Gap * Rating	-0.0029*** (0.0004)	-0.0091*** (0.0021)	-0.0087*** (0.0019)	-0.0086*** (0.0019)	-0.0086*** (0.0019)
Restaurant FE		x	x	x	x
Calendar FE		x	x	x	x
Time-varying county characteristics			x	x	x
City-specific time trend				x	
County-year FE					x

Table 6. Are results driven by restaurant prices?

	Likelihood of Exit (%)			
	0.4			
	(1)	(2)	(3)	(4)
Panel A				
Minimum Wage	0.0125 (0.0300)	0.1921** (0.0796)	0.1654** (0.0758)	0.2029** (0.0803)
Minimum Wage * Price	0.0019 (0.0102)	0.0059 (0.0106)	0.0035 (0.0103)	0.0054 (0.0107)
Rating		0.1464 (0.1424)	0.1452 (0.1441)	0.1479 (0.1435)
Minimum Wage * Rating		-0.0516*** (0.0177)	-0.0518*** (0.0178)	-0.0519*** (0.0178)
Panel B				
Gap	0.0019 (0.0028)	0.0339*** (0.0080)	0.0330*** (0.0075)	0.0352*** (0.0076)
Gap * Price	-0.0002 (0.0013)	0.0002 (0.0014)	-0.0001 (0.0013)	0.0000 (0.0013)
Rating		-0.2607*** (0.0211)	-0.2633*** (0.0210)	-0.2616*** (0.0210)
Gap * Rating		-0.0089*** (0.0019)	-0.0088*** (0.0018)	-0.0088*** (0.0019)
Restaurant FE	x	x	x	x
Calendar FE	x	x	x	x
Time-varying county characteristics	x	x	x	x
City-specific time trend	x		x	
County-year FE				x

Standard errors are clustered at the city level

Price indicates the price category of the restaurant, which ranges from 1 to 4

Gap is a measure of the proportional increase of the city minimum wage over the state mandate

Number of observations = 2,370,963

Calendar fixed effects = dummies for season and year

Table 7. Minimum wage effects on exit, entry, and number of restaurants

	Exit rate (%) (Mean = 0.4)			Entry rate (%) (Mean = 0.6)			Restaurants per 10,000 pop (Mean = 45.3)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Minimum Wage	0.0038 (0.0116)	-0.0030 (0.0126)	0.0185 (0.0138)	-0.0425*** (0.0139)	-0.0251* (0.0146)	-0.0449** (0.0193)	-0.1989 (0.1478)	-0.1149 (0.1187)	-0.1470 (0.1534)
Gap	0.0018* (0.0011)	0.0018 (0.0012)	0.0037*** (0.0013)	-0.0035*** (0.0010)	-0.0026*** (0.0009)	-0.0041*** (0.0015)	-0.0102 (0.0098)	-0.0026 (0.0079)	-0.0096 (0.0117)
Restaurant FE	x	x	x	x	x	x	x	x	x
Calendar FE	x	x	x	x	x	x	x	x	x
Time-varying county characteristics	x	x	x	x	x	x	x	x	x
City-specific time trend		x			x			x	
County-year FE			x			x			x

Each cell represents a different regression. Regressions are weighted by the number of restaurants at the city level.

Standard errors are clustered at the city level

Gap is a measure of the proportional increase of the city minimum wage over the state mandate

Number of observations = 8,134

Calendar fixed effects = dummies for season and year

Table 8. The impact of the minimum wage on survival rates

	Hazard Rate (Failure = Exit)			
	(1)	(2)	(3)	(4)
Minimum wage	0.0333 (0.0429)	0.4197*** (0.0854)		
Minimum wage * Rating		-0.1027*** (0.0199)		
Gap			0.0046 (0.0036)	0.0309*** (0.0076)
Gap * Rating				-0.0071*** (0.0018)
Rating		0.8606*** (0.1707)		-0.0133 (0.0188)

Standard errors are clustered at the city level. Coefficients are reported.

Gap is a measure of the proportional increase of the city minimum wage over the state mandate

Number of observations = 18,631

The survival model includes controls for the total number of ratings at exit or end of panel, time-varying county level characteristics, price category of the restaurant, and dummies for year of entry.



COMMUNITY DEVELOPMENT
DEPARTMENT

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EMAIL comdev@wilmette.com

Date: April 4, 2018
To: Michael Braiman, Assistant Village Manager
From: Lucas Sivertsen, Business Development Coordinator
Subject: Minimum Wage & Paid Sick Leave Interview Summary

The Minimum Wage & Paid Sick Leave Working Group asked Village staff to interview area real estate brokers, municipalities and their businesses to help understand the effects of the Cook County Ordinances. Questions for the interviews were developed by the Working Group. Below is a summary of the responses to those interviews.

Municipality Interviews

Village staff reached out to municipal officials of surrounding communities who have opted in to the Cook County Ordinances or who did not have an option because they are not home rule. These communities include Glencoe, Winnetka, Skokie and Evanston. Each of these contacts were made via phone calls placed with economic development offices or municipal administration.

None of the communities agreed to respond to the survey questions. While Glencoe did not formally respond to the questions, they did say too little time has passed to gain much insight into the impact of the ordinances. In addition, no businesses had contacted Glencoe since the ordinances became effective in July of 2017.

Skokie and Winnetka stated they did not wish to participate in the surveys after checking with municipal administration (Winnetka letter attached). In those cases, they felt the issue was still very sensitive in their business community. They did not want to spark additional discourse by participating in the survey. Evanston did not respond to our inquiries. None of the four communities wished to assist the Village in finding potential businesses to survey.

Business Surveys

As instructed, all interviews were conducted over the phone and were collected anonymously. An attempt was made to obtain an equal proportion of surveys from business categories in other communities as exist in Wilmette. For example, if 20% of the businesses in Wilmette are retailers, 20% of the survey responses would be from retailers. This proved to be difficult, as most businesses did not wish to participate in the survey.

Of the 70 businesses contacted only eight businesses agreed to be interviewed. Four were retailers, two were restaurants and two were professional businesses. Many questions were not answered because the business did not wish to answer the question or were not sure how to answer the question.

Minimum Wage Question Results

- Three retailers had less than 4 employees and were not subject to the ordinance
- One retailer already paid above minimum wage
- Two restaurants said they were impacted and increased prices, but did not want to quantify
- Two professional offices already paid above minimum wage

Sick Leave Question Results

- One business started providing paid sick leave
- Two businesses were unaware of the paid sick leave ordinance
- Four businesses were already providing paid sick leave
- One business did not respond to the question

Three businesses track sick time manually, three use computer software and one outsources this function. The one business who outsources tracking is a professional office with 22 employees. This business provided paid time and outsourced payroll prior to the Cook County ordinance.

When asked if the minimum wage and paid sick leave ordinances would be a factor in opening or relocating their business three of the eight businesses provided a response. Two businesses said it would not be a factor and one business said it would account for 5-10% of their decision.

Broker Survey

Three commercial real estate brokers were interviewed. Each of them have listings throughout the North Shore with a good understanding of local transactions. None of the brokers had heard the issue brought up when discussing locations with clients. They stated the most important factors tenants look for when selecting a location were the price of rent, physical location, and access to parking. They did not think the existence of the Cook County ordinances would have an impact in attracting or dissuading a tenant from locating in a particular community.



VILLAGE · OF · WINNETKA

Incorporated in 1869

Office of the Village President
(847) 716-3541

February 13, 2018

Mr. Bob Bielinski
Village President, Village of Wilmette
1200 Wilmette Avenue
Wilmette, Illinois 60091

Re: Minimum Wage & Paid Sick Leave Surveys

Dear Bob:

Village Staff recently received contact from the Village of Wilmette regarding participation in a survey about Cook County's Minimum Wage and Paid Sick Leave Ordinances, which became effective in 2017. As you know, the Village of Winnetka is currently complying with both ordinances and has not formally considered "opting-out" as some other nearby communities have recently done.

Staff was informed that an ad hoc committee in Wilmette wished to survey our staff about the impact of these ordinances; they also sought our assistance in contacting Winnetka businesses to participate in a separate survey on the topic. Unfortunately, we are not able to assist Wilmette in surveying about Cook County's ordinances at this time.

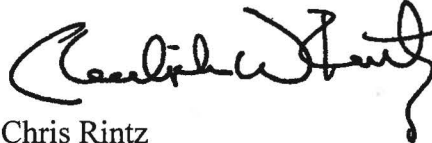
The Village of Winnetka has been engaged in ongoing and sensitive negotiations with Cook County for over a year related to our Stormwater Management Program and potential improvements to County Forest Preserve District property that may help us reach our flood mitigation goals. We have signed a Memorandum of Understanding (MOU) with the County, but discussions continue and the process is not nearly complete.

While I know that some of our local businesses have expressed concerns about impacts from the Minimum Wage and Paid Sick Leave Ordinances, the Village has been transparent to say that the time is not ripe for us to evaluate opting-out. Our business community has been supportive of the Stormwater Management Program, and in return, we have committed to evaluating the Ordinances after concluding our negotiations. Unfortunately, we do not have a set timeline as to when that will take place.

February 13, 2018
Page 2

If you have questions, please reach out to me directly to discuss. I appreciate your understanding of our flood mitigation priorities and the reasoning as to why we cannot participate or assist in your Committee's survey efforts at this time.

Sincerely,

A handwritten signature in black ink, appearing to read "Chris Rintz". The signature is fluid and cursive, with a large initial "C" and a long, sweeping tail.

Chris Rintz
Village President

cc: Robert M. Bahan, Village Manager
Megan E. Pierce, Assistant Village Manager
Michael Martella, Economic Development Coordinator
Terry Dason, Executive Director, Winnetka-Northfield Chamber of Commerce
Michael Braiman, Assistant Village Manager, Village of Wilmette
Lucas Sivertsen, Business Development Coordinator, Village of Wilmette

MEMORANDUM

To: Minimum Wage & Paid Sick Leave Working Group
Michael Braiman, Assistant Village Manager
John Prejzner, Assistant Director of Administrative Services

From: Gina Kennedy

Date: January 12, 2018

Re: Summary of Recent Tax Law Changes Affecting Businesses in
Wilmette

Introduction

This past December Congress passed, and the President signed into law, the "Tax Cuts and Jobs Act" (the "Act") which makes significant changes to the federal income tax laws effective, generally, January 1, 2018. Proponents for this legislation asserted that, by reducing the federal tax burden on businesses, including small businesses, businesses will have additional capital to hire more employees and increase existing employees' compensation. You have asked me to summarize those changes to federal tax law most likely to have a significant impact on businesses, particularly small businesses, in Wilmette.

Please keep in mind that the law is long and complex. (No one would claim that it *simplifies* anything!) This memorandum will address only certain widely applicable changes in tax law. There are, of course, hundreds of additional changes that will affect some taxpayers, possibly significantly, that are beyond the scope of this memorandum.

Be aware, too, that because of the size and scope of the Act, the speed with which it was drafted, the lack of public hearings, and the abbreviated consideration process, there are many provisions that are ambiguous or conflicting, and others that will need further legislation to correct omissions or errors that make them hard to interpret or difficult to apply. As yet, there has been no guidance from the Internal Revenue Service, the Treasury Department or from the Congressional committees responsible for crafting the legislation that might clarify or elucidate the Act's provisions. Moreover, tax professionals have had very little time since the Act's passage to digest it thoroughly. Accordingly, no one can now state definitively what impact the Act will have on taxpayers, generally or specifically.

Finally, please keep in mind that, no two taxpayers are exactly alike. Each will be impacted slightly differently by the Act. In this memorandum I have

attempted to summarize how a *hypothetical* "typical" small business in Wilmette will likely be affected by the Act; no *actual* business or individual should rely on what is said in this memorandum.

This memorandum has been prepared for informational purposes only, and is not intended to provide, and should not be relied upon, for tax, legal or accounting advice. Every business entity and individual should consult with its, or his or her, own tax, legal and accounting advisors.

Changes Affecting "C Corporations"

How a U.S. business is taxed, under the Internal Revenue Code (the "Code"), depends on how it is organized. Typically, a business is organized as a sole proprietorship, partnership, limited liability company or corporation.ⁱ In this memorandum, businesses organized as partnerships, limited liability companies or corporations that have elected to be taxable under chapter S of the Code ("S Corporations") will be referred to as "Pass-Through Entities." Corporations that are generally taxable under chapter C of the Code will be referred to in this memorandum as "C Corporations." This section of the memorandum addresses the impact of the Act on C Corporations for tax years beginning on or after January 1, 2018.

Prior to the Act, C Corporations, like individual taxpayers, were subject to federal income tax at graduated rates. The rate of tax increased as the corporation's taxable income increased, with the top marginal rate being 35%. The Act replaced these graduated rates with a flat rate of 21%. The Act also repealed the corporate alternative minimum tax ("AMT") which was intended to insure that all corporations, regardless of their eligibility to claim certain deductions, exemptions, etc., pay at least a minimum amount of tax. This reduction in the rate of tax generally applies, with some exceptions, to all C Corporations regardless of the business in which they are engaged.ⁱⁱ These changes to the Code are permanent.ⁱⁱⁱ

The Act also permits C Corporations to deduct in full capital investments in property (except as noted below), whether new or used, on or after January 1, 2018. Prior to the Act, such businesses were subject to depreciation rules that effectively spread the deductibility of such expenses over a period of years following the acquisition of property. Being able to take the full amount of a capital expenditure into account up front is considerably more favorable to the taxpayer than spreading the expense over a period of years. These changes do *not* apply to investments in real estate, including investments in leasehold improvements. Under the Act, these changes will be phased out beginning in 2023 and the prior depreciation rules will again be in effect.

Another significant, widely applicable change involves a new limitation on the deductibility of interest by some C Corporations. This limitation is not, however, applicable to corporations that derive their income principally from the performance of personal services or to small businesses (i.e., businesses whose average annual gross receipts over a three year period do not exceed \$25 million).

Finally, it is important to note that the Act does *not* limit the ability of C Corporations to deduct any state and local taxes paid in the course of their business. The Act's highly publicized limitations on the deductibility of state and local taxes apply solely to taxes, paid by individual taxpayers, that are unrelated to business activities.

Individual Taxpayers, Sole Proprietorships and Pass-Through Entities

The most widely publicized changes made by the Act are a reduction of the tax rates and rate brackets applicable to individual taxpayers. These changes apply not only to an individual taxpayer's wages, salaries, investments and retirement earnings, but also to an individual taxpayer's business income derived from a sole proprietorship or a Pass-Through Entity. Below are two charts reflecting the rates in effect for tax years beginning before January 1, 2018, and the new rates in effect for certain tax years beginning on or after January 1, 2018.^{iv}

Married Taxpayers, Filing Jointly

Rates for 2017

Rates for 2018 - 2025

10%	\$0 to 19,050		10%	\$0 to 19,050
15%	\$19,051 to 77,400		12%	\$19,051 to 77,400
25%	\$77,401 to 156,150		22%	\$77,401 to 165,000
28%	\$156,151 to 237,950		24%	\$165,001 to 315,000
33%	\$237,951 to 424,950		32%	\$315,001 to 400,000
35%	\$424,951 to 480,050		35%	\$400,001 to 600,000
39.6%	more than \$480,050		37%	more than \$600,000

Single Taxpayers

Rates for 2017

Rates for 2018 - 2025

10%	\$0 to 9,525		10%	\$0 to 9,525
15%	\$9,526 to 38,700		12%	\$9,526 to 38,700
25%	\$38,701 to 93,700		22%	\$38,701 to 82,500
28%	\$93,701 to 195,450		24%	\$82,501 to 157,500
33%	\$195,451 to 424,950		32%	\$157,501 to 200,000
35%	\$424,951 to 426,700		35%	\$200,001 to 500,000
39.6%	more than \$426,700		37%	more than \$500,000

These changes to tax rates for individuals, unlike the corresponding changes to C Corporation tax rates, are *not* permanent. They will expire December 31, 2025, and the brackets and higher rates in effect for 2017 will be reinstated unless new legislation is enacted extending these changes.

The Act also includes a new annual deduction from income equal to 20% of an individual taxpayer's "qualified business income" from a sole proprietorship or Pass-Through Entity, subject to some exceptions and limitations. This deduction is available whether or not the taxpayer otherwise claims the standard deduction or chooses to itemize his or her deductions on Schedule A. "Qualified business income," for this purpose, generally is income from a trade or business other than one which involves the performance of services in certain specified fields (for example, health care, law, accounting, business consulting, financial services, brokerage or investment management services and the like).^v

This new deduction is complicated and there are numerous limitations and exceptions that affect some taxpayers who derive "qualified business income" from a sole proprietorship or Pass-Through Entity. In particular, S Corporation shareholders may not be able to claim the deduction as readily as other taxpayers whose businesses are conducted through partnerships or limited liability companies.^{vi} In general, many of these limitations and exceptions are intended to prevent taxpayers from deducting any portion of their business income that is in the nature of compensation for their own services rendered to the business. A complete discussion of the twists and turns in this new deduction is beyond the scope of this memorandum.

Like the changes to tax rates for individuals, this 20% deduction of "qualified business income" is not permanent. It will expire December 31, 2025.

Finally, although the Act imposes a limit on an individual taxpayer's deduction of state and local taxes on Schedule A -- a provision that has attracted a good deal of attention in the press and will likely affect many Wilmette residents -- this limitation does *not* affect the ability of individuals doing business as a sole proprietorship or Pass-Through Entity to deduct any state and local taxes related to their business.

Miscellaneous Tax Changes

Obviously, the Act contains a multitude of other changes that will affect businesses of one type or another,^{vii} including a few businesses in Wilmette, but probably will not affect our *hypothetical* "typical" small business. One additional provision of the Act, however, warrants mention here. It affects the depreciation recovery period for certain capital investments made by restaurants and retail businesses. This provision affects all such businesses, whether they are C Corporations or Pass-Through Entities. Prior to the Act, these businesses' special recovery period for qualified leasehold improvements and qualified retail improvements (what people commonly call "build-out costs") was 15 years, which was shorter than the recovery period for most other leasehold improvements. The Act repeals this special rule, but does not specify clearly what recovery period applies in its stead. It is possible that the provision will be construed as requiring restaurants and retail establishments to recover costs incurred after December 31, 2017, over a period as long as 39 years. The change will not affect these businesses' recovery period for qualified leasehold improvements put in service before January 1, 2018. Nonetheless, this change could be detrimental to some businesses, particularly new ones. Unfortunately, there is too much uncertainty regarding this provision to say what its impact might be.

Effect of Federal Tax Law Changes on State Tax Obligations

Some of the changes discussed above will impact not only a business's federal tax liability but also its state tax liability. Changes in the federal tax rates don't have any effect on state taxes, but changes to the computation of federal adjusted net income or taxable income, often do. Illinois, like many states, piggy-backs off the federal tax computation of these amounts for both C Corporations and individual taxpayers. For a C Corporation, Illinois begins with the corporation's "taxable income" (currently, the figure on line 30 of the federal Form 1120). For an individual taxpayer, Illinois begins with his or her "adjusted gross income" (currently, the figure on line 37 of federal Form 1040).

Of the various federal tax law changes discussed above, the provision allowing C Corporations to expense certain investments in property (until December 31, 2025) and the provision affecting the depreciation of restaurant and retail leasehold improvements will affect these businesses' state tax liability - the former favorably, the latter unfavorably. Conversely, the provision allowing some individual taxpayers to deduct up to 20% of "qualified business income" from Pass-Through Entities (until January 1, 2025) will *not* affect their state tax liability, as this deduction is taken after the computation of "adjusted gross income" for federal purposes.

Summary

The changes in federal tax law effectuated by the Act are among the most wide-ranging in recent decades. Overall, they will result in a significant reduction in the federal tax obligations (and, in some cases, a corresponding reduction in the state tax obligations) of businesses and their owners. While these benefits are not evenly distributed across businesses -- larger businesses clearly stand to gain more than smaller ones, in general, and certain classes of businesses are favored over others -- most businesses and their owners will realize a substantial increase in their after-tax income. As such, the effect of these changes has a bearing on the issues under consideration by your committee and the Wilmette Village Board.

ⁱ Federal tax law recognizes sub-sets of these organizations -- such as REITs and RICs -- and subjects them to special treatment for tax purposes. I will not be addressing these sorts of businesses in this memorandum.

ⁱⁱ To give a sense of how significant this rate reduction is, the Congressional Budget Office estimated that this provision alone will reduce federal tax revenues over the next ten years by \$1.35 trillion.

iii "Permanent," in this context, means that the Act does not impose a sunset date on these changes as it does in the case of other changes in the Code, most notably the changes made to the taxation of individual taxpayers and Pass-Through Entities. It does *not* mean that the changes cannot be repealed or modified by future legislation.

iv I have set out only the rates for single taxpayers and for married taxpayers filing jointly, as these are the most common filing statuses. The Act made similar changes to the rates applicable to other taxpayers.

v The deduction may apply in some cases, however, to income from a service trade or business if the taxpayer's taxable income does not exceed \$315,000 (for married individuals filing jointly) or \$157,500 (for other individuals).

vi It is unclear whether this was intentional on the part of the Act's authors. It may have been a drafting error that could be corrected in subsequent legislation.

vii In particular, there are significant changes directed at businesses like financial institutions, financial investors and managers, real estate investors, insurance companies, large manufacturers, and businesses with significant foreign source income or operations abroad.

MINIMUM WAGE & PAID SICK LEAVE STUDY*Village of Wilmette, Illinois**4/11/2018 - 4/14/2018**N=303, +/- 5.62%**Adults 18 Years of Age or Older**(percentages may not add up to 100% due to rounding)*

www.FallonResearch.com

Q. 1. Would you say that the Village of Wilmette is going in the right direction, or has it gotten off onto the wrong track?

67.8% Right direction
 15.3 Wrong track
 4.4 Mixed/both (volunteered)
 12.5 Unsure/no answer

Turning to a specific topic...

Q. 2. As you may or may not know, the minimum wage established by the State of Illinois that businesses in the Village of Wilmette are required to pay employees is \$8.25 per hour. Do you think that this amount is too high, generally pretty fair or too low?

2.6% Too high
 27.1 Pretty fair
 65.7 Too low
 4.6 Unsure/no answer

Q. 3. How much news and information have you heard, read or seen about a minimum wage ordinance that Cook County has adopted, which raised the minimum wage, so that it will gradually go up to \$13 per hour by the year 2020? Have you heard, read or seen a lot of news and information about it, some news and information or nothing at all?

18.8% A lot
 56.1 Some
 23.7 Nothing
 1.4 Unsure/no answer

Q. 4. The Cook County ordinance requires local businesses to increase the minimum wage for most employees except those in businesses with less than 4 employees, teens under age 18, trainees during their first 90 days, and independent contractors. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?

66.4% Should follow
 26.7 Should not
 6.9 Unsure/no answer

Q. 5. How much news and information have you heard, read or seen about a new paid sick leave

ordinance that Cook County has adopted, which requires businesses to provide most employees with up to 40 hours of paid sick leave each year? Have you heard, read or seen a lot of news and information about it, some news and information or nothing at all?

12.4% A lot
32.7 Some
54.2 Nothing
.7 Unsure/no answer

Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?

67% Should follow
24.6 Should not
8.4 Unsure/no answer

Q. 7. Were you aware that the Wilmette Village Board voted to exempt local businesses from Cook County's minimum wage and paid sick leave ordinances?

44.8% Yes, aware
54.4 No, not aware
.8 Unsure/no answer

Q. 8. Does knowing that the Village has chosen not to follow the Cook County ordinances, so businesses in Wilmette are not required to increase the minimum wage or offer paid sick leave to employees, make your opinion of the Village of Wilmette more favorable, less favorable or does it not affect your views?

15.2% More favorable
44.4 Less favorable
36.5 No effect
3.9 Unsure/no answer

(ROTATED NEXT 2 QUESTIONS)

Q. 9. Since research shows that restaurant prices tend to increase following a raise in the minimum wage, would you be more or less likely to patronize Wilmette restaurants if the minimum wage is increased or does it have no effect on your decision?

10.3% More likely
12.3 Less likely
74.5 No effect
2.9 Unsure/no answer

Q. 10. Since research shows that, in order to avoid losing wages, nearly half of food service

employees who are ill come to work sick if they don't have paid sick leave, would you be more or less likely to patronize Wilmette restaurants if local restaurants provide paid sick leave or does it have no effect on your decision?

32.3% More likely
8.4 Less likely
55.6 No effect
3.7 Unsure/no answer

Finally, I have a few short questions for statistical purposes...

Q. 11. I would like to read you a list of age groups. Please stop me when I get to the one you are in.

30.7% 18 to 44
36.4 45 to 64
31.9 65 and older
1 Unsure/no answer

Q. 12. Do you have any children under 18 years of age in your household?

42.7% Yes
55.5 No
1.7 Unsure/no answer

Gender:

47.2% Male
52.8 Female

		GENDER:		AGE GROUP:				Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 1. Would you say that the Village of Wilmette is going in the right direction, or has it gotten off onto the wrong track?	Right direction	69.7%	66.1%	70.7%	65.6%	66.4%	100.0%	67.8%
	Wrong track	13.6%	16.8%	10.2%	18.1%	17.5%	.0%	15.3%
	Mixed/both	4.7%	4.2%	2.4%	8.2%	2.2%	.0%	4.4%
	DK/NA	12.0%	12.9%	16.7%	8.0%	14.0%	.0%	12.5%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 1. Would you say that the Village of Wilmette is going in the right direction, or has it gotten off onto the wrong track?	Right direction	74.1%	62.6%	80.0%	67.2%	75.7%	67.8%
	Wrong track	11.9%	18.4%	.0%	15.4%	14.6%	15.3%
	Mixed/both	5.3%	3.3%	20.0%	4.8%	.0%	4.4%
	DK/NA	8.8%	15.8%	.0%	12.7%	9.7%	12.5%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 2. Do you think that this amount is too high, generally pretty fair or too low?	Too high	4.0%	1.3%	3.2%	3.4%	1.1%	.0%	2.6%
	Pretty fair	33.1%	21.8%	28.0%	29.9%	21.8%	66.7%	27.1%
	Too low	56.8%	73.7%	61.8%	64.7%	71.6%	33.3%	65.7%
	DK/NA	6.1%	3.2%	7.0%	1.9%	5.5%	.0%	4.6%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 2. Do you think that this amount is too high, generally pretty fair or too low?	Too high	3.6%	1.9%	.0%	2.8%	.0%	2.6%
	Pretty fair	30.5%	24.2%	40.0%	25.8%	43.8%	27.1%
	Too low	62.8%	68.7%	40.0%	66.4%	56.2%	65.7%
	DK/NA	3.1%	5.3%	20.0%	4.9%	.0%	4.6%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 3. How much news and information have you heard, read or seen about a minimum wage ordinance that Cook County has adopted, which raised the minimum wage, so that it will gradually go up to \$13 per hour by the year 2020?	A lot	21.2%	16.6%	22.3%	21.6%	12.9%	.0%	18.8%
	Some	52.8%	59.0%	50.0%	65.1%	51.3%	66.7%	56.1%
	Nothing	23.7%	23.7%	25.3%	12.4%	34.7%	33.3%	23.7%
	DK/NA	2.3%	.7%	2.4%	1.0%	1.1%	.0%	1.4%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 3. How much news and information have you heard, read or seen about a minimum wage ordinance that Cook County has adopted, which raised the minimum wage, so that it will gradually go up to \$13 per hour by the year 2020?	A lot	24.8%	14.2%	20.0%	19.6%	8.5%	18.8%
	Some	56.5%	55.6%	60.0%	55.4%	64.6%	56.1%
	Nothing	17.9%	28.2%	20.0%	23.4%	26.9%	23.7%
	DK/NA	.8%	2.0%	.0%	1.6%	.0%	1.4%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		Q. 2. Do you think that this amount is too high, generally pretty fair or too low?				Q. 3. How much news and information have you heard, read or seen about a minimum wage ordinance that Cook County has adopted, which raised the minimum wage, so that it will gradually go up to \$13 per hour by the year 2020?				Table Total
		Too high	Pretty fair	Too low	DK/NA	A lot	Some	Nothing	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 4. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?	Should follow	.0%	31.8%	86.3%	23.8%	60.9%	66.7%	69.6%	75.8%	66.4%
	Should not	86.5%	62.3%	7.9%	52.0%	35.7%	28.2%	17.6%	.0%	26.7%
	DK/NA	13.5%	5.9%	5.8%	24.2%	3.3%	5.1%	12.8%	24.2%	6.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 4. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?	Should follow	59.4%	72.7%	67.2%	63.9%	68.6%	66.7%	66.4%
	Should not	35.5%	18.8%	29.3%	31.7%	18.3%	33.3%	26.7%
	DK/NA	5.1%	8.4%	3.5%	4.4%	13.1%	.0%	6.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 4. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?	Should follow	65.5%	68.0%	40.0%	66.5%	65.3%	66.4%
	Should not	31.6%	22.5%	40.0%	26.5%	29.8%	26.7%
	DK/NA	2.8%	9.5%	20.0%	7.0%	4.9%	6.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 5. How much news and information have you heard, read or seen about a new paid sick leave ordinance that Cook County has adopted, which requires businesses to provide most employees with up to 40 hours of paid sick leave each year?	A lot	15.4%	9.7%	15.6%	17.2%	4.4%	.0%	12.4%
	Some	29.5%	35.5%	27.7%	37.8%	31.7%	33.3%	32.7%
	Nothing	54.4%	54.1%	56.7%	45.1%	61.8%	66.7%	54.2%
	DK/NA	.7%	.7%	.0%	.0%	2.2%	.0%	.7%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 5. How much news and information have you heard, read or seen about a new paid sick leave ordinance that Cook County has adopted, which requires businesses to provide most employees with up to 40 hours of paid sick leave each year?	A lot	19.0%	7.1%	20.0%	12.4%	12.3%	12.4%
	Some	36.9%	29.8%	20.0%	33.6%	21.1%	32.7%
	Nothing	44.1%	61.8%	60.0%	53.2%	66.6%	54.2%
	DK/NA	.0%	1.3%	.0%	.8%	.0%	.7%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		Q. 5. How much news and information have you heard, read or seen about a new paid sick leave ordinance that Cook County has adopted, which requires businesses to provide most employees with up to 40 hours of paid sick leave each year?				Table Total
		A lot	Some	Nothing	DK/NA	
		Col %	Col %	Col %	Col %	Col %
Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?	Should follow	43.5%	70.1%	71.3%	.0%	67.0%
	Should not	54.2%	24.6%	16.9%	100.0%	24.6%
	DK/NA	2.2%	5.3%	11.8%	.0%	8.4%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:			Table Total	
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?	Should follow	59.6%	73.6%	72.6%	59.2%	70.5%	66.7%	67.0%
	Should not	33.2%	16.9%	23.7%	32.4%	16.4%	33.3%	24.6%
	DK/NA	7.2%	9.5%	3.8%	8.4%	13.1%	.0%	8.4%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?	Should follow	66.8%	67.3%	60.0%	67.0%	66.3%	67.0%
	Should not	28.5%	21.1%	40.0%	24.6%	24.9%	24.6%
	DK/NA	4.7%	11.5%	.0%	8.4%	8.7%	8.4%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		Q. 3. How much news and information have you heard, read or seen about a minimum wage ordinance that Cook County has adopted, which raised the minimum wage, so that it will gradually go up to \$13 per hour by the year 2020?				Q. 5. How much news and information have you heard, read or seen about a new paid sick leave ordinance that Cook County has adopted, which requires businesses to provide most employees with up to 40 hours of paid sick leave each year?				Table Total
		A lot	Some	Nothing	DK/NA	A lot	Some	Nothing	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 7. Were you aware that the Wilmette Village Board voted to exempt local businesses from Cook County's minimum wage and paid sick leave ordinances?	Yes, aware	80.3%	46.1%	14.8%	24.2%	84.8%	67.9%	22.3%	.0%	44.8%
	No, not aware	19.7%	52.6%	85.2%	75.8%	15.2%	32.1%	76.9%	50.0%	54.4%
	DK/NA	.0%	1.4%	.0%	.0%	.0%	.0%	.8%	50.0%	.8%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		Q. 4. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?			Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?			Table Total
		Should follow	Should not	DK/NA	Should follow	Should not	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 7. Were you aware that the Wilmette Village Board voted to exempt local businesses from Cook County's minimum wage and paid sick leave ordinances?	Yes, aware	44.9%	51.0%	19.3%	42.6%	57.1%	26.5%	44.8%
	No, not aware	55.1%	47.7%	74.7%	57.4%	41.5%	68.6%	54.4%
	DK/NA	.0%	1.3%	6.0%	.0%	1.4%	4.9%	.8%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:			Table Total	
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 7. Were you aware that the Wilmette Village Board voted to exempt local businesses from Cook County's minimum wage and paid sick leave ordinances?	Yes, aware	42.4%	46.9%	45.7%	56.1%	31.4%	33.3%	44.8%
	No, not aware	56.0%	53.1%	53.0%	43.9%	67.5%	66.7%	54.4%
	DK/NA	1.6%	.0%	1.3%	.0%	1.1%	.0%	.8%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 7. Were you aware that the Wilmette Village Board voted to exempt local businesses from Cook County's minimum wage and paid sick leave ordinances?	Yes, aware	56.7%	35.8%	40.0%	46.3%	24.7%	44.8%
	No, not aware	43.3%	62.8%	60.0%	52.8%	75.3%	54.4%
	DK/NA	.0%	1.4%	.0%	.8%	.0%	.8%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		Q. 4. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?			Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?			Table Total
		Should follow	Should not	DK/NA	Should follow	Should not	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 8. Does knowing that the Village has chosen not to follow the Cook County ordinances, so businesses in Wilmette are not required to increase the minimum wage or offer paid sick leave to employees, make your opinion of the Village of Wilmette more...	More favorable	3.3%	45.8%	10.2%	4.3%	48.8%	3.3%	15.2%
	Less favorable	61.2%	10.3%	15.3%	60.6%	6.4%	27.1%	44.4%
	No effect	33.9%	40.0%	48.5%	32.1%	43.3%	52.2%	36.5%
	DK/NA	1.6%	3.9%	26.1%	3.1%	1.4%	17.4%	3.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 8. Does knowing that the Village has chosen not to follow the Cook County ordinances, so businesses in Wilmette are not required to increase the minimum wage or offer paid sick leave to employees, make your opinion of the Village of Wilmette more...	More favorable	20.9%	10.0%	11.6%	22.3%	10.9%	.0%	15.2%
	Less favorable	39.9%	48.5%	47.9%	42.9%	43.2%	33.3%	44.4%
	No effect	36.2%	36.9%	38.2%	32.8%	39.3%	33.3%	36.5%
	DK/NA	3.0%	4.6%	2.4%	1.9%	6.6%	33.3%	3.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 8. Does knowing that the Village has chosen not to follow the Cook County ordinances, so businesses in Wilmette are not required to increase the minimum wage or offer paid sick leave to employees, make your opinion of the Village of Wilmette more...	More favorable	16.4%	14.0%	20.0%	15.0%	17.2%	15.2%
	Less favorable	47.1%	43.1%	20.0%	44.6%	42.8%	44.4%
	No effect	35.7%	37.1%	40.0%	36.3%	40.0%	36.5%
	DK/NA	.8%	5.8%	20.0%	4.2%	.0%	3.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		Q. 4. In general, do you think that the Village of Wilmette should or should not follow the Cook County Minimum Wage Ordinance?			Table Total
		Should follow	Should not	DK/NA	
		Col %	Col %	Col %	Col %
Q. 9. Since research shows that restaurant prices tend to increase following a raise in the minimum wage, would you be more or less likely to patronize Wilmette restaurants if the minimum wage is increased or does it have no effect on your decision?	More likely	12.9%	5.4%	5.1%	10.3%
	Less likely	4.5%	33.4%	5.1%	12.3%
	No effect	81.7%	56.3%	75.9%	74.5%
	DK/NA	.9%	4.9%	13.9%	2.9%
Table Total		100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:			Table Total	
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 9. Since research shows that restaurant prices tend to increase following a raise in the minimum wage, would you be more or less likely to patronize Wilmette restaurants if the minimum wage is increased or does it have no effect on your decision?	More likely	6.8%	13.6%	7.0%	14.1%	9.6%	.0%	10.3%
	Less likely	14.9%	9.9%	16.4%	15.1%	4.4%	33.3%	12.3%
	No effect	73.6%	75.3%	74.4%	68.5%	81.7%	66.7%	74.5%
	DK/NA	4.7%	1.3%	2.1%	2.3%	4.4%	.0%	2.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 9. Since research shows that restaurant prices tend to increase following a raise in the minimum wage, would you be more or less likely to patronize Wilmette restaurants if the minimum wage is increased or does it have no effect on your decision?	More likely	10.9%	10.3%	.0%	10.5%	8.7%	10.3%
	Less likely	14.1%	10.0%	40.0%	12.6%	7.7%	12.3%
	No effect	72.2%	76.7%	60.0%	74.1%	79.7%	74.5%
	DK/NA	2.8%	3.0%	.0%	2.8%	3.9%	2.9%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		Q. 6. In general, do you think the Village of Wilmette should or should not follow the Cook County Ordinance requiring local businesses to provide paid sick leave annually to most employees?			Table Total
		Should follow	Should not	DK/NA	
		Col %	Col %	Col %	Col %
Q. 10. Since research shows that, in order to avoid losing wages, nearly half of food service employees who are ill come to work sick if they don't have paid sick leave, would you be more or less likely to patronize Wilmette restaurants if local...	More likely	43.1%	6.7%	20.8%	32.3%
	Less likely	8.8%	6.4%	11.6%	8.4%
	No effect	44.7%	85.5%	55.2%	55.6%
	DK/NA	3.4%	1.4%	12.5%	3.7%
Table Total		100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %
Q. 10. Since research shows that, in order to avoid losing wages, nearly half of food service employees who are ill come to work sick if they don't have paid sick leave, would you be more or less likely to patronize Wilmette restaurants if local...	More likely	23.1%	40.5%	35.2%	34.4%	28.2%	.0%	32.3%
	Less likely	9.9%	7.1%	4.8%	9.6%	10.9%	.0%	8.4%
	No effect	62.8%	49.2%	57.8%	55.3%	52.4%	100.0%	55.6%
	DK/NA	4.2%	3.1%	2.1%	.8%	8.5%	.0%	3.7%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %
Q. 10. Since research shows that, in order to avoid losing wages, nearly half of food service employees who are ill come to work sick if they don't have paid sick leave, would you be more or less likely to patronize Wilmette restaurants if local...	More likely	33.9%	32.0%	.0%	33.1%	22.1%	32.3%
	Less likely	4.8%	10.9%	20.0%	8.8%	3.9%	8.4%
	No effect	58.5%	52.7%	80.0%	54.2%	74.1%	55.6%
	DK/NA	2.8%	4.4%	.0%	3.9%	.0%	3.7%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		AGE GROUP:				CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		18 to 44	45 to 64	65 and older	DK/NA	Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %
GENDER:	Male	47.9%	53.2%	38.0%	100.0%	46.6%	46.0%	100.0%	46.2%	60.3%	47.2%
	Female	52.1%	46.8%	62.0%	.0%	53.4%	54.0%	.0%	53.8%	39.7%	52.8%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			VOTING STATUS:		Table Total
		Male	Female	Yes	No	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %
AGE GROUP:	18 to 44	31.1%	30.3%	50.1%	16.7%	.0%	30.5%	32.2%	30.7%
	45 to 64	41.0%	32.2%	48.6%	26.9%	40.0%	35.1%	53.3%	36.4%
	65 and older	25.7%	37.5%	1.3%	56.5%	.0%	33.2%	14.6%	31.9%
	DK/NA	2.2%	.0%	.0%	.0%	60.0%	1.1%	.0%	1.0%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				VOTING STATUS:		Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	Registered to vote	Not registered to vote	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %
CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:	Yes	42.2%	43.2%	69.8%	57.1%	1.7%	.0%	40.6%	70.8%	42.7%
	No	54.1%	56.8%	30.2%	41.0%	98.3%	.0%	57.6%	29.2%	55.5%
	DK/NA	3.7%	.0%	.0%	1.9%	.0%	100.0%	1.9%	.0%	1.7%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

		GENDER:		AGE GROUP:				CHILDREN UNDER 18 WITHIN THE HOUSEHOLD:			Table Total
		Male	Female	18 to 44	45 to 64	65 and older	DK/NA	Yes	No	DK/NA	
		Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %	Col %
VOTING STATUS:	Registered to vote	90.8%	94.6%	92.5%	89.5%	96.7%	100.0%	88.1%	96.2%	100.0%	92.8%
	Not registered to vote	9.2%	5.4%	7.5%	10.5%	3.3%	.0%	11.9%	3.8%	.0%	7.2%
Table Total		100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%

REPORT ON INVESTIGATION OF RESIDENT SURVEY INCIDENT

A recent incident raised doubts about the fairness of the telephone survey recently conducted by the Village's Minimum Wage and Paid Sick Days Working Group. The survey was conducted over three days -- April 11, 12, and 13 -- by Fallon Research. It was intended to elicit residents' input concerning the question underlying the work of the Working Group -- whether the Village Board should "opt-in" to Cook County's Minimum Wage and Paid Sick Days Ordinances. This memorandum states the facts that Michael Braiman, Assistant Village Manager, and I gathered during our investigation of this incident. After this statement of facts, I set forth my (not Mr. Braiman's) conclusions.

THE FACTS

The incident involved a telephone call made by a Fallon Research pollster to a Wilmette resident. The details of the call are not in dispute. After an introductory statement, the pollster asked the resident, "Would you say that the Village of Wilmette is going in the right direction, or has it gotten off onto the wrong track?" Of several choices listed by the pollster, the resident selected, "Wrong track". The pollster then responded that the survey was over. The resident challenged the pollster, "What do you mean it's over? I thought you were going to ask me questions." The pollster explained simply that "we've filled our quota." The resident persisted, "If I had answered Wilmette is headed in the right direction, would you have more questions for me? Is it over because I said Wilmette is headed in the wrong direction?" The pollster then hung up.

The resident reasonably concluded, based on this exchange, that her "wrong track" answer was the reason for the discontinuation of the survey. And if some or all "wrong track" respondents were excluded from the survey process, the results might be flawed - - biased in favor of residents who think the Village is going in the "right direction" and against those who believe it's on the "wrong track".

This incident was brought to my attention by Mr. Braiman on Thursday, April 19 and again by a resident (not the one involved in the call) on Friday, April 20. We decided to investigate promptly by speaking to Paul Fallon himself to get the details of the survey process.

Our telephone conversation with Paul Fallon occurred on Monday, April 23. A summary of the conversation that I prepared immediately thereafter is as follows:

"The last day of interviewing was April 14. Paul wasn't exactly sure of the time of day when the survey ended and said he doubted that he could recover this information.

"As the process drew to a close, a group of interviewers were speaking to respondents. At some point, the manager of the process observed that the quota of 300 respondents had been met, and he/she stopped further interviewing. The surveys of two people were terminated mid-stream because the quota of 300 had been reached. Paul would not identify these respondents or their phone numbers, citing the promise of confidentiality. Before the manager was able to totally effectuate the shutdown, a total of 303 interviews had actually been completed.

"The first question on the survey was asked solely for three reasons: (1) to ease respondents into the process of responding to the questions; (2) to help weed out respondents who were not Wilmette residents; and (3) to help interviewers identify whether the respondent was a male or female (there was a quota for each gender, based on Wilmette's demographic composition). Four interviews of male respondents were terminated mid-stream during the interview process because the quota for males was reached before the quota for females was reached. Again, Paul declined to disclose their names or phone numbers.

"Paul insisted repeatedly that no survey was terminated mid-stream based on the respondent's answer to the first question, and that the survey results are valid and he stands behind them.

"Paul agreed to see if he could determine the time when the survey was cut off and how that process was handled by the manager. He also said he would check whether he would be available for the Village Board's May 15 meeting."

Later in the same day, Mr. Fallon phoned Mr. Braiman and related the following additional information: First, the pollsters are not made aware of the specific quota being met (male/female or # of respondents); thus, they are unable to inform the resident of the reason for terminating the call (the computer simply tells them a quota has been met and new or newly initiated surveys are stopped by the supervisor). Second, they try not to exceed 1% of the number of surveys paid for (in this instance, the Village paid for 300, and 1% is 303). Third, Mr. Fallon is not available to attend the May 15th Village Board meeting.

The resident who initially contacted me about this situation stated that she could furnish me the name and contact information for the resident involved in this incident, but added, "They only ask that their identity be kept confidential." I responded that I would appreciate receiving the contact information for purposes of our investigation, and I agreed that "I will respect the person's concern about confidentiality." The name and contact information were then furnished to me.

I contacted the resident on April 23. She was cooperative and forthcoming. After several back-and-forth emails, she and I identified the date, time, duration, and originating

telephone number of the survey call, based on her phone records. The phone call occurred on April 13 at 5:00 p.m. and lasted for 73 seconds (which is consistent with the time that would be required for the pollster to read the opening statement and for the pollster and the resident to have the brief conversation summarized above). According to the resident's phone records, the originating phone number was 847-519-2002 and came from Hoffman Estates.

As stated above, Mr. Fallon declined to furnish information about specific calls during our conversation on April 23. He insisted that providing this information would violate the confidentiality pledge made to respondents at the outset of the calls and would also violate the ethics of the polling profession. However, Mr. Braiman and I continued to press for information that, in our opinion, was not confidential, and Mr. Fallon ultimately divulged (on April 26) that the survey ended in the afternoon of April 13 (not on April 14 as originally stated) and that 847-519-2002 was one of three originating numbers used by Fallon Research to make the calls. Prior to Mr. Fallon's providing this supplemental information on April 26, neither Mr. Braiman nor I furnished him information we had learned from the resident about the date, time, and originating number of the call.

CONCLUSIONS

1. Fallon Research's procedure of terminating survey interviews mid-stream without explanation upon reaching a quota did not serve the Village well. In this case, the interviewee and other residents who became aware of the incident reasonably concluded that the survey was terminated based on the interviewee's answer to the first question. That conclusion logically leads to questions about possible bias of the survey. I feel compelled to make this comment about Fallon Research: This firm was representing to interviewees that they "have been retained by the Village of Wilmette to conduct a public survey". It should have been more sensitive to how residents might react to a mid-stream, unexplained termination of the survey and how this action might affect residents' opinion of the Village and the survey. I was quite disappointed by this aspect of Fallon Research's performance.
2. Despite this, I believe Fallon Research's explanation that the survey was terminated because the quota was reached, not because of the respondent's "wrong track" response. Significantly, the call involved in this incident occurred late in the afternoon on the final day of the survey, and this fact supports Fallon Research's explanation that the interview was terminated because the "quota" was reached. While I wish that Fallon Research would have provided Mr. Braiman and me with more detailed information regarding the name, phone number, date, and time of surveys terminated mid-stream because of the quota, I somewhat understand their confidentiality and ethics concerns. (Mr. Braiman and I did not

request information about any respondent's *answers* to questions, as *this information would clearly be confidential.*)

3. Other factors point to the conclusion set forth in paragraph 2:
 - a. The explanation of Fallon Research for asking the "right direction/wrong track" question is plausible -- the question is a device that facilitates the interview process. No importance is attached to respondents' answers to this question, and a "right direction" answer was not a criterion for proceeding to the balance of the survey questions. Indeed, 15.3% of respondents gave the "wrong track" answer, but the pollsters continued and completed their interviews. Moreover, the 15.3% "wrong track" response rate was consistent with a survey conducted by the Village late last year in which 14.1% of respondents gave the "wrong track" answer to the same question. This similar rate of response indicates that "wrong track" respondents were not excluded from the second survey, and they participated in accordance with their rate of representation in the community.
 - b. Fallon Research was well-aware of the Working Group's desire for an objective, neutral, valid survey. This was emphasized to Mr. Fallon repeatedly by Mr. Braiman and me as the Working Group framed the questions, and he himself emphasized it by pushing back against the wording of some of the questions submitted by the Working Group for inclusion in the survey. The idea that Fallon Survey would conduct the survey in a manner that might undermine this goal makes no sense. Fallon Research has a professional reputation to protect and had no reason to put it in jeopardy.
 - c. Neither Fallon Research nor the Working Group had any reason to know whether excluding "wrong direction" respondents would tilt the survey results one way or another. In other words, if Fallon Research were trying to slant the outcome of the survey, excluding "wrong track" respondents would be a highly uncertain way to accomplish this. Consider these possibilities: Some respondents who favor "opting in" to the County minimum wage and paid sick days ordinances may have given the "wrong track" answer because they disagree with the Village Board's earlier decision to "opt out" of the County Ordinances, while other respondents who oppose "opting in" may have given the "wrong track" answer because the Village is now considering whether to "opt in".

- d. Finally, no other residents have come forward to report similar incidents, even though this incident received widespread publicity.

John Jacoby, Chair, Working Group

From: [Jeff Lehn](#)
To: [Braiman, Michael](#)
Subject: Re: Village of Wilmette Minimum Wage and Paid Sick Leave Working Group
Date: Monday, March 5, 2018 3:47:59 PM

Dear Michael,

Good afternoon! I hope you are well on this overcast Monday. I'm writing in response to John Prejzner's email of last week about the work of the Wilmette Minimum Wage and Paid Sick Leave Working Group. Thanks for reaching out to Wilmette clergy for input. Please let me know if I can be of any further help to the Working Group or if you'd like me to elaborate on anything I've shared.

My name is Jeff Lehn and I'm a Presbyterian pastor serving First Presbyterian Church (located at the corner of 9th and Greenleaf). I've only been in my role of senior pastor for a year, but have already come to love and appreciate our community a great deal.

First, let me thank you for serving on this Work Group. I'm grateful that, even though the Village Board voted to opt out of Cook County's minimum wage and sick leave provisions last year, we are now going to be studying it.

I'm not a policymaker and don't pretend to understand the full range of issues involved in raising the minimum wage or offering paid sick leave for local businesses. But I do speak from the perspective of my Christian faith, which calls for abundant life for all God's children. Where all have enough to reach their potential, take care of their family, share their gifts with the world, make it a more loving, kind and hopeful place. I know it's the desire of each one of you on this Working Group. I also know it's the desire of employees who work in local businesses making the current minimum wage and without paid sick days.

We are all blessed beyond measure to live in a community like Wilmette with tremendous wealth and resources. In 2015, the annual median household income in the Village was \$132,110. In contrast, people who work fulltime at the level of the current minimum wage make only \$17,000 per year. (As you know, minimum wage laws across the country have lagged way behind inflation over the past 50 years.) While it is financially impossible for most of these workers to live in our community, they still work in local businesses and deserve to be valued and supported by all of us.

My hope is that the Village Board changes its mind and courageously opts back into the Cook County minimum wage and paid sick leave changes. We would be setting a compelling example on the North Shore. And other communities may feel pressure to follow suit. Instead of conforming to the decisions of our neighbors, we ought to step out boldly. Why not us?

Mother Teresa once said that the problem with the world is that we've forgotten we belong to one another. I think she was right. Our call is to see ourselves in relationship with all of God's children, including those struggling to get by, working two jobs making minimum wage just to pay the rent and put food on the table each month. Raising wages is the most effective way to end hunger and reduce poverty.

As a Christian pastor, I could cite many passages from our scriptures and theological tradition about the call to support the last, the lost and the least among us. From the prophet Isaiah, the teachings of Jesus, and so on. But I'll refrain from doing so (for the sake of brevity) and just share that I think we are ultimately judged by how we treat, care for and remunerate our most vulnerable citizens.

Opting back into the Cook County changes will not usher in utopia. But it will help a little bit. It will put more money in the pockets of the poor in our community and surrounding communities. For a single mom raising two children it will make a big difference. And it will make a powerful symbolic statement that the Village of Wilmette wants to do better.

You have my prayers for wisdom, courage and hope in the days ahead. Please let me know if I can be of any further help. Again, thanks for reaching out and for reading my comments.

With gratitude and lots of hope,
Jeff

Jeffrey Lehn
Pastor
First Presbyterian Church of Wilmette
600 Ninth Street
Wilmette, IL 60091
jlehn@fpcw.org
847.256.3010 x12

I always hope to return emails in a timely fashion, but please know my Sabbath is Friday and I typically don't respond to email on that day or on Saturday.

From: "Prejzner, John" <prejznerj@wilmette.com>
Date: Wednesday, February 28, 2018 at 1:39 PM
To: "Braithman, Michael" <braithmanm@wilmette.com>
Subject: Village of Wilmette Minimum Wage and Paid Sick Leave Working Group

Good Afternoon,

Attached is a letter on behalf of the Wilmette Minimum Wage and Paid Sick Leave Working Group. The working group is seeking information that will assist the Village Board in making policy decisions regarding minimum wages and sick days for employees of Wilmette businesses. The Working Group is seeking your input about the proposed changes which are outlined in the attached info table.

To provide any comments you may have please contact Michael Braithman at (847) 853-7506 or braithmanm@wilmette.com.

Thank you,

John Prejzner
Assistant Director of Administrative Services
(847) 853-7502

From: [Sam Gordon](#)
To: [Braidman, Michael](#); [John Jacoby](#); bfabes@ccachicago.org
Subject: Minimum wage and paid sick leave
Date: Wednesday, February 28, 2018 1:57:06 PM

Dear Mr. Braidman:

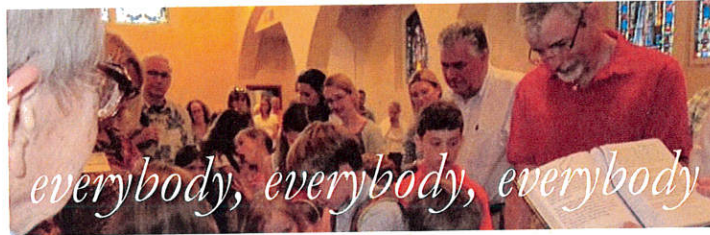
As a Wilmette resident of almost 38 years, and as a Rabbi in this community, I am writing to support the Cook County ordinance regarding minimum wage and paid sick leave. I believe that Wilmette should not “opt out” of this ordinance. My own faith tradition throughout the Hebrew Bible instructs us to care for the laborer and worker. We are commanded to treat others with dignity and insure equality and justice.

I believe that it is incumbent upon us to provide a living wage that is adequate to provide for working families. In addition, one should not need to choose between one’s health and the ability to financially provide for one’s family. I very much value the unique qualities of the Wilmette community. Those who work in our Village are not strangers. They are part of our Village. I would urge the Board to adopt the Cook County ordinance and apply it to all those who work within Wilmette.

Thank you for your consideration.

Sincerely,
Rabbi Sam Gordon

Rabbi Samuel N Gordon
Congregation Sukkat Shalom
1001 Central Ave
Wilmette, IL. 60091
847-251-2675



March 7, 2018

RECEIVED MAR 12 2018

Michael Braiman
Assistant Village Manager
1200 Wilmette Avenue
Wilmette, IL 60091

Dear Mr. Braiman and Members of the Wilmette Minimum Wage and Paid Sick Leave Working Group:

Thank you for the invitation to respond to the Village Board decision to opt out of the Cook County ordinance requiring employers to pay an increased minimum wage and grant paid sick leave. I am grateful for your recognition that this is both a moral and an ethical issue.

I serve as a priest of the Episcopal Church and am blessed to live and serve here in this community as Rector of St. Augustine's Church, now in my sixth year of this call.

In the tenth chapter of Matthew's gospel, Jesus is preparing to send out disciples, two by two, to preach and to heal people. He anticipates that as they do that work, they will be cared for, because "the laborer deserves to be fed." Jesus sends his friends out into the community to do work that needs to be done, expecting that the community will respond to the disciples' - the *workers'* - basic needs.

You have before you the opportunity to address a matter of dignity and justice. The Cook County ordinance raising the minimum wage and ensuring paid sick leave is an attempt to move toward provision for workers' basic needs in this county. You already have the support of this community for both aspects of the ordinance, by way of overwhelmingly favorable votes in 2014 and 2016, pertaining to an increase in the minimum wage and the opportunity for workers to accrue paid sick leave.

As a priest of the Church, I believe it is a matter of basic dignity that workers deserve a just return for their labor. Raising the minimum wage and providing for paid sick leave is an issue of fairness to those who work here and a sign to the community of our commitment to do the right thing. As a priest of the Church and as a resident of Wilmette, I urge your recommendation that the Village Board reverse its previous decision and opt back in to the Cook County ordinance.

Sincerely,

Kristin

The Reverend Kristin Uffelman White, Rector
St. Augustine's Episcopal Church
1140 Wilmette Avenue Wilmette, Illinois 60091
847.251.6922 | www.staschurch.org

Work Area Profile Report All Jobs

Total All Jobs

	2015	
	Count	Share
Total All Jobs	7,257	100.0%

Jobs by Worker Age

	2015	
	Count	Share
Age 29 or younger	1,694	23.3%
Age 30 to 54	3,536	48.7%
Age 55 or older	2,027	27.9%

Jobs by Earnings

	2015	
	Count	Share
\$1,250 per month or less	2,131	29.4%
\$1,251 to \$3,333 per month	2,492	34.3%
More than \$3,333 per month	2,634	36.3%

Jobs by NAICS Industry Sector

	2015	
	Count	Share
Agriculture, Forestry, Fishing and Hunting	16	0.2%
Mining, Quarrying, and Oil and Gas Extraction	0	0.0%
Utilities	0	0.0%
Construction	206	2.8%
Manufacturing	44	0.6%
Wholesale Trade	131	1.8%
Retail Trade	1,275	17.6%
Transportation and Warehousing	9	0.1%
Information	135	1.9%
Finance and Insurance	461	6.4%
Real Estate and Rental and Leasing	283	3.9%
Professional, Scientific, and Technical Services	437	6.0%
Management of Companies and Enterprises	0	0.0%
Administration & Support, Waste Management and Remediation	151	2.1%
Educational Services	1,363	18.8%
Health Care and Social Assistance	634	8.7%
Arts, Entertainment, and Recreation	468	6.4%
Accommodation and Food Services	779	10.7%
Other Services (excluding Public Administration)	569	7.8%
Public Administration	296	4.1%

Jobs by Worker Race

	2015	
	Count	Share
White Alone	5,948	82.0%
Black or African American Alone	717	9.9%
American Indian or Alaska Native Alone	21	0.3%
Asian Alone	484	6.7%
Native Hawaiian or Other Pacific Islander Alone	5	0.1%
Two or More Race Groups	82	1.1%

Jobs by Worker Ethnicity

	2015	
	Count	Share
Not Hispanic or Latino	6,410	88.3%
Hispanic or Latino	847	11.7%

Jobs by Worker Educational Attainment

	2015	
	Count	Share
Less than high school	630	8.7%
High school or equivalent, no college	1,186	16.3%
Some college or Associate degree	1,574	21.7%
Bachelor's degree or advanced degree	2,173	29.9%
Educational attainment not available (workers aged 29 or younger)	1,694	23.3%

Jobs by Worker Sex

	2015	
	Count	Share
Male	3,046	42.0%
Female	4,211	58.0%

WORKING FAMILIES TASK FORCE



Final Report



April 4th, 2016

EXECUTIVE SUMMARY

After passing an ordinance that will provide more than 400,000 Chicago workers with a raise over the next five years, Mayor Emanuel launched the Working Families Task Force to identify additional reforms to strengthen the protections in place for the city's workers. In addition, nearly 82 percent of Chicago voters supported the adoption of paid sick days for workers citywide via a non-binding referendum held in February 2015. The Task Force examined three issue areas: (1) paid sick leave, (2) schedule predictability for shift workers, and (3) paid family and medical leave. After 6 months of research, community engagement, and deliberation, the task force is proposing a framework for expanding access to sick leave and family and medical leave while recommending further research and discussion on schedule predictability before any legislative action is taken.

The following is a summary of Task Force recommendations:

Paid Sick Leave

The Task Force recommended a framework that would provide workers with paid sick leave while having a nominal impact on employer costs. This proposal would:

- Allow workers to accrue and use up to 5 earned sick days over the course of 1 year.
- Workers would earn sick time at a rate of 1 hour earned for every 40 hours worked. This approach ensures that employees earn and accrue sick time at a proportional rate based on hours worked.
- Accrued sick leave could be used by new employees after an initial 6-month probationary period.
- Allow employees to roll over up to 2.5 unused sick days to the following year.
- Exempt employers that offer combined leave benefits such as Paid Time Off (PTO) from these requirements as long as employees could accrue and use up to 5 days of PTO within a calendar year.
- This framework would not require the pay out of unused sick days by the employer and it would also exempt sick leave benefits that are negotiated as part of a collective bargaining agreement.

A cost model developed by the Civic Consulting Alliance found that this framework would lead to less than a 0.7-1.5% increase in labor costs for most employers.

Family and Medical Leave (FMLA)

The Task Force's proposed framework would allow employees to bank a portion of their earned paid sick time to use exclusively for FMLA-eligible purposes. Only companies that are subject to federal FMLA requirements and only employees eligible for federal FMLA benefits would be qualified for this benefit. Qualified employees could bank up to 5 days of accrued sick leave for FMLA-related purposes only (such as child birth and the treatment of serious illnesses). In the same year as accessing banked FMLA days, employees would be allowed, at minimum, to take up 7.5 days in a calendar year.

Scheduling Practices

Over three months, the Task Force reviewed research, data, and testimonials to better understand the impact that scheduling practices have had on workers and their families. Existing and proposed legislation focused on a variety of provisions that try to address the core objective of increasing stability for working families, each from different angles. Toward that end, there was general agreement within the Task Force that efforts should be made to better understand and reduce unwanted and harmful levels of hour unpredictability for employees – both with regard to income levels and unpredictable schedules – while simultaneously not limiting the exercising of mutually desired flexibility between employers and employees. However, the group believes further examination is warranted before any legislation is proposed, given the high levels of complexity with the issue. Therefore, the task force did not recommend any specific policy proposals to be implemented by legislation, but rather focus on continuing the conversation with key stakeholders.

Additional recommendations

The Task Force proposed additional recommendations to support and incentivize businesses to adopt paid leave policies. These recommendations include creating a City recognition programs for businesses with family friendly policies, and supporting further research into models that expand access to family leave for employees that reduce the cost burden on employers.

2 – PAID SICK LEAVE

2.1 – Context

Paid sick leave policies, also known as earned sick leave, provide workers with the ability to take time off when they are too sick to work, or if they need to provide care to a sick family member. These policies help families economically by preventing lost income due to illness, and also mitigate the negative effects on public health. Studies suggest that paid sick leave reduces health care costs by promoting usage of primary and preventive care instead of emergency services. Earned sick leave policies encourage individuals to take advantage of preventative service – individuals are 40% more likely to obtain the flu vaccine,¹⁷ 16% more likely to use outpatient services, 20% less likely to use the ER,¹⁸ and more likely to have a mammogram, pap smear, and a regular doctor’s visit.¹⁹ In addition, several studies have found that workers who come to work sick generally experience twice the number of heart related health incidences²⁰ and were the primary cause of 70% of norovirus outbreaks, the most common related food-borne illness.²¹ Further, paid sick leave policies have been associated with reducing the spread of illness,²² sending fewer sick children to school,²³ and reduced occupational injuries.²⁴

Some employers argue that these policies generate benefits for the employer by improving retention, reducing absenteeism, and improving productivity at work.

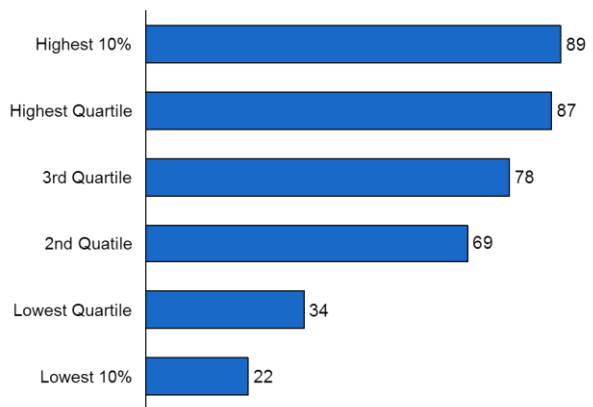
Access to paid sick leave provided by employers on a voluntary basis has increased in the US in recent years, rising from 50% in 1992 to 61% in 2015,²⁵ but there is a significant divide in terms of who gets paid sick leave and who does not. In particular, while 82% of management and professional occupations have access to paid sick leave, only 40% of service occupations do so. Much lower access to the same benefits is provided to part-time workers.²⁶

As a result, millions of American workers are going to work sick. In many employment situations, employees face pressure to work even while contagious, either because of the threat of disciplinary action by their employer, or because they simply cannot afford to take unpaid time off.

Recent surveys and polls have indicated that there is popular support for a change to the status quo. In addition to the non-binding referendum in the City of Chicago discussed above, national surveys have indicated that a significant majority of Americans believe that earned sick leave is an important benefit for both employees and employers.²⁷

Exhibit 2

Access to Paid Sick Leave Benefits by Average Wages



Source: Bureau of Labor Statistics, Employee Benefits in the United States (March, 2015)

Exhibit 3

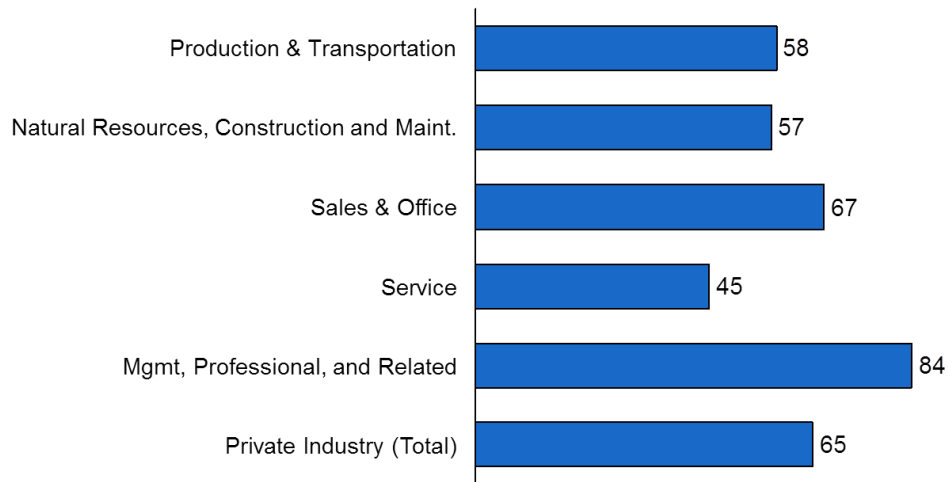
Access to Paid Sick Leave at Private US Companies (%)



Source: Bureau of Labor Statistics, *Beyond the Numbers*, Robert W. Van Giezen, "Paid Leave in Private Industry over the past 20 Years," (Aug. 2013); Bureau of Labor Statistics, Employee Benefits, National Compensation Survey (March, 2014)

Exhibit 4

Percentage with Access to Paid Sick Leave by Job Category



Source: Bureau of Labor Statistics, National Compensation Survey (March, 2015)

2.2 – Impact in Chicago

According to one study, 42 percent of private-sector workers in Chicago, or roughly 460,000 people, do not have access to paid sick days. Such workers are predominantly low-wage – more than three quarters of Chicago workers earning less than \$20,000 per year lack access to paid sick days. In contrast, roughly half of employees earning between \$20,000 and \$35,000 have access to paid sick days, and four fifths of workers who earn more than \$65,000 per year have access.

Disparities in access also exist by occupational category. In Chicago, between 70-80% of employees in service sector occupations (e.g. food preparation and service, personal care and service) do not have access to paid sick days. Additional industrial sectors where employees have limited access to sick leave include construction (71%), transportation and material moving (56%), maintenance and cleaning (56%), and production (53%).²⁸

The Task Force’s focus groups provided insights into the benefits and challenges of paid sick leave policies. As discussed previously, employees consistently considered earned sick leave as their most important priority, not only for personal use but also to care for dependents. Those employees that currently lack access to paid sick leave indicated that they have and will continue to work while sick, due both to their inability to afford the loss of income and fear of reprisals from employers. When employees took unpaid time off due to illness, they were sometimes required to provide a doctor’s note. This requirement was said to present logistical and financial barriers, particularly when required for absences of only one to two days. Employees also reported instances of being disciplined, varying from fewer scheduled hours to the termination of their employment.

As discussed previously, from the employers’ perspective, the Task Force heard from both those that do not offer paid sick leave and those that do. Employers that do not offer paid sick leave expressed reticence on a number of themes identified in the overarching concerns discussed above (e.g. costs, administrative complexity, and potential abuse). Some indicated that they simply could not offer it due to economic factors. Others did not offer certain benefits as part of collective bargaining agreements with their workforce.

In contrast, employers that do offer paid sick leave did so for various reasons, including to improve staff retention in a competitive employment environment and to reduce the likelihood that employees would show up to work sick. Many employers did indicate, however, that the division between paid sick time and vacation time may not be necessary. Instead, they provide a combined paid time off policy, which they have found to address both the issues surrounding complexity and limiting the potential for abuse. As discussed above, the Task Force also heard numerous examples of different treatment for salaried vs. hourly employees – in particular, salaried employees were much more likely to receive paid sick leave than hourly employees.

2.3 – Current Legislation

Exhibit 5

Overview of Paid Sick Leave Legislation

State/Locale	Minimum Employment ¹	Days until usage ²	Work hours to accrue one hour	Accrual begins	Maximum hours accrued ³
California	30 days/ year	90	30	Immediate	24
Connecticut	None	680 hours	40	Immediate	40
Massachusetts	None	90	30	Immediate	40
Oregon	None	90	30	Immediate	40
Vermont	>18 hrs/week	365	52	Immediate	40
Montgomery County, MD	>8 hrs/ week	90	30	Immediate	32 – 56
Washington, DC	None	90	37 - 87	Immediate	24 – 56
Emeryville, CA	2 hrs/ year	90	30	Immediate	48 – 72
Oakland, CA	2 hrs/ week	90	30	Immediate	40 – 72
San Francisco, CA	None	Once accrued	30	90 days	40 – 72
Plainfield, NJ	80 hrs/ year	100	30	Immediate	24 – 40
Jersey City, NJ	80 hrs/ year	90	30	Immediate	40
Newark (and 8 other NJ cities)	80 hrs/ year	90	30	Immediate	24 – 40
New Brunswick, NJ	>20 hrs/week	120	30	Immediate	24-40
New York City, NY	80 hrs/ year	120	30	Immediate	40
Portland, OR	240 hrs/ year	90	30	Immediate	40
Philadelphia, PA	40 hrs/ year	90	40	Immediate	40
Pittsburgh, PA	None	90	35	Immediate	24 – 40
Seattle, WA	240 hrs/ year	180	30 - 40	Immediate	40 – 72
Spokane, WA	240 hrs/year	90	30	Immediate	24 – 40
Tacoma, WA	80 hrs/ year	180	40	Immediate	24

1. Minimum employment within the locale required to be eligible for paid sick leave

2. Minimum number of days after the commencement of employment after which paid sick leave can be used

3. Ranges are typically based on company size, with smaller businesses required to provide fewer hours of paid sick leave (in some cases, such leave is also unpaid)

At least 145 countries ensure access to paid sick days for short- or long-term illnesses, with 127 providing a week or more annually, through a combination of government programs and mandates on employers.²⁹ In the United States, by contrast, there is no such national requirement – proposed legislation exists (H.R. 932, the Healthy Families Act) but has not been enacted into law. Instead, legislative action in the United States has taken place at the state and municipal level. Over the last decade, more than 20 cities, five states, and one county have passed legislation that mandates sick leave (see Exhibit 5 for a sample of municipal and state laws).

Currently, there is no Illinois law requiring private employers to provide their workers sick leave, paid or unpaid, although many employers do grant it as an employee benefit. In 2015, Sen. Toi Hutchinson introduced SB1836, the Healthy Workplace Act, to require paid sick leave statewide. Its provisions include:

- All employees who work in Illinois would have the ability to earn paid sick leave;
- The hours would begin to accrue at the commencement of employment and could be used 120 days following commencement of employment;
- For every 30 hours worked the employee would accrue one hour of earned sick time, up to a maximum of 56 hours (or 7 standard work-days) of paid sick time during a 12 month period.³⁰

As previously discussed, there has also been proposed legislation in Chicago. The 2014 ordinance introduced by Ald. Moreno and Ald. Foulkes garnered the backing of a majority of Aldermen, and would have allowed all eligible employees to earn one hour of sick time for every 30 hours worked, with a cap of five to nine days a year depending on the size of their employer. The draft ordinance was not voted on, however, and so expired at the conclusion of the previous Council term.

2.4 – Possibilities for Expanding Paid Sick Leave to Paid Family and Medical Leave

In addition to paid sick leave, a number of employee participants in the Task Force’s focus groups identified paid family and medical leave as a critical priority for the stability of their families. When employees do not have access to paid family and medical leave, they are often forced to choose between important family obligations, such as caring for a newborn or an elderly parent, or going to work for the compensation necessary to support themselves and their loved ones. Such difficult choices are likely not as common as those alleviated by earned sick leave policies, but many Task Force members felt that they are no less worthy of consideration in public policy discussions. The United States is the only industrialized nation without mandated paid family and medical supports, and it is not a commonly offered benefit: nationally, only 13% of private sector employees have access to paid family and medical leave, with the Midwest slightly lower at 12%. Moreover, workers with higher wages are significantly more likely to have access to paid family leave: while only 5% of employees in the lowest quartile of wage-earners have access to paid sick leave, 21% of workers in the highest quartile do. Even for unpaid family leave, the same disparity exists, with rates of 78% and 93%, respectively, for the lowest and highest wage quartiles.³¹

Only three American states have paid parental leave in effect – California, New Jersey, and Rhode Island – while a fourth, Washington, has enacted legislation but has yet to set a timetable for implementation. These states offer the benefit usually through existing state temporary disability insurance programs.

Taking the focus group comments and this information into consideration, the Task Force considered the possibility that a paid sick time policy could simultaneously increase access to paid family and medical leave. In conjunction with an earned sick leave policy, this idea could be applied in one or both of the following two forms of coverage. First, the eligible usages for paid sick leave could be expanded to include all FMLA-eligible usages, including the care of a newborn, newly-adopted, or newly-placed child (for either parent, if applicable). Second, unused time at the conclusion of the year could be maintained specifically for FMLA-eligible usages.

2.5 – Recommendations

The Task Force reviewed research, data, and testimonials to better understand the impact paid sick leave policies have on businesses and employees. Based on this information, the Task Force reached general consensus among a significant majority of members on the following eight key principles regarding paid sick leave that should be considered in future discussions by policy makers and stakeholders on the issue.

1. **Employee Eligibility** – An employee that completes 80 hours of work within 120 days of employment should be eligible to accrue earned sick days from the employer.

Rationale:

Based on the discussions in the focus groups, the Task Force concluded that a uniform, easy to understand eligibility criteria would be administratively simpler. This eligibility criterion would capture all employees who regularly work roughly five hours per week or more for a given employer.

2. **Waiting Period Before Usage** – Eligible employees should begin to accrue benefits from the commencement of employment, but should not be eligible to use earned sick leave until sometime ranging from 120 – 180 days after the commencement of employment.

Rationale:

The Task Force wanted to respond to the concerns employers expressed in the focus groups regarding temporary and seasonal employees, as well as new hires. However, the Task Force did not want to define what constitutes temporary or seasonal, and instead decided to address the issue by recommending a waiting period before usage would be allowed. By delaying usage of paid sick days until 120 – 180 days after the commencement of employment, paid sick leave would not be available to employees traditionally considered short-term, seasonal, and/or temporary employees.

Moreover, this waiting period would address business concerns about new employees who do not complete their probationary period (typically 90 – 180 days), especially with regard to unsatisfactory employees abusing any accrued paid sick time. At the same time, since accrual would still begin at the commencement of employment, employees who do make it beyond the probationary period would thus have access to whatever time they accrued in their first 120 – 180 days. Such a policy is also consistent with existing legislation addressing paid sick leave in the United States.

3. **Accrual Rate** – Employees should earn one hour of paid sick leave per every 40 hours worked.

Rationale:

The Task Force wanted to ensure that both full- and part-time employees would have access to paid sick time, but felt that the difference in weekly hours worked should be recognized. As such, rather than recommending that employees be given a set number of days or hours of earned sick leave, the Task Force concluded that pro-rating sick time based on hours worked was a reasonable solution. The recommendation is consistent with the prevailing norm in existing legislation addressing paid sick leave in the United States.

4. **Cap for Accrued Hours** – At minimum, employees should be able to earn and use up to 40 hours of paid sick leave per year, with no differing tiers based on employer size.

Rationale:

The Task Force recognized that different employers have different economic situations, and as such considered ways in which to distinguish between firms. However, the Task Force ultimately concluded that, as a matter of policy recommendation, a single tier was preferable, as any tiered system would impose undue administrative complexity and tracking; both for government and businesses, and especially with regard to businesses that operate at or near the demarcating line.

The Task Force concluded that 40 hours was a reasonable cap for yearly accrual, as a full time employee would accrue roughly 40 hours, or a full work week, of paid sick time in less than a year's time.

5. **Rolling Over Unused Sick Time** – Employees should, at minimum, be allowed to roll over up to 20 hours of unused sick days at the end of the year for use in the immediate following year.

Rationale

This recommendation will ensure that employees who have accrued sick time can take advantage of earned leave early in the calendar year if they or one of their dependents gets sick. "Use it or lose it" policies can have an unintended incentive for employees to max out their benefits to avoid "losing" a benefit. This roll over proposal would help avoid this incentive and ensure that a reasonable amount of earned sick leave is available to employees at the height of flu season in the beginning of the year.

6. **Rolling Over Sick Time for Paid Family and Medical Leave** – Employees should be allowed to roll over, bank, and use at minimum up to 40 hours of unused sick days at the end of the year for future (but within a specified duration) FMLA-eligible uses, which include:

- The birth of a son or daughter, and to care for the newborn child;
- The placement with the employee of a child for adoption or foster care, and to care for the newly placed child;
- To care for an immediate family member (spouse, child, or parent – but not a parent "in-law") with a serious health condition;
- When the employee is unable to work because of a serious health condition.³²

Only those employers that are currently subject to FMLA requirements would be subject to this provision.

Rationale

This recommendation would provide employees with the ability to plan for major life events, such as the birth of a child or a major surgery, by providing them with some paid family and medical leave while not dramatically adding to costs. For example, if an employee worked full-time and accrued 40 hours over the course of a year and used 20 of them (within the estimated ranges of average paid sick day usage on a national level),³³ they could only bank the remaining 20 hours toward FMLA-uses. Moreover, this policy could assist in addressing the potential for employees to abuse paid sick days by eliminating the incentive to "use or lose" benefits.

7. **No Payout of Unused Accrued Sick Leave** – Employers should not be obligated to pay out any hours for accrued unused or banked sick leave.

Rationale

The Task Force concluded that ascribing a cash value to earned sick leave would pose an undue burden on businesses (both due to additional costs and because it would require them to carry these accruing costs on their balance sheets), and would not align with the stated reasons for providing all employees with paid sick leave. Namely, earned sick leave would alleviate the public health concerns involved with employees going to work while sick, as well as allow employees to avoid difficult choices between caring for sick dependents and going to work. Neither of these would be better served by requiring sick days to be cashed out.

8. **Interaction with Existing Paid Time Off Policies** – Employers that offer employees equivalent paid sick time benefits through paid time off (PTO) policies should not be mandated to make adjustments to their policies. Equivalent PTO policies would allow employees to accrue an equal or greater number of hours (or days) of paid sick time per year as defined by state or local policy. If days are given up front such as

at the beginning of each calendar year, they should be at least equal to the minimum cap for paid sick leave accrual as defined by local or state policy.

Rationale

The Task Force did not want to require businesses that already offer such policies to change their practices, or to impose additional burdens upon such businesses. The Task Force also heard from several employers who offer PTO as a way to simplify the administration of benefits, and who said that any requirement to shift back to a defined allocation between paid sick time and vacation would require them to change existing policies, that result in a net loss of paid time off.

Concerns

The following concerns with the above recommendations were discussed and raised by some members of the Task Force:

Cost and complexity

- Employers believed that paid sick leave could increase administrative complexity by requiring new software or processes, forcing employers to spend greater time on administration, and increase the complexity of staffing and scheduling.
- Some members preferred recommending that one hour be accrued per every 30 hours worked as a way to align with other legislative models, while others suggested tying the accrual rate to the definition of full-time work under the Affordable Care Act given that it may be changed legislatively in the future.
- Some task force members would have preferred to create a detailed list of exclusions and definitions for temporary, seasonal, holiday, and other non-eligible employees, in order to lower costs for employers who rely on these occupations.
- Some task force members preferred a waiting period longer than that of the recommendation, as a way to both lower costs and incentivize employees toward longer tenures at an employer. However, several other task force members registered concern that any waiting period would be detrimental to maintaining a healthy workplace, and would have preferred a waiting period of 90 days or less.
- Some task force members believed that any legislation – state or local – should mirror existing employer exemptions found in minimum wage legislation, but others felt strongly that all employees who met the hours-worked criteria should be eligible.
- Some members felt that the provision on capping the amount of hours that can roll over each year at 20 was irresponsible, as it would penalize employees who did not use their sick days and make it difficult if an extensive medical leave were required early in the following year. Moreover, they worried that such a provision would incentivize employees to take any hours in addition to that threshold as “personal days” at the end of the year.
- Some members were concerned about not further distinguishing benefits between full and part-time employees and would have preferred ranges based on hours of employment, while other members were concerned about any employers being exempt from standards.

Further analysis desired

- Some task force members wanted more analysis to be done on how banking paid sick time toward FMLA uses would interact with existing PTO policies before issuing it as a recommendation.

Impact on small businesses

- Some members raised concerns about a single tier having a deleterious impact on small businesses that do not have the cash flow or the experience to implement a sick leave regime, although it was noted by

some that the amount of revenue – rather than the number of employees – would be a more accurate indicator of the potential for deleterious impact.

- Some members were concerned about not further distinguishing benefits between sizes of businesses and would have preferred ranges based on size of employment.

Cost Model

While considering the recommendations described above, the Task Force asked for a cost analysis to be done to determine the recommendations' potential cost implications for businesses. The model was built based on the model used in the Mayor's Minimum Wage Working Group. It relied upon the best publicly available sources of data, including:

- Bureau of Labor Statistics data on wages in the Chicago metropolitan statistical area³⁴
- Bureau of Labor Statistics data on the projected inflation in wages and healthcare,³⁵ as well as Congressional Budget Office estimates of the Consumer Price Index³⁶
- Estimates of benefit costs to employers from the Bureau of Labor Statistics and major payroll³⁷ and accounting firms³⁸

Using this information, the cost model provided an estimate of the potential costs. Because of its underlying assumptions, the numbers it produced were not meant to be taken as the certain outcome of any policy, but instead as a tool for the Task Force to use when evaluating its proposals. Using the Task Force recommendations discussed above (in particular, a 40 hour cap with one hour accrued for every 40 hours worked and a 20 hour cap on time rolling over from year to year), the model projected that for a full-time, non-tipped worker making the median hourly wage in the Chicago area in 2016, the proposals would cost employers:

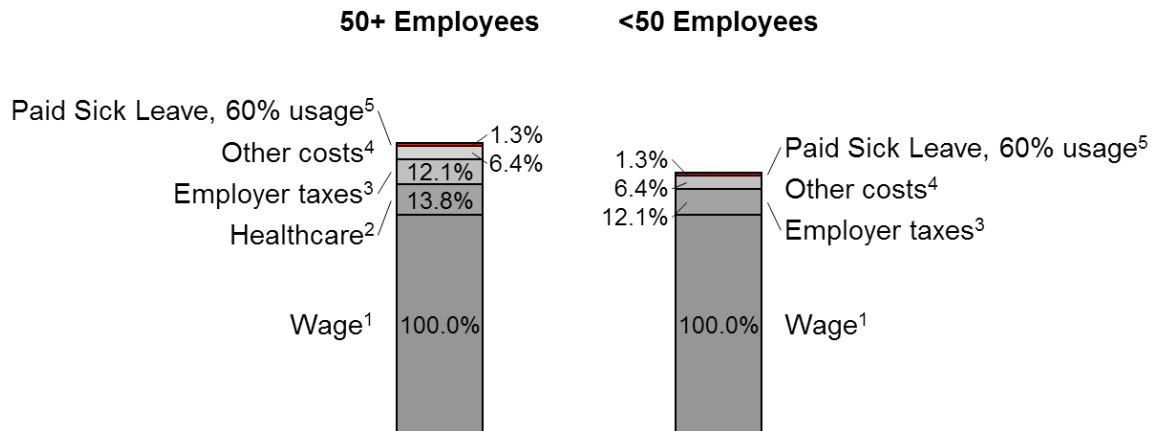
- 0.8-1.5% of base wages
- 0.7-1.3% of compensation costs under current law for large companies
- 0.7-1.5% of compensation costs under current law for small companies
- The ranges are based on different assumptions about usage, ranging from 40% (low) to 80% (high). For reference, national estimates on usage range from two days in leisure and hospitality to four days in professional services³⁹ which would be between 40% to 80% of the proposed cap.

The cost of the Task Force's proposal on banking paid sick days toward days that could be used for FMLA-eligible purposes was not included in the model as data on usage of paid FMLA leave in the United States is less commonly available. Initial estimates would indicate that these costs would be significantly lower than those of paid sick leave, but further study on this issue is encouraged.

It should be noted that the model was meant to capture the additional costs imposed on companies by the proposed policies, and does not cover the *already existing* costs employers incur to replace sick employees when they take unpaid time off. Moreover, it did not include any potential cost-savings to employers, which could result due to improved employee morale, decreased turnover, or increased productivity. While any potential cost-savings are not included in the model given the lack of data available specific to the Chicago region, the task force considered studies that outline these potential benefits (found at the end of this report).

Exhibit 6

Estimated cost of paid sick leave in 2016 under Task Force proposals



1. Wages based on Chicago MSA median wage in 2016 (estimated to be \$18.45 per hour). 2. Based on an estimated healthcare cost of \$2.43 per hour (average cost in private industry in March 2015). 3. Based on the Minimum Wage Working Group model: Employer taxes include estimates of Social Security, Medicare, and state and federal unemployment insurance. 4. Based on the Minimum Wage Working Group model: Includes estimated costs of vacation, overtime, shift changes, and workers' compensation. 5. 60% usage would equal 24 hours, or 3 standard 8-hour workdays.



San Francisco's Paid Sick Leave Ordinance: Outcomes for Employers and Employees

Robert Drago and Vicky Lovell



About This Report

This study examines the effects of San Francisco's recent paid sick days legislation on employees and employers. New survey evidence is presented on how paid sick days are being used, the costs and benefits for employees and employers, and rates of employer compliance. The research represents part of a broader body of work undertaken by the Institute for Women's Policy Research on the costs and benefits of proposed paid sick days legislation. The research was made possible by grants from the Public Welfare Foundation, the Ford Foundation, and the Annie E. Casey Foundation.

About the Institute for Women's Policy Research

The Institute for Women's Policy Research (IWPR) conducts rigorous research and disseminates its findings to address the needs of women, promote public dialogue, and strengthen families, communities, and societies. The Institute works with policymakers, scholars, and public interest groups to design, execute, and disseminate research that illuminates economic and social policy issues affecting women and their families, and to build a network of individuals and organizations that conduct and use women-oriented policy research. IWPR's work is supported by foundation grants, government grants and contracts, donations from individuals, and contributions from organizations and corporations. IWPR is a 501(c)(3) tax-exempt organization that also works in affiliation with the women's studies and public policy programs at George Washington University.

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San Francisco's Paid Sick Leave Ordinance: Outcomes for Employers and Employees

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Executive Summary

The nation's first policy allowing all workers to earn and use paid sick days was implemented in San Francisco in 2007. In general, surveys of workers and employers suggest that the law is functioning well. Most employers support the law and relatively few report adverse effects. Among employees, 59,000 or 17 percent of San Francisco's workforce, worked in firms that offered no paid sick days in the past, but are now covered, and more than half of all San Francisco employees who now have paid sick days report some benefit due to the law. Evidence suggests that it is rare for employees to misuse paid sick days. More education and enforcement may be needed to address remaining instances of employer non-compliance.

This report provides results from recent surveys of 727 employers and 1,194 employees working in San Francisco regarding the effects of the Paid Sick Leave Ordinance (PSLO). For workers, survey results find:

- Despite the availability of either five or nine sick days under the PSLO, the typical worker with access used only three paid sick days during the previous year, and one-quarter of employees with access used zero paid sick days.
- More than half of San Francisco employees with access reported benefitting from the PSLO either because their employer became more supportive of usage, the number of sick days provided increased, or they were better able to care for themselves or family members.
- Black, Latino, and low-wage workers were those who most often benefitted from the law, but were also those most likely to report employer non-compliance.
- Parents with paid sick days were more than 20 percent less likely to send a child with a contagious disease to school than parents who did not have paid sick days.

For employers, survey results show:

- Employer profitability did not suffer. Six out of seven employers did not report any negative effect on profitability as a result of the PSLO.
- Most employers reported no difficulty providing sick days to their employees under the ordinance. Approximately one-third of employers reported any difficulties implementing the PSLO, and only one-sixth needed to introduce an entirely new paid sick days policy because of the law. However, some employers (also around one-sixth) are in violation of the law and still did not offer paid sick days at the time of the survey.
- Employers are supportive. Two-thirds of employers support the PSLO and one-third are "very supportive."

Rates of utilization well below the caps of five and nine days suggest that employees view paid sick days as a form of insurance—a valuable benefit when illness strikes, but saved until then and only used as needed. For employers, the findings imply that they will never pay for many paid sick days earned under the PSLO.

The findings that many employees benefitted from the PSLO, and were more often able to keep ill children at home, as well as high levels of employer support, imply that the PSLO generated health benefits. Health care costs for employers and the public should have declined both because sick individuals and their children could get low-cost preventive care, and by reducing the spread of contagious illnesses in workplaces and schools.

Introduction

As of mid-2010, paid sick days policies had been considered in nearly half the states, several cities, and in Congress,¹ but in only one case had a universal policy been enacted. In November 2006, San Francisco voters approved an ordinance allowing any and all workers to earn and use paid sick days (PSD) (Exhibit 1).² The Paid Sick Leave Ordinance (PSLO) allows workers to earn paid sick days after three months on the job, to earn up to a maximum of five days per year in small firms and nine days per year in larger firms, and to use those days for their own health needs, as well as those of other family members (including a “designated person”).

Exhibit I:

Key Provisions of the San Francisco Paid Sick Leave Ordinance

- Workers begin to accrue leave 90 calendar days after the date of hire.
- Workers earn one hour of paid leave for every 30 hours of paid work, accumulating a maximum of nine days in firms with 10 or more employees and five days in smaller firms.
- Leave may be used for workers’ own illness, injury, health conditions, and medical appointments, and to care for family members or a “designated person.”
- Unused leave (up to the maximum of five or nine days) carries over from one year to the next.
- It is unlawful for employers to retaliate against workers for requesting or using leave under the PSLO.
- Employers are required to post information about the PSLO and maintain records on hours worked and PSD used.
- The San Francisco Office of Labor Standards Enforcement has authority to enforce the PSLO and levy penalties.³

Under the PSLO, the number of paid sick days that can be carried over from year to year is capped at nine days for large and five days for small employers. The PSLO cap minimizes employer costs while encouraging workers to use paid sick days when needed.

Both San Francisco city officials and San Francisco employer groups have characterized the PSLO as having a low impact on employers and being relatively easy to implement, but to date no empirical research has been available to guide policy development elsewhere.⁴ This report provides evidence from workers and benefits managers about the effect of San Francisco’s policy. These data can instruct businesses, employees, and decision leaders in other areas who are considering similar policies.

The IWPR survey of employees was conducted by telephone by David Binder Research in January and February 2010. The sample frame was based on zip codes inside and outside San Francisco and included both land-line and cell phone numbers. Survey respondents were at least 18 years old and had worked an average of at least 10 hours per week for at least three months for a private-sector San Francisco firm at some time after February 2007. Interviews were completed with 1,194 workers.

The IWPR survey of employers was conducted by telephone by National Research LLC in July through December 2009. The sample was stratified by non-profit status and firm size, and was targeted at benefits managers. Interviews were completed with 727 San Francisco firms, and the overall response rate was 19 percent among eligible phone numbers attempted.

Appendices A and B describe the surveys and methodology of this report in detail.

Background

Research suggests that paid sick days policies could create various benefits and costs for workers, employers, and the general public.

Paid Sick Days Promote Health at Low Cost for Employees and Their Families

Paid sick days reduce the prevalence of employees attending work when they or a family member are ill. Workers recover from health problems faster when they can take time to recuperate.⁵ As such these policies are like insurance: Many people do not become ill in a given month or even year, but if they work long enough, at some point they will need paid sick days, and use them if they are available.

One criticism of paid sick days legislation is that it is unnecessary because workers already stay home when they or other family members are ill. But in many employment situations, workers face the pressures of “presenteeism,” or the practice of workers being on the job when they have a contagious illness.⁶ Research indicates that paid sick days policies reduce presenteeism.⁷ Some workers report going to work while sick because they can’t afford to take unpaid time off, which should also occur less frequently under paid sick days policies because employees do not lose earnings while they are out sick.⁸

Policies that support employees’ health needs may also reduce stress, increase loyalty, and improve morale,⁹ thereby improving the quality of employees’ lives and improving their performance on the job.

Sending children to school or child care when they have a contagious illness is equivalent to presenteeism for workers: It spreads disease to other children and their families, as well as to teachers.¹⁰ Having paid leave is the primary factor in parents’ decisions about staying home when their children are sick,¹¹ and research suggests that having paid sick days is more effective than vacation leave in allowing parents to stay home with sick children.¹² These effects may exist because some vacation leave policies are not flexible enough to be used when children become sick.

The paid sick days legislation in San Francisco might involve costs for employees. For example, employers could legally reduce all employees’ wages,¹³ or require that all employees work harder to make up for paid sick days. However, if employers obstruct paid sick days policies by requiring individual employees to make up for lost time, or requiring medical certification of illness when it is not legally permitted, or docking employee wages for time out of work that is supposed to be paid, they are out of compliance with the law.

Paid Sick Days Provide Benefits at Low Cost to Employers

Some employers are concerned about the possible implementation costs and increased paperwork of paid sick days legislation. For employers paying workers on an hourly basis, existing work hours tracking systems may need to be expanded to count earned paid sick days. Additionally, payroll costs can increase directly among employers who provided unpaid sick days in the past and need to switch to a paid sick days approach, or if the employer needs to hire replacement workers when employees are out sick. Some people fear that an increase in payroll costs due to paid sick days legislation will lead employers, and particularly small businesses, to engage in less hiring or to lay-off existing employees.¹⁴

Employers also enjoy benefits from paid sick days. Personnel experts note that the costs of presenteeism include not just lost productivity—sick workers are paid their full salary, but can’t perform at their peak—but also a greater likelihood of injuries and mistakes.¹⁵ Employers can experience “an even greater reduction in productivity” if an illness spreads through a workplace because of presenteeism.¹⁶ Research following the 2009 H1N1 pandemic in the United States suggested that more than one-quarter of private sector employees who contracted the disease did so because of others coming to work while infected,¹⁷ causing unnecessary suffering, deaths, and productivity losses. Paid sick days may allow workers to address their health needs more quickly and miss less work overall.

Paid sick days are likely to improve job satisfaction. Higher job satisfaction is in turn associated with higher business profitability, and employment benefits affect job satisfaction.¹⁸

Paid sick days offer an additional benefit to employers in terms of scheduling. Absences that are not planned in advance are more difficult and costly for employers to deal with than scheduled time off—and are nearly half again as expensive in terms of lost productivity.¹⁹ Workers with the right to use paid sick days may be more likely to let their managers know in advance about time needed for family health care or medical appointments, rather than calling in at the last minute.

Finally, workers with paid sick days are less likely than others to switch jobs, and workers with paid leave are more likely to return to work after serious health problems.²⁰ Turnover is costly to employers, involving expenses for advertising, interviewing, testing, and training new workers.²¹

Some employers may respond to paid sick days by trying to contain costs or expand revenues. For example, an employer might lay employees off, reduce employee compensation or other benefits (e.g., vacation days), require that employees work harder in order to cover for any lost but paid working time, or raise prices.

Paid Sick Days Improve Public Health

Public health may improve due to paid sick days. As mentioned earlier, presenteeism leads employees to attend work while contagious, needlessly spreading disease and adversely affecting public health. Sending children to school or child care when they have a contagious illness spreads disease to children, teachers, and their families.²²

Some researchers expect that paid sick days policies will have longer-term positive effects by increasing preventive care, reducing emergency room visits by allowing people to go to the doctor during working hours, and improving the management of chronic diseases, such as diabetes. These effects could reduce health care costs for workers and their families, employers, and the public, but it is difficult to calculate a dollar value for these effects with available data.

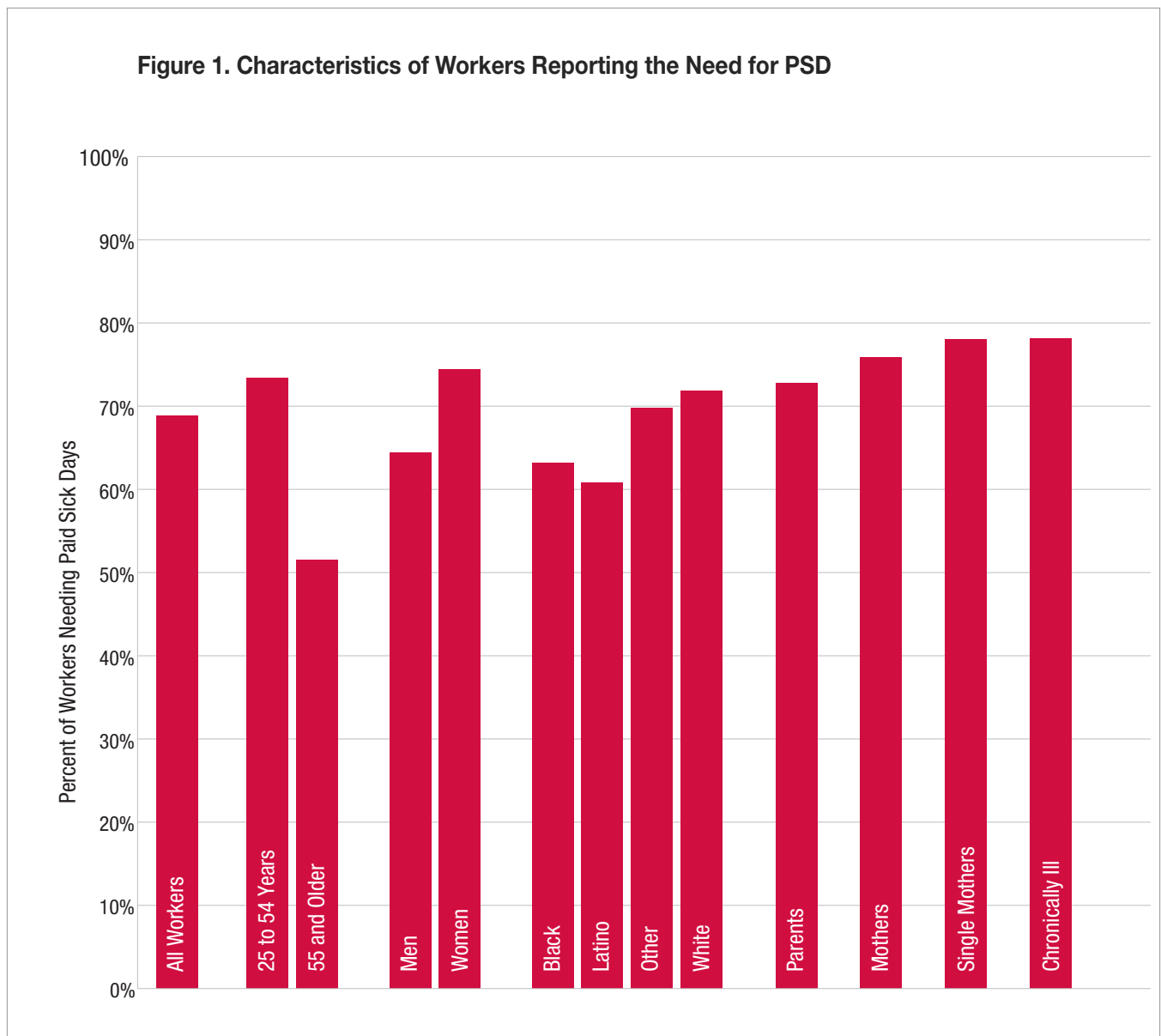
Findings: Workers and the Paid Sick Leave Ordinance

Paid Sick Days Are Important for Workers

The IWPR employee survey asked whether employees needed paid sick days, and found that **more than two-thirds of all workers had wanted to stay home in the previous year either because they were sick or they needed to care for a sick family member** (Figure 1 and Table 1). Some workers were more likely to need paid sick days than others:

- Single mothers and workers with chronic health conditions were the most likely to report needing paid sick days;²³
- Women and workers in the prime working years (ages 25 to 54 years old) were somewhat more likely than the average worker to need paid sick days; and
- Latino workers were less likely than white, black, or “other” workers to report needing paid sick days.

Clearly, the need for paid sick days is common among employees, even though many do not need paid sick days during a given year.



Source: IWPR analysis of employee survey data.

Table 1. Percentage and Characteristics of Workers Reporting the Need for PSD

	Needed PSD	Didn't Need PSD	Total	N
All Workers	68.9%	31.1%	100.0%	1,176
Age				
25 to 54 Years	73.4%	26.6%	100.0%	723
55 or Over	51.5%	48.5%	100.0%	347
Sex				
Men	64.4%	35.6%	100.0%	630
Women	74.4%	25.6%	100.0%	546
Race and Ethnicity				
Black	63.2%	36.8%	100.0%	84
Latino	60.8%	39.2%	100.0%	108
Other	69.8%	30.2%	100.0%	196
White	71.8%	28.2%	100.0%	747
Parents				
With Children	72.8%	27.2%	100.0%	302
No Children	67.3%	32.7%	100.0%	874
Mothers				
Mother	75.9%	24.1%	100.0%	146
Not a Mother	67.7%	32.3%	100.0%	1,030
Single Mothers				
Single Mother	78.0%	22.0%	100.0%	35
Not a Single Mother	68.5%	31.5%	100.0%	1,141
Workers with Chronic Health Conditions				
With Chronic Health Condition	78.1%	21.9%	100.0%	277
No Chronic Health Condition	66.5%	33.5%	100.0%	888
Wage Quartiles				
Bottom Wage Quartile	63.1%	36.9%	100.0%	188
Second Wage Quartile	68.1%	31.9%	100.0%	294
Third Wage Quartile	75.2%	24.8%	100.0%	363
Top Wage Quartile	69.9%	30.1%	100.0%	331
Union Member				
Union	66.9%	33.1%	100.0%	217
Non-Union	69.0%	31.0%	100.0%	948

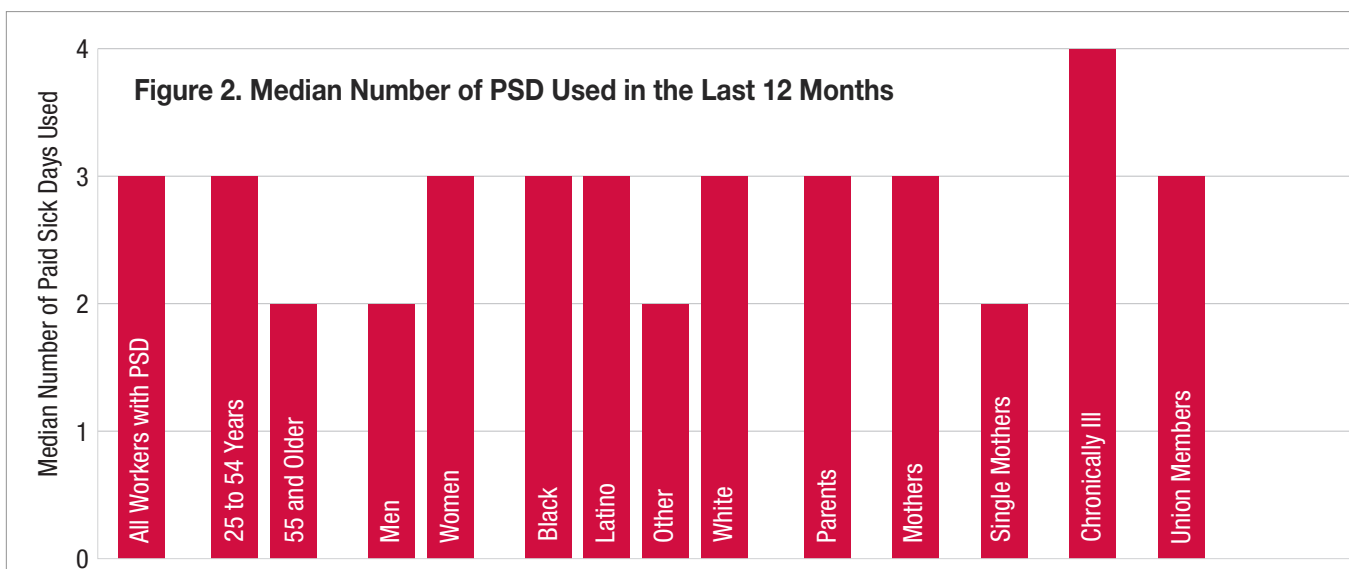
Source: IWPR analysis of employee survey data.

How Was the PSLO Used?

The median worker – the one who took more paid sick days than one half of all workers but fewer than the other half – reported using just three days of paid sick days in the previous year (Table 2 and Figure 2). This is far fewer days than the PSLO provides.²⁴ If this pattern holds in future years, the law’s restriction on days that can be carried over from year to year implies that **employees will never use and employers will never pay for many paid sick days earned under the PSLO**. This finding makes sense if the PSLO is functioning as a form of insurance: for example, many individuals pay for but do not use health insurance in a given year and, among those who use it, it is rare for an individual to use each and every benefit provided in a given plan. The finding also fits national estimates, which find covered workers in small firms use an average of 2.2 days per year, and those in large firms use 3.1 days per year.²⁵

Some workers were more or less likely than others to use paid sick days (Figure 3). Worker groups that were the least likely to use paid sick days included:

- Workers age 55 and older;
- Men;
- Latinos; and
- Single mothers.



Note: For subsample of workers reporting access to paid sick days.

Source: IWPR analysis of employee survey data.

All in all, more than **one-quarter of all workers with access did not use PSD during the last year.**

Among the workers who used some paid sick days:

- Workers with chronic health conditions used one more paid sick day than other workers, for an average of five days;²⁶ and
- Single mothers used just three days.

These results suggest that in general the PSLO is effective in providing sick days to those who need them most. There appears to be an exception for the single mothers who report a greater need for but actually use fewer days, perhaps due to fears of (illegal) employer penalties for missing too many days or a more urgent need to save days in case a child becomes sick, or because they tend to change jobs frequently so will not have accumulated as many sick days as other employees.²⁷

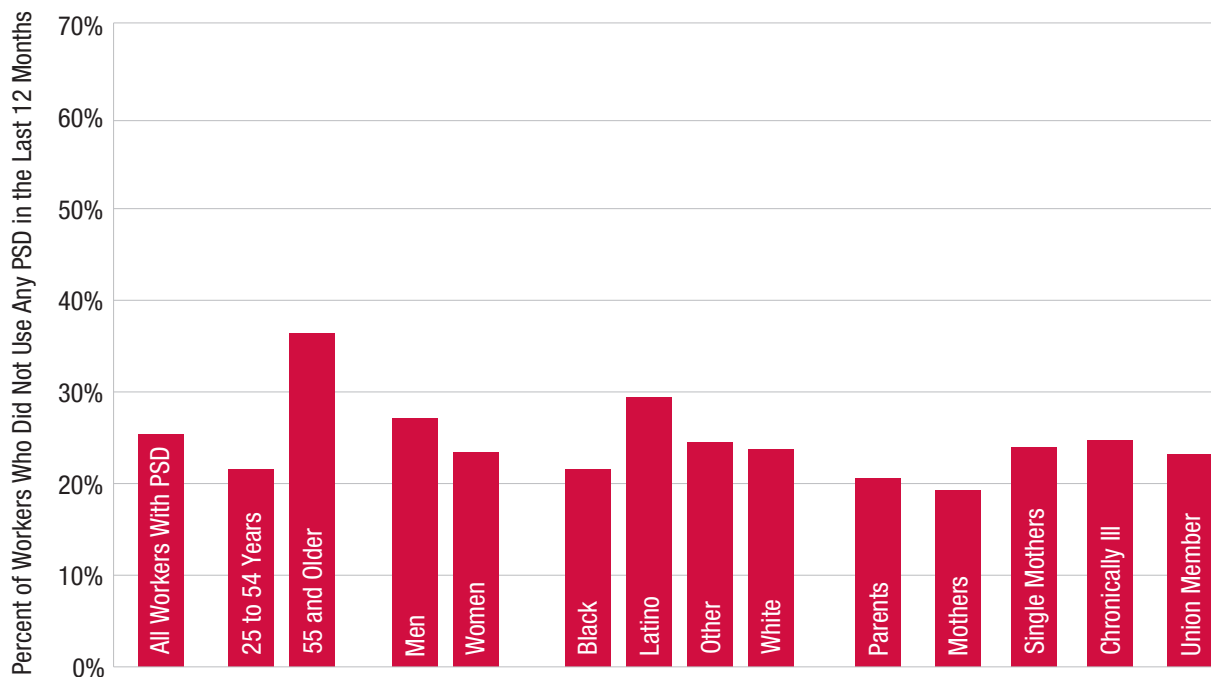
The survey also asked workers to list reasons for using paid sick days during the previous year, and permitted multiple answers. Workers responded that the primary reason for using paid sick days was due to their own health: More than

Table 2. Median Number of PSD Used and Share of Workers Not Using PSD in the Last 12 Months

	ALL WORKERS		WORKERS WHO USED PSD IN THE LAST 12 MONTHS	
	Median Number of PSD Used	Share Not Using PSD	Median Number of PSD USED	N
All Workers	3.0	25.4%	4.0	624
Age				
25 to 54 Years	3.0	21.6%	4.0	425
55 and Older	2.0	36.4%	4.0	148
Sex				
Men	2.0	27.1%	4.0	314
Women	3.0	23.4%	4.0	310
Race and Ethnicity				
Black	3.0	21.5%	4.0	49
Latino	3.0	29.4%	4.0	53
Other	2.0	24.5%	3.0	113
White	3.0	23.7%	4.0	394
Parents	3.0	20.6%	4.0	164
Mothers	3.0	19.3%	4.0	82
Single Mothers	2.0	23.9%	3.0	17
Chronically Ill	4.0	24.7%	5.0	146
Union Member	3.0	23.2%	4.0	136

Note: For subsample of workers reporting access to paid sick days.
Source: IWPR analysis of employee survey data.

Figure 3. Share of Workers Who Did Not Use PSD in the Last 12 Months



Note: For subsample of workers reporting access to paid sick days.
Source: IWPR analysis of employee survey data.

four out of five workers who took paid sick days reported using it for their own health needs (Table 3).²⁸ In addition:

- One-third used paid sick days to visit a doctor or dentist;²⁹
- One out of five workers used the time to care for a sick child;
- One out of six used paid sick days to care for an adult relative; and
- One out of 10 workers used the time for another purpose, such as a mental health day, a family reason including death, or a vacation.³⁰

Workers who used paid sick days varied by their family circumstances and other demographic characteristics. Specifically:

- Older workers who took paid sick days were somewhat less likely than prime-working-age individuals to use paid sick days for their own health needs. They were more likely to care for an adult relative while taking paid sick days, and more often used paid sick days for doctor visits;

Table 3. Reasons for Using Leave, of Workers Using PSD in the Last 12 Months

	Own Health	Visit their Doctor	Care for a Child	Care for an Adult	Other	N
All Workers	82.8%	32.8%	19.9%	16.1%	10.7%	656
Age						
25 to 54 Years	83.7%	31.0%	23.7%	13.7%	10.3%	446
55 or Over	82.6%	40.5%	8.1%	19.2%	11.2%	154
Sex						
Men	85.3%	34.2%	16.3%	18.7%	11.4%	330
Women	80.0%	31.3%	24.1%	13.0%	9.9%	326
Race and Ethnicity						
Black	72.8%	43.9%	33.7%	14.0%	9.1%	52
Latino	76.9%	39.6%	26.5%	23.3%	9.7%	56
Other	72.7%	27.4%	26.1%	14.4%	8.9%	121
White	89.8%	32.4%	13.8%	13.3%	11.5%	412
Parents						
With Children	66.7%	31.2%	60.0%	19.1%	9.7%	181
No Children	89.9%	33.6%	2.2%	14.7%	11.1%	475
Mothers						
Mother	60.9%	29.9%	67.6%	11.3%	6.0%	93
Not a Mother	87.0%	33.4%	10.8%	17.0%	11.6%	563
Single Mothers						
Single Mother	58.5%	32.1%	86.3%	3.6%	2.6%	21
Not a Single Mother	83.7%	32.9%	17.5%	16.5%	11.0%	635
Workers with Chronic Health Conditions						
With Chronic Health Conditions	87.7%	37.6%	14.6%	16.7%	12.7%	153
No Chronic Health Conditions	81.7%	31.9%	21.5%	15.4%	10.0%	498
Wage Quartiles						
Bottom Wage Quartile	79.7%	29.6%	18.7%	12.9%	7.0%	82
Second Wage Quartile	74.5%	40.4%	24.0%	14.6%	13.9%	171
Third Wage Quartile	89.4%	32.4%	16.2%	16.1%	12.2%	223
Top Wage Quartile	87.4%	28.3%	20.6%	20.3%	9.0%	180
Union Member						
Union	77.4%	40.3%	28.5%	9.9%	12.5%	137
Non-Union	83.9%	31.3%	18.1%	16.9%	10.4%	517

Source: IWPR analysis of employee survey data.

- Men who used paid sick days were more likely than women to use paid sick days for their own health needs or to care for an adult family member, while women were more likely to use paid sick days to care for a child;
- Parents who used paid sick days were much less likely than other workers to use it for their own health, with more than half taking time to care for their children. Mothers were particularly likely to use paid sick days to care for their children³¹ and particularly unlikely to use it for their own health needs. Parents were also slightly more likely than other workers to take paid sick days to care for an adult relative. In fact, one in ten parents reported using PSD to care for both a child and an older relative;
- Workers with chronic health conditions who took paid sick days were more likely than other workers to use paid sick days for their own health needs and were more likely than other workers to use PSD to visit a doctor; and

- Of all racial/ethnic groups, black workers who took paid sick days were the least likely to use it for their own health, but the most likely to take it to visit a doctor or to care for a child. Latino workers who used paid sick days were the most likely of all race/ethnic groups to use the leave to care for an adult and were more likely than workers overall to use PSD to see a doctor.

These findings suggest that many employees make trade-offs when using paid sick days. Workers with caregiving commitments, whether for children or other adults in need, tend to use paid sick days to care for others, perhaps using less time for their own health needs. The PSLO is designed to permit this sort of flexibility, so it supports families in ways that traditional sick days systems—permitting absence only for one’s own illness or injury—formally do not.³² Further, much of the caregiving for others and many of the doctor visits are likely preventive in nature, indicating that access to paid sick days is likely to improve health over time, improve job performance, and reduce future absences.³³

Workers and Their Families Benefited from the PSLO

Most employees had access to paid sick days before the PSLO went into effect.³⁴ The employer survey found that two-thirds of employers offered paid sick days prior to the PSLO (see below). These employees (and their employers) might have expected little change in sick days policies under the law. Seen in this light, it is surprising that, among employees who had the same employer before and after the PSLO went into effect and who report access to paid sick days, **more than half of workers (53.9 percent) reported one or more of the following benefits of the PSLO:** their employer became more supportive of using PSD, they gained additional PSD, or they were better able to care for their own or their families’ health needs (Table 4). In addition:

- More than one-quarter of workers noted that their employer became more supportive of their taking PSD because of the PSLO;
- Black, Latino, older, and low-wage workers were among those reporting stronger employer support of paid sick days following implementation of the PSLO;
- One out of four workers (25.1 percent) reported that they were better able to care for their own and their families’ health needs because the PSLO was adopted;
- Workers of color, including 29.1 percent of black workers and 31.2 percent of Latino workers; older workers (34.0 percent); mothers (27.3 percent); and union members (33.8 percent) were more likely to report better management of health needs; and
- One out of 10 workers said that they had more days of paid sick leave after the PSLO was implemented than before. Latino and low-wage workers were the most likely to report this, and mothers were the least likely.

Table 4. Workers’ Reported Benefits from PSD

	Employer More Supportive	More Days	Better Able to Care for Own or Families’ Health Needs	At Least One Benefit	N
All Workers	28.2%	11.4%	25.1%	53.9%	671
Age					
25 to 54 Years	26.7%	10.1%	21.1%	57.5%	391
55 or Over	34.1%	13.6%	34.0%	43.5%	230
Sex					
Men	28.4%	11.2%	23.1%	53.8%	349
Women	28.1%	11.8%	27.5%	54.0%	322
Race/Ethnicity					
Black	41.0%	12.3%	29.1%	45.4%	51
Latino	31.0%	18.6%	31.2%	40.8%	60
Other	32.7%	9.3%	30.6%	49.8%	113
White	25.8%	10.3%	21.9%	57.7%	425
Parents	28.9%	11.1%	26.3%	53.6%	164
Mothers	29.7%	6.6%	27.3%	53.9%	83
Chronically Ill	26.6%	8.2%	24.8%	52.0%	168
Union Members	27.9%	12.1%	33.8%	47.0%	153
Low-Wage Workers	32.2%	19.5%	29.6%	44.5%	73

Note: Data are for workers employed by the same firm before and after the PSLO was implemented.
Source: IWPR analysis of employee survey data.

This evidence regarding gains from the PSLO suggests the law is generally functioning as intended to level the playing field across employers and spread the insurance benefits of paid sick days to many employees who needed, but did not have, paid sick days prior to the PSLO.

The PSLO had a positive effect on parents’ ability to care for their children. Parents who had paid sick days were much less likely to report sending a sick child to school in the last year because the parent could not stay home with the child. This experience was very common among parents—two-thirds (66.4 percent) reported their child had gone to school while sick:

- Most parents who lacked paid sick days (75.9 percent) had sent a sick child to school; while
- Half of parents who had paid sick days had done so (53.8 percent). The reduction in the behavior is **over 20 full percentage points**.

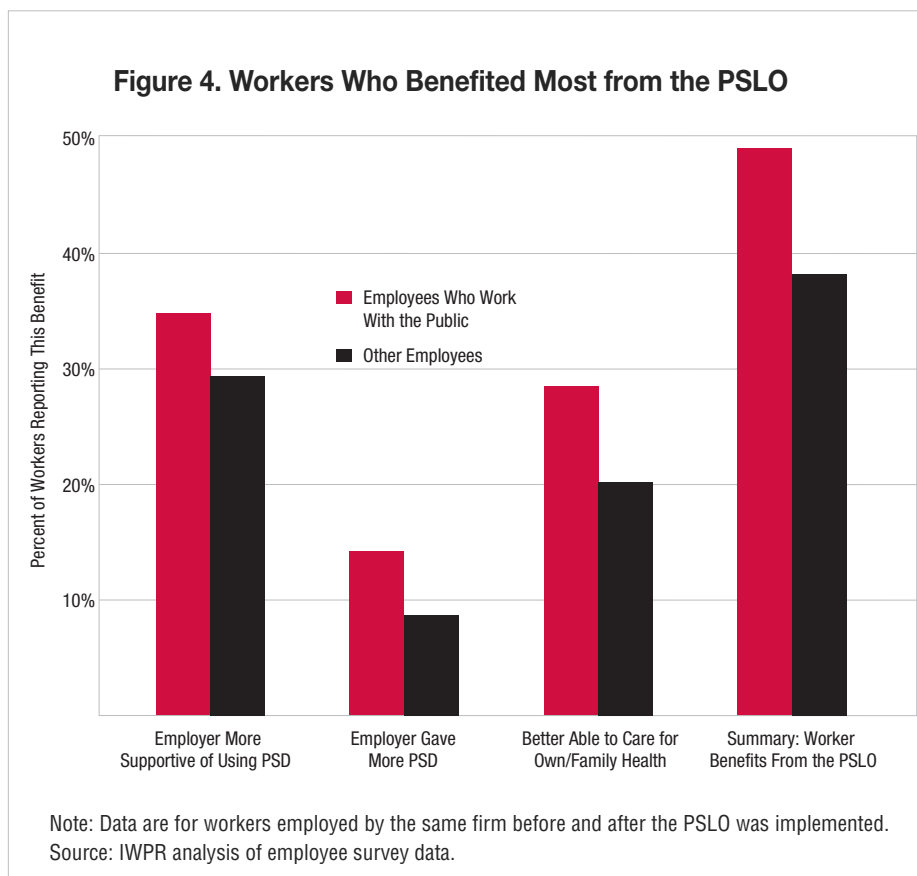
It should be noted that those who believe they lack paid sick days are in fact covered by the law. Although knowledge of paid sick days coverage dramatically reduces the rates of sick children being sent to school, the rates of parents sending a contagious child to school remain high even among those who know they have access to the PSLO. This may reflect a pressure that workers feel not to take many sick days even when they are available, or parents whose children became ill after all available paid sick days were used up.

One of the major reasons for passing the PSLO was the hope that it would particularly benefit employees who interact with the public. Anecdotal evidence suggested that, before the PSLO was adopted, many workers in low-wage occupations that involve public contact—food service, for instance—were not able to stay home when they were sick.³⁵ Presenteeism among these workers could lead to the spread of disease among the general public.

Evidence from the employee survey suggests the PSLO had precisely this intended effect (Figure 4). Specifically:

- More than one-third (34.8 percent) of workers who dealt directly with the public—in food service, health care, or retail situations, for example—reported that their employers were more supportive of workers’ use of paid sick days because of the PSLO.³⁶
- One out of eight workers with public contact (13.2 percent) reported that the PSLO reduced the level of presenteeism—sick workers on the job—in their workplace.

At the same time, however, workers who had direct contact with the public were more than half again as likely to go to work when they were sick, even after the PSLO was adopted (24.3 percent), compared to other workers (14.1 percent).³⁷



Few Workers Were Negatively Affected by the PSLO

Six out of seven workers reported that their employer did not reduce raises, bonuses, or other benefits to implement the PSLO (Table 5). Specifically:

- Four out five workers (78.3 percent) reported that there were no increased work demands in their workplace because of the PSLO; and
- Overall, two-thirds of workers (67.8 percent) reported that their employer did not increase work demands, reduce work hours, or reduce compensation in response to the PSLO.

Low-wage workers were more likely than higher-wage workers to report that their employers took action to reduce costs in implementing the PSLO;³⁸ however, many low-wage workers also reported having benefited from the PSLO (see Table 4).

Table 5. Possible Negative Effects of Increased PSD

	Layoffs, Total Hours Reduced	Fewer Raises/ Bonuses, Other Benefits Reduced	Increased Work Work Demands	Workers Paid in at Least One Way	N
All Workers	15.2%	14.1%	21.7%	32.2%	784
Firm Size					
Less Than 10	18.1%	17.8%	19.4%	34.9%	202
10 to 24	18.7%	19.6%	24.9%	41.0%	127
25 to 99	8.4%	10.1%	19.4%	23.8%	152
100 or More	13.2%	11.2%	19.1%	28.3%	303
Industry					
Information and Professional and Business Services	14.6%	11.5%	17.9%	27.5%	211
Financial Activities	9.3%	5.8%	17.7%	24.8%	96
Educational and Health Services	16.5%	11.3%	25.0%	38.6%	158
Leisure and Hospitality	18.7%	24.3%	25.3%	38.3%	79
Other Services	13.4%	20.6%	26.6%	36.2%	106
Other	18.8%	16.7%	22.0%	32.3%	120
Wage Quartile					
Bottom Wage Quartile	28.4%	23.1%	32.0%	51.1%	101
Second Wage Quartile	20.5%	17.9%	24.8%	36.7%	192
Third Wage Quartile	8.8%	11.4%	18.9%	27.6%	254
Top Wage Quartile	5.4%	6.0%	13.3%	16.6%	237
Union Member	14.7%	17.4%	33.1%	37.0%	167
Part-Time	25.4%	16.0%	24.5%	36.3%	177
Temporary or Seasonal Worker	23.2%	20.0%	18.2%	35.3%	91
College Graduate	9.4%	11.5%	14.2%	23.4%	545
Has Paid Vacation Days or PTO	14.3%	14.7%	23.1%	32.1%	615
Has Health Insurance	13.2%	12.4%	21.8%	30.0%	696
Has Paid Sick Days	8.5%	12.5%	16.9%	24.5%	374
No PSD/Not All Uses/Not Enough	27.5%	18.6%	28.1%	43.8%	209
Don't Know	14.9%	12.9%	25.8%	35.3%	189

Note: Data are for workers employed by the same firm before and after the PSLO was implemented.
Source: IWPR analysis of employer survey data.

Employees Report That Most Employers Are Compliant

Most workers reported that their employers were implementing the PSLO (Table 6). Relatively small shares of workers who had used paid sick days reported the following violations:

- Having to “make up” paid sick days (one out of 10) or find a replacement for their time off (one out of eight);
- Being threatened with loss of wages for using paid sick days (one out of 13) or actually losing wages (one out of 38); and
- Being assigned fewer or less desirable work hours or worse tasks because they used paid sick days (one out of 20).

Table 6. Employees Reporting Improper Implementation of the PSLO

	Had to Make Up Hours	Had to Find Replacement	Had to Make up Hours or Find Replacement	Threatened With wage Loss or Was Written Up	Lost Wages	Given Fewer/Worse Hours or Worse Tasks	Employer Retaliates for Use of PSD	Not in Compliance With the PSLO	N
All Workers	11.3%	13.9%	20.3%	6.3%	2.6%	4.5%	9.7%	23.7%	477
Age									
Age 25 to 54 Years	12.8%	14.2%	21.3%	6.2%	2.4%	4.8%	10.1%	25.2%	331
Age 55 or Over	7.0%	9.3%	14.8%	8.1%	2.9%	4.3%	9.2%	16.6%	133
Sex									
Men	7.7%	10.6%	15.7%	3.9%	1.8%	3.6%	7.6%	18.6%	234
Women	15.4%	17.5%	25.5%	8.9%	3.4%	5.5%	12.1%	29.3%	243
Race/Ethnicity									
Black	19.2%	32.0%	34.9%	21.5%	13.7%	10.4%	26.0%	40.6%	37
Latino	32.8%	16.9%	38.6%	10.4%	5.0%	3.9%	16.7%	44.9%	30
Other	12.7%	15.0%	23.5%	11.6%	1.3%	7.4%	14.9%	27.3%	85
White	6.2%	11.2%	14.7%	1.6%	1.7%	2.6%	4.4%	17.3%	315
Parents									
Parents	14.9%	10.8%	23.3%	5.7%	3.1%	6.1%	9.5%	27.1%	131
Mothers									
Mothers	16.4%	13.9%	28.1%	8.8%	4.3%	4.3%	9.3%	30.1%	66
Chronically Ill									
Chronically Ill	8.7%	15.0%	20.0%	4.0%	3.9%	5.0%	10.0%	26.2%	115
Union Members									
Union Members	7.1%	19.4%	24.5%	12.1%	0.0%	7.0%	17.0%	29.4%	99
Firm Size									
Less Than 10	8.2%	18.2%	23.8%	4.4%	3.6%	7.8%	8.6%	27.5%	86
10 to 24	22.0%	22.2%	33.6%	8.1%	0.5%	5.8%	13.4%	38.9%	77
25 to 99	12.4%	11.9%	17.5%	6.8%	2.1%	3.6%	8.0%	18.0%	109
100 or More	7.6%	9.5%	14.6%	6.0%	3.3%	2.8%	9.6%	18.5%	205
Industry									
Information and Professional and Business Services	14.4%	13.8%	20.7%	6.5%	2.5%	4.2%	9.6%	22.8%	169
Financial Activities	6.9%	6.0%	10.2%	0.0%	1.5%	0.0%	1.5%	11.7%	53
Educational and Health Services	12.3%	12.3%	22.2%	7.4%	3.7%	8.6%	18.8%	34.1%	101
Leisure and Hospitality	1.2%	22.6%	23.8%	0.0%	0.0%	3.5%	3.5%	27.4%	37
Other Services	13.1%	16.3%	22.0%	9.7%	4.1%	1.6%	9.7%	23.4%	64
Other	14.7%	17.6%	26.0%	14.4%	3.8%	8.7%	14.2%	26.0%	48
Wage Quartiles									
Bottom Wage Quartile	20.0%	35.7%	39.9%	20.0%	2.6%	7.0%	26.9%	44.2%	45
Second Wage Quartile	8.8%	16.8%	21.7%	7.3%	4.3%	7.2%	10.1%	25.4%	118
Third Wage Quartile	8.5%	10.7%	15.7%	3.3%	2.1%	3.5%	6.0%	19.5%	172
Top Wage Quartile	11.4%	3.3%	13.3%	0.9%	1.7%	2.0%	3.7%	15.3%	142

Note: Data are for workers who used PSD in the last 12 months.
 Source: IWPR analysis of employee survey data.

Women, black, and Latino workers; parents; and low-wage workers were more likely to report having to “make up” their PSD or find a worker to fill in for their leave. Workers in the leisure and hospitality industry and “other” industries were the most likely to report one of these situations.

The PSLO allows employers to require confirmation from a health care practitioner when workers use paid sick days for more than three consecutive days, but workers’ reports suggest that some employers request documentation for shorter leaves. For instance, two-fifths of low-wage workers who took paid sick days reported that they had been asked to show that they needed the time off—but only one-fourth had taken a total of more than three days (Table 7).

Although half of workers benefited from the PSLO, and most employers implemented paid sick days, implementation was not always perfect. Most workers reported that they could use paid sick days when they were ill themselves, and two-thirds reported that they could use paid sick days to care for family member or to see a doctor (see Appendix Table 1). Similarly, around two-thirds of workers accrued paid sick days at the required rate of one hour for every 30 hours of work. However, 29.0 percent of workers reported either not having all uses available or not accruing sick days at the required rate, and an additional 26.9 percent were not certain whether they were covered by paid sick days as required by the PSLO. In particular:

- Men were less likely than women, and older workers less likely than younger workers, to report that they could use PSD to care for a family member;
- Latino and black workers were less likely to report having full PSLO benefits;
- Parents, and particularly single mothers, were less likely than other workers to report that they could use PSD for their own health needs, and relatively few single mothers (29.9 percent) report having all the required elements of the PSLO.

Table 7. Characteristics of Workers Who Were Asked for Documentation for Using PSD

	Yes	No	N
All Workers	23.9%	76.1%	709
Firm Size			
Less Than 10	21.3%	78.7%	145
10 to 24	31.8%	68.2%	114
25 to 99	24.3%	75.7%	159
100 or More	21.5%	78.5%	291
Industry			
Information and Professional and Business Services	17.4%	82.6%	234
Financial Activities	15.5%	84.5%	77
Educational and Health Services	23.8%	76.2%	145
Leisure and Hospitality	30.9%	69.1%	58
Other Services	29.0%	71.0%	103
Other	34.5%	65.5%	82
Wage Quartiles			
Bottom Wage Quartile	38.6%	61.4%	91
Second Wage Quartile	29.4%	70.6%	189
Third Wage Quartile	17.7%	82.3%	242
Top Wage Quartile	10.3%	89.7%	187
Union Member			
Union	41.0%	59.0%	140
Non-Union	20.3%	79.7%	565
Part-Time			
Part-Time	39.8%	60.2%	114
Full-Time	22.1%	77.9%	595
Temporary or Seasonal Worker			
Temporary/Seasonal	37.2%	62.8%	66
Not Temporary/Seasonal	22.7%	77.3%	629
College Graduate			
College Graduate	11.7%	88.3%	523
Not College Graduate	39.5%	60.5%	181
Has Paid Vacation or PTO			
Has Paid Vacation/PTO	23.2%	76.8%	629
No Paid Vacation/PTO	28.8%	71.2%	70
Has Health Insurance			
Has Health Insurance	22.0%	78.0%	655
No Health Insurance	44.0%	56.0%	51

Note: Data are for workers who used PSD in the last 12 months.
Source: IWPR analysis of employer survey data.

Employers and the Paid Sick Leave Ordinance

Few Employers Had to Change Policies for the PSLO

Patterns of employer sick leave provision and compliance reflected in the employer survey are consistent with reports from employees. As was found in the employee survey results, it should be the case that most employers already offered paid sick days policies. Other employers should have responded to the ordinance with a new or expanded paid sick days policy. The employee survey results also suggest that non-compliance will be concentrated among small businesses. The employer survey results fit these expectations.

Two-thirds of San Francisco’s employers offered paid sick days before the PSLO went into effect, according to employer reports (Table 8). In response to the PSLO:

- Approximately one out of six firms enacted a new paid sick days policy. A similar share increased their existing PSD accrual rate, and one-sixth of employers expanded the share of their workforce covered by paid sick days (Appendix Table 2); and
- Overall, one-third of employers made at least one of these three changes, and **most employers (two out of three) were unaffected by the PSLO.**

Table 8. Firms Providing PSD Before and After the PSLO

Firm Characteristics	Provided PSD Before the PSLO	Provides PSD Now
All Firms	65.1%	82.1%
Number of Employees		
1 to 9	63.3%	78.4%
10 to 24	66.1%	92.0%
25 to 49	74.3%	97.5%
50 or More	83.6%	99.4%
Industry		
Accommodation and Food Service	23.6%	62.1%
Construction	29.8%	69.3%
Education, Health Care, and Social Services	67.7%	89.2%
Finance, Insurance, and Real Estate	87.0%	93.2%
Professional, Scientific, and Technical Services	79.9%	85.3%
Retail and Wholesale Trade	61.5%	77.9%
Other Services	54.5%	78.5%
Other	79.6%	91.6%
Wage Levels		
Low-Wage Firms	51.1%	69.7%
High-Wage Firms	71.4%	87.1%
Firms Work-Hours		
Low-Hours Firms	52.6%	73.7%
High-Hours Firms	78.6%	88.7%
Female Workforce		
More Than 80 Percent of Employees Are Women	67.7%	82.3%
Other Firms	56.8%	80.8%
Diverse Workforce		
50% or More of Employees Are Non-White	72.9%	84.5%
Other Firms	48.7%	76.1%
Has Unionized Workers		
Some Employees Are Union Members	80.4%	91.8%
No Union Members	64.4%	81.7%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-hours firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week. Source: IWPR analysis of employee and employer survey data.

According to the employer survey, the PSLO expanded paid sick days coverage to more than 59,000 San Francisco workers, or 17 percent of all San Francisco employees; although it is important to recall (see Table 4, above) that half of all employees reported some benefit from the ordinance.³⁹

San Francisco’s smallest firms were the least responsive to the PSLO. Among employers with fewer than 10 workers, one-quarter implemented or expanded a paid sick days policy in response, but one-third did not have a paid sick days policy at the time of the survey. On the other hand, nearly all firms with 10 or more workers offered paid sick days at the time of the survey, and more than half had enacted one or more changes in response to the PSLO.

Firms that enacted a new paid sick days policy in response to the PSLO tended to be:

- In the construction and accommodation and food service industries;
- Low-wage firms; and
- Firms with low work hours.

Employers Had Little Difficulty Implementing the PSLO

As discussed earlier, employers may report costs around implementing the PSLO or increased benefits costs, particularly if they need to hire replacements for employees using paid sick days. The employer survey asked about implementation issues and hiring replacements, with benefits costs implicitly accounted for in a question regarding profitability (see following page).

San Francisco employers reported little to moderate difficulty in implementing the PSLO (Table 9). Specifically:

- Half reported that it was “not difficult” or “not too difficult” to understand the requirements of the PSLO, and another one out of seven did not know;
- Half reported that it was “not difficult” or “not too difficult” to administer the PSLO, and another one out of seven did not know; and
- Half reported that it was “not difficult” or “not too difficult” to reassign or delay work while workers used paid sick days, and another one out of 10 did not know.

Table 9. Firms’ Reports of Difficulty Implementing the PSLO

Firm Characteristics	SHARE OF FIRMS REPORTING:								
	Understanding the Requirements of the PSLO			Administering the PSLO			Reassigning or Delaying Work		
	“Not too Difficult” or “Not Difficult”	“Somewhat Difficult” or “Very Difficult”	“Don’t Know”	“Not too Difficult” or “Not Difficult”	“Somewhat Difficult” or “Very Difficult”	Don’t Know”	“Not too Difficult” or “Not Difficult”	“Somewhat Difficult” or “Very Difficult”	Don’t Know”
All Firms	52.0%	34.6%	13.4%	53.9%	31.4%	14.7%	48.2%	42.7%	9.0%
Number of Employees									
1 to 9	49.3%	34.1%	16.6%	53.7%	28.3%	18.0%	45.0%	44.2%	10.8%
10 to 24	57.4%	39.9%	2.7%	53.8%	41.0%	5.2%	57.5%	39.6%	2.8%
25 to 49	66.6%	29.4%	3.9%	53.7%	42.5%	3.9%	61.6%	34.8%	3.6%
50 or More	65.4%	30.9%	3.6%	56.5%	39.8%	3.7%	59.2%	36.5%	4.4%
Industry									
Accommodation and Food Service	44.4%	50.0%	5.5%	36.6%	57.9%	5.5%	40.7%	59.0%	0.3%
Construction	53.0%	46.1%	0.9%	41.0%	53.2%	5.8%	53.7%	40.5%	5.8%
Education, Health Care, and Social Services	60.6%	23.2%	16.2%	69.9%	12.6%	17.5%	51.1%	39.7%	9.3%
Finance, Insurance, and Real Estate	51.5%	27.9%	20.6%	68.1%	17.1%	14.8%	58.7%	33.7%	7.7%
Professional, Scientific, and Technical Services	63.7%	30.8%	5.5%	55.3%	33.8%	10.9%	49.8%	40.1%	10.0%
Retail and Wholesale Trade	44.8%	39.8%	15.4%	55.8%	26.3%	17.9%	54.6%	29.9%	15.5%
Other Services	55.2%	34.2%	10.6%	56.2%	31.4%	12.4%	34.0%	55.8%	10.2%
Other	41.8%	32.2%	26.0%	40.5%	33.1%	26.4%	41.0%	50.8%	8.2%
Wage Levels									
Low-Wage Firms	55.5%	37.5%	7.0%	57.0%	30.7%	12.3%	52.7%	38.7%	8.6%
High-Wage Firms	57.6%	22.0%	20.4%	58.1%	20.7%	21.2%	50.9%	36.3%	12.8%
Firms Work-Hours									
Low-Hours Firms	56.5%	30.3%	13.1%	56.7%	28.4%	14.9%	49.1%	42.7%	8.2%
High-Hours Firms	47.9%	34.3%	17.8%	47.7%	32.7%	19.6%	46.8%	41.9%	11.3%
Female Workforce									
More Than 80 Percent of Employees Are Women	52.1%	36.6%	11.3%	54.6%	34.0%	11.4%	45.8%	45.0%	9.3%
Other Firms	53.0%	27.3%	19.6%	53.5%	22.3%	24.2%	56.6%	35.0%	8.4%
Diverse Workforce									
50 Percent or More of Employees Are Non-White	54.8%	28.8%	16.4%	58.9%	22.6%	18.5%	46.0%	42.5%	11.6%
Other Firms	47.7%	43.5%	8.9%	45.9%	44.8%	9.3%	51.7%	43.6%	4.6%
Union Workforce									
Some Employees Are Union Members	47.2%	49.5%	3.3%	47.8%	48.9%	3.3%	46.2%	51.0%	2.8%
No Union Members	52.2%	33.8%	13.9%	54.2%	30.5%	15.3%	48.3%	42.3%	9.3%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-hour firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.

Source: IWPR analysis of employer survey data

It is possible that one response to these difficulties lay in non-compliance, and one-quarter of employers who reported difficulties implementing the PSLO also reported providing no paid sick days in the survey.

Smaller firms reported finding it easier to administer the PSLO, but more difficult to adjust workload for absent workers. They were also much more likely to answer that they did not know whether it was difficult to understand or implement the PSLO or to manage workload when workers used paid sick days. The restaurant and hospitality industry showed higher rates of difficulty implementing the PSLO, perhaps because few employers had paid sick days before the ordinance, or because they often needed to hire replacement workers.

Employers were also asked about hiring replacement workers to cover for workers using paid sick days. Few did so; nine out of 10 firms answered that they never or rarely hired replacements when workers used paid sick days (Figure 5 and Appendix Table 3). The hospitality industry was the only industry that significantly used replacements, with just under one-third of firms reporting that they brought in outside workers “always” or “frequently” to replace workers using paid sick days. However, hospitality firms that actually provided paid sick days reported rarely using replacement workers; the firms that did not offer paid sick days were more likely to use this staffing strategy.⁴⁰ The generally low rates of replacement hiring are consistent with the finding that employees reported increased work demands as the most common employer response to the PSLO (21.7 percent overall, and 25.3 percent in the leisure and hospitality industry; see Table 5).

Employer Benefits from the Paid Sick Leave Ordinance

Employer reports of cost-savings or improved employee performance should be limited to the minority of employers who implemented or improved policies in response to the PSLO. It follows that most employers will report only minimal changes in these aspects of business performance, and the data reflect this pattern.

Most employers reported that the PSLO had no effect on the predictability of employee absence, employee morale, customer service, or employees coming to work sick (Table 10 and Appendix Table 4). Approximately one out of seven did not know if complying with the PSLO had affected these aspects of their business.

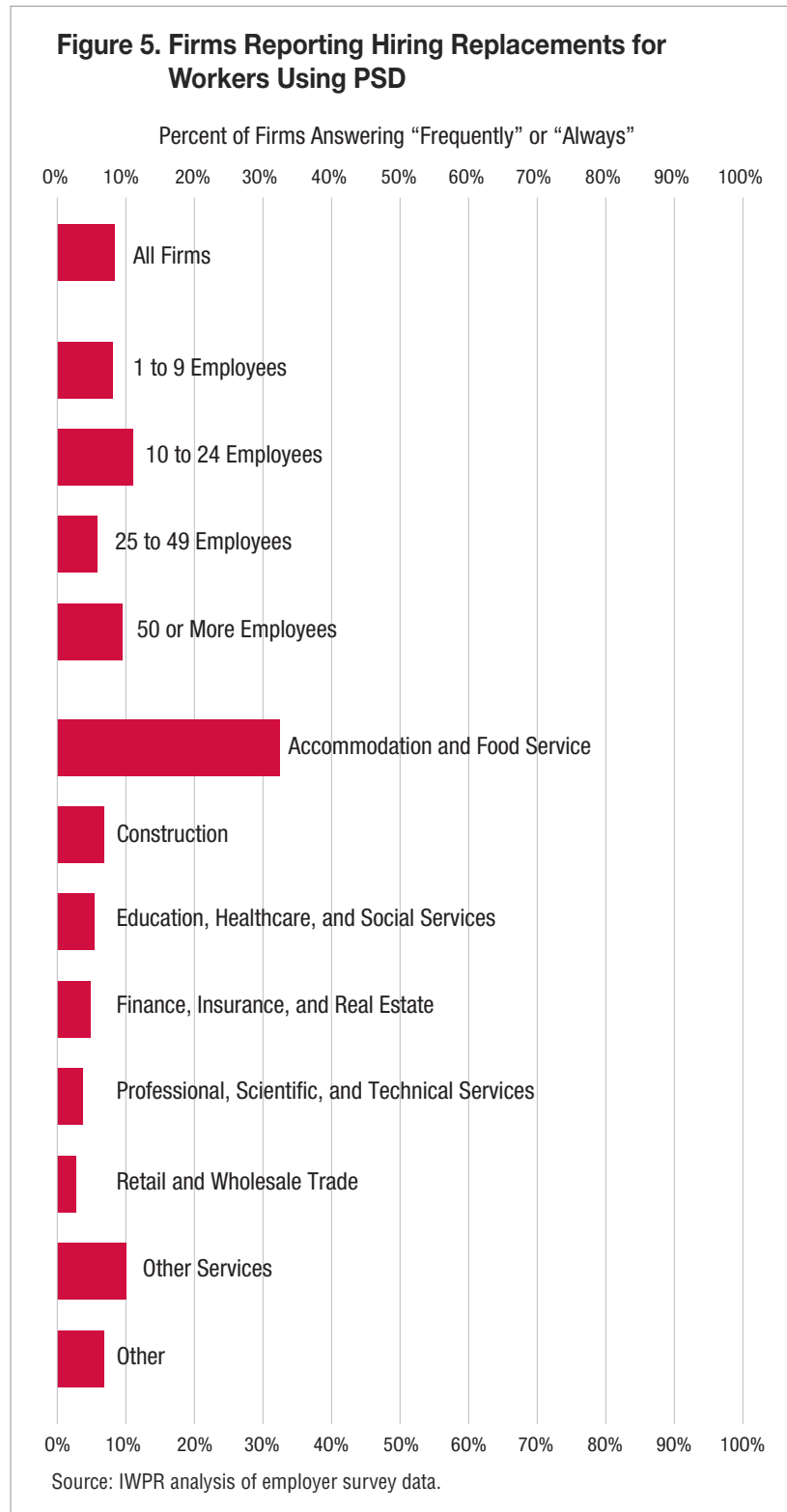


Table 10. Effect of PSLO on the Predictability of Absence

	Impact on Predictability of Employee Absences			
	Better	About the Same	Worse	Don't Know
All Firms	3.8%	75.5%	6.9%	13.9%
Number of Employees				
1 to 9	3.8%	75.7%	3.5%	16.9%
10 to 24	3.3%	74.8%	17.1%	4.8%
25 to 49	5.3%	76.0%	15.0%	3.7%
50 or More	3.3%	72.8%	18.7%	5.2%
Industry				
Accommodation and Food Service	1.6%	65.8%	11.8%	20.7%
Construction	20.2%	54.8%	15.9%	9.2%
Education, Health Care, and Social Services	3.5%	67.5%	11.8%	17.2%
Finance, Insurance, and Real Estate	5.4%	84.4%	2.8%	7.4%
Professional, Scientific, and Technical Services	6.4%	81.0%	1.8%	10.8%
Retail and Wholesale Trade	0.4%	78.1%	6.1%	15.4%
Other Services	1.9%	67.4%	3.5%	27.3%
Other	1.6%	83.8%	12.0%	2.6%
Wage Levels				
Low-Wage Firms	1.8%	76.7%	7.0%	14.5%
High-Wage Firms	5.3%	76.3%	4.7%	13.8%
Firms Work-Hours				
Low-Hours Firms	5.4%	70.5%	10.9%	13.3%
High-Hours Firms	4.4%	79.3%	5.5%	10.8%
Female Workforce				
More Than 80 Percent of Employees Are Women	4.8%	73.8%	7.3%	14.1%
Other Firms	0.2%	80.8%	6.0%	13.0%
Diverse Workforce				
50 Percent or More of Employees Are Non-White	5.0%	77.4%	2.7%	14.9%
Other Firms	1.8%	72.1%	13.0%	13.1%
Has Unionized Workers				
Some Employees Are Union Members	6.2%	74.2%	14.6%	5.1%
No Union Members	3.6%	75.5%	6.5%	14.3%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-hour firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.

Source: IWPR analysis of employer survey data.

Net Effects and Employer Support for the PSLO

As shown below (Table 11), most employers—more than two out of three—reported no impact on profitability, whereas an additional one-seventh reported they did not know if there were any effects. The group that reported positive profitability effects is very small (0.6 percent), and a larger group reported negative effects (14.2 percent). Overall, **six out of every seven San Francisco employers did not report negative profitability effects from the PSLO.**

The industries where reports of adverse profitability effects occurred most frequently were in accommodation and food service, construction, retail and wholesale trade, and other services. Not surprisingly, these are the same industries where new paid sick days were implemented most frequently in response to the PSLO (see Table 8).

Although respondents in this minority of industries reported perceived profitability declines, higher coverage in these industries is especially likely to result in public health gains. Excepting construction, new paid sick days coverage in these industries should have generated substantial public health benefits due to employees working closely with customers. A check revealed that profitability declines were not associated with reports of policy change in response to the PSLO (three-quarters of respondents reporting policy change also reported no impact on profits).

Table 11. Effect of PSLO on Profitability

	Impact on Profitability			
	Better	About the Same	Worse	Don't Know
All Firms	0.6%	70.6%	14.2%	14.6%
Number of Employees				
1 to 9	0.6%	70.3%	12.0%	17.0%
10 to 24	0.2%	71.0%	22.7%	6.2%
25 to 49	2.0%	73.8%	19.9%	4.3%
50 or More	0.4%	70.4%	18.4%	10.9%
Industry				
Accommodation and Food Service	0.0%	64.9%	17.4%	17.7%
Construction	5.7%	62.2%	25.6%	6.6%
Education, Health Care, and Social Services	2.1%	70.4%	6.9%	20.6%
Finance, Insurance, and Real Estate	1.1%	78.6%	7.9%	12.4%
Professional, Scientific, and Technical Services	0.1%	78.2%	10.1%	11.6%
Retail and Wholesale Trade	0.0%	71.3%	16.0%	12.7%
Other Services	0.0%	45.9%	24.0%	30.1%
Other	0.4%	76.2%	17.4%	6.1%
Wage Levels				
Low-Wage Firms	1.1%	63.2%	15.9%	19.8%
High-Wage Firms	0.6%	76.3%	10.4%	12.7%
Firms Work-Hours				
Low-Hours Firms	1.2%	68.8%	17.3%	12.8%
High-Hours Firms	0.7%	70.9%	16.7%	11.8%
Female Workforce				
More Than 80 Percent of Employees Are Women	0.8%	68.0%	16.1%	15.1%
Other Firms	0.2%	77.6%	9.2%	12.9%
Diverse Workforce				
50 Percent or More of Employees Are Non-White	0.3%	71.0%	14.4%	14.3%
Other Firms	1.3%	69.5%	14.9%	14.3%
Has Unionized Workers				
Some Employees Are Union Members	1.1%	56.0%	32.5%	10.4%
No Union Members	0.6%	71.3%	13.3%	14.8%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-hour firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.

Source: IWPR analysis of employer survey data.

Some firms may have reported no adverse profitability effects because they found ways to contain costs or improve revenues while implementing the PSLO. However, more than eight in 10 employers report that they did not make any of the changes asked about in the survey. Further, only 14.1 percent of employees reported that their employer reduced compensation or benefits (see Table 5), while a very similar proportion of employers (12.8 percent) reported reductions in employee compensation (Table 12). Specifically, 2.8 percent reduced vacation leave; 7.1 percent converted vacation leave to paid time off or paid sick days; and 7.1 percent reduced raises or bonuses.

A smaller proportion of employers (10.9 percent) said they increased prices in response to the PSLO (results not shown). These employers were concentrated in the same industries where the PSLO led to the most dramatic expansion in coverage, and where employers reported adverse profitability effects—accommodation and food services, construction, retail and wholesale trade, and other services. In at least some cases, these employers were able to pass along these costs to customers.

Table 12. Employer Changes to Compensation in Response to the PSLO

Firm Characteristics	Decreased Vacation Time	Converted Vacation to PTO or PSD	Reduced Raises or Bonuses	One or More of These Responses		
				Yes	Don't Know	No
All Firms	2.8%	7.1%	7.1%	12.8%	6.6%	80.6%
Number of Employees						
1 to 9	1.4%	5.0%	6.2%	10.1%	7.6%	82.3%
10 to 24	8.8%	15.5%	11.7%	22.7%	4.2%	73.1%
25 to 49	5.9%	11.8%	9.8%	21.5%	2.7%	75.9%
50 or More	5.4%	11.6%	5.5%	17.7%	1.8%	80.6%
Industry						
Accommodation and Food Service	5.1%	6.9%	10.0%	16.4%	20.7%	62.8%
Construction	15.6%	19.8%	15.6%	34.5%	0.0%	65.5%
Education, Health Care, and Social Services	0.4%	12.1%	20.6%	30.1%	2.3%	67.6%
Finance, Insurance, and Real Estate	0.9%	5.1%	0.1%	6.0%	1.0%	92.9%
Professional, Scientific, and Technical Services	1.5%	8.0%	4.3%	9.4%	1.8%	88.8%
Retail and Wholesale Trade	1.0%	2.8%	1.2%	4.1%	5.9%	89.9%
Other Services	2.4%	3.7%	11.1%	13.2%	18.7%	68.1%
Other	6.3%	9.0%	7.8%	13.1%	3.4%	83.4%
Wage Levels						
Low-Wage Firms	4.7%	4.3%	6.6%	11.4%	10.0%	78.7%
High-Wage Firms	1.8%	8.0%	6.7%	13.1%	2.5%	84.4%
Firms Work-Hours						
Low-Hours Firms	2.2%	10.5%	8.1%	18.9%	12.7%	68.4%
High-Hours Firms	3.2%	6.6%	9.2%	13.0%	3.4%	83.6%
Female Workforce						
More Than 80 Percent of Employees Are Women	3.7%	7.8%	6.9%	12.6%	7.9%	79.5%
Other Firms	0.5%	5.3%	8.1%	13.7%	2.8%	83.5%
Diverse Workforce						
50 Percent or More of Employees Are Non-White	1.5%	5.9%	6.4%	10.6%	2.9%	86.6%
Other Firms	5.4%	9.3%	9.2%	17.4%	12.0%	70.6%
Has Unionized Workers						
Some Employees Are Union Members	3.8%	6.9%	8.2%	12.1%	3.2%	84.7%
No Union Members	2.8%	7.1%	7.1%	12.8%	6.8%	80.4%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-hours” firms work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.

Source: IWPR analysis of employer survey data.

Two out of three firms were “supportive” or “very supportive” of the PSLO, of which one-third reported being “very supportive” (Figure 6 and Table 13). The same four industries where reported profitability issues emerged were also those with the lowest levels of support for the PSLO—accommodation and food services, construction, retail and wholesale trade, and other services. However, **even in these industries, more than 60 percent of employers in accommodation and food services and in retail and wholesale trade are supportive, whereas fewer than 30 percent of employers in the construction industry report not being supportive.**⁴¹ Most employers supported the PSLO.

Figure 6. Firms Supporting the PSLO



Source: IWPR analysis of employer survey data.

Table 13. Percentage of Firms Supporting the PSLO

Firm Characteristics	Share of Firms Stating Specified Level of Support			
	Very	Somewhat	Not Supportive	Don't Know
All Firms	33.9%	34.5%	23.6%	8.0%
Number of Employees				
1 to 9	34.9%	33.7%	22.0%	9.4%
10 to 24	27.3%	39.5%	31.5%	1.7%
25 to 49	38.7%	32.5%	24.3%	4.4%
50 or More	33.3%	35.0%	23.0%	8.7%
Industry				
Accommodation and Food Service	12.0%	53.7%	31.6%	2.7%
Construction	7.7%	40.3%	28.1%	23.8%
Education, Health Care, and Social Services	52.2%	32.9%	12.3%	2.6%
Finance, Insurance, and Real Estate	59.9%	23.4%	12.2%	4.6%
Professional, Scientific, and Technical Services	30.3%	43.2%	16.0%	10.5%
Retail and Wholesale Trade	34.4%	27.4%	30.9%	7.2%
Other Services	29.3%	21.7%	35.6%	13.4%
Other	28.5%	32.5%	29.4%	9.6%
Wage Levels				
Low-Wage Firms	30.8%	40.7%	20.3%	8.2%
High-Wage Firms	37.6%	31.4%	19.8%	11.2%
Firms Work-Hours				
Low-Hours Firms	27.2%	39.5%	26.3%	7.0%
High-Hours Firms	34.4%	32.2%	24.1%	9.3%
Female Workforce				
More Than 80 Percent of Employees Are Women	32.0%	34.0%	24.5%	9.5%
Other Firms	39.0%	37.4%	21.1%	2.4%
Diverse Workforce				
50 Percent or More of Employees Are Non-White	39.2%	32.0%	21.9%	7.0%
Other Firms	28.0%	37.1%	27.2%	7.8%
Has Unionized Workers				
Some Employees Are Union Members	23.1%	19.9%	49.3%	7.8%
No Union Members	34.5%	35.3%	22.3%	8.0%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-hours firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.

Source: IWPR analysis of employer survey data.

Among the minority of employers who were not supportive of the PSLO, most (two-thirds) reported hiring replacement workers always or frequently.

The generally high levels of support may reflect the fact that paid sick days are simply becoming a way of doing business in the city, rather than the burden some expected to see.

Conclusion:

How Well Is the Paid Sick Leave Ordinance Working?

San Francisco's Paid Sick Leave Ordinance expanded paid sick leave to thousands of employees, according to both employee and employer reports, although only one in six employers implemented a new paid sick days policy to comply with the ordinance.⁴² Most San Francisco employers reported that implementing the PSLO was not difficult and that it did not negatively affect their profitability. Half of San Francisco workers benefited directly from the PSLO in terms of their ability to use paid sick days—even though more than one-third of workers said they did not use any paid sick days in the previous year. Thus, the evidence presented in this report suggests that the PSLO is functioning as intended to level the playing field across employers and spread the benefits of paid sick days to employees who previously needed but did not have paid sick days.

The PSLO created costs for a relatively small share of employers and employees. Only one in seven employers reported adverse effects on profitability, and a similar proportion of employees reported fewer raises or bonuses or reductions in other benefits. In part, it appears that many employers offset potential costs by having co-workers fill in when others were absent, rather than hiring replacement workers. Of course, these employers may ultimately benefit due to reductions in employee turnover, and by having a healthier and more productive workforce.

The finding that so few employers were adversely affected contradicts speculation from employer groups that paid sick days policies would increase costs among employers who already had paid sick days systems in place.⁴³ The reason this speculation turned out to be unfounded is simple: employees in San Francisco use fewer than half of the sick days available under the PSLO, and employers will never pay for many of these unused days. This finding is not surprising, given the way that workers earn paid time off under the PSLO. Employees can earn a maximum of up to five or nine paid sick days, and carry up to that many days over from one year to the next. This structure gives employees an incentive to treat paid sick days as a form of insurance, saving paid sick days in case they need the time for their own illness or that of a family member. Many workers will end up not having to use any paid sick days in a given year.

Workers' reports of their reasons for using paid sick days suggest that many employees make strategic trade-offs in taking this leave. For instance, workers with caregiving commitments, whether for children or other adults in need, tend to use paid sick days to care for others, not themselves. This underscores the importance of the PSLO's design, as compared to sick leave policies that can be used only for a worker's own illness or injury.

Employer and worker reports suggest that some challenges remain in order to fully implement the PSLO; however, extending public and employer education about the law and employer compliance are paramount. Although most employers appear to be in compliance with the requirements of the PSLO and are supportive of the law, a significant minority of workers report that they still lack paid sick days (see Appendix Table 1). And nearly one in five employers report that they do not offer paid sick days. Finally, it appears that many employers and workers are not familiar with the requirements of the PSLO, despite the outreach conducted by several public agencies to familiarize employers and workers with the law.⁴⁴

If there is a bottom line to San Francisco employer responses to the PSLO, it does not necessarily lie in perceptions of costs, benefits, or profitability, but in whether they support the law. Although most businesses already provide paid sick days in the United States, employer organizations have often opposed mandates such as the PSLO.⁴⁵ However, two-thirds of employers in the only city with experience with a paid sick days mandate for all workers are supportive of the policy.

Appendix A: Methodology for the Survey of Workers

The survey of workers was conducted by telephone by David Binder Research in January and February 2010. The sample frame was constructed by David Binder Research based on zip codes inside and outside San Francisco and included both land-line and cell phone numbers. The survey was designed by the Institute for Women's Policy Research. Survey respondents were at least 18 years old and had worked an average of at least 10 hours per week for at least three months for a private-sector San Francisco firm at some time after February 2007. Interviews were completed with 1,194 workers.

The Institute for Women's Policy Research imputed missing wage data and created post-stratification weights using Stata's raking procedure to represent the demographic (gender, age, ethnicity, and education) and employment (industry, wage, and work hours) characteristics of private-sector San Francisco workers in 2008 according to the American Community Survey.

Appendix Table A: Sample Sizes and Weighted Distribution, Worker Survey

	Sample Size	Weighted Distribution		Sample Size	Weighted Distribution
Total	1,194	100.0%	Industry		
			Information and Prof. and Business Services	353	28.1%
Age			Financial Activities	128	14.6%
18 to 24 Years	41	6.4%	Educational and Health Services	240	14.9%
25 to 54 Years	733	68.4%	Leisure and Hospitality	117	12.3%
55 or Over	420	25.2%	Other Services	164	11.2%
			Other	173	19.0%
Gender			Wage Quartiles		
Men	639	55.2%	Bottom Wage Quartile	189	26.6%
Women	555	44.8%	Second Wage Quartile	296	24.8%
			Third Wage Quartile	372	23.6%
Race and Ethnicity			Top Wage Quartile	337	25.0%
Black	86	5.7%	Union Member		
Latino	108	15.9%	Union	220	17.1%
Other	197	25.4%	Non-Union	963	82.9%
White	761	53.0%	Part-Time		
Parents			Part-Time	281	16.1%
Has Children	307	28.5%	Not Part-Time	913	83.9%
No Children	887	71.5%	Temporary or Seasonal Worker		
Mothers			Temporary or Seasonal Worker	154	14.2%
Mother	150	14.0%	Not Temporary or Seasonal Worker	1,017	85.8%
Not A Mother	1,044	86.0%	College Graduate		
Single Mothers			College Graduate	856	57.4%
Single Mothers	35	3.6%	Not a College Graduate	328	42.6%
Others	1,159	96.4%	Has Paid Vacation or PTO		
Workers with Chronic Health Conditions			Has Paid Vacation or PTO	890	76.2%
Has Chronic Health Condition	281	20.2%	No Paid Vacation or PTO	275	23.8%
No Chronic Health Condition	902	79.8%	Has Health Insurance		
Union Member			Has Health Insurance	1,034	84.2%
Union Member	220	17.1%	No Health Insurance	154	15.8%
Non-Union	963	82.9%	Firm Size		
Firm Size			Less Than 10	320	28.1%
Less Than 10	320	28.1%	10 to 24	197	17.6%
10 to 24	197	17.6%	25 to 99	248	21.1%
25 to 99	248	21.1%	100 or More	429	33.3%
100 or More	429	33.3%			

Source: IWPR analysis of employee survey data.

Appendix B: Methodology for the Survey of Employers

The 2009 Bay Area Employer Health Benefits Survey (BAEHBS) was conducted by telephone by National Research LLC in July through December 2009.⁴⁶ The survey was designed by William H. Dow, Arindrajit Dube, and Carrie Hoverman Colla of the University of California Berkeley as part of an evaluation of San Francisco's Health Care Security Ordinance (HCSO); staff of the Institute for Women's Policy Research collaborated on writing the questions about the PSLO. The sample frame was based on the 2007 Dun and Bradstreet database of businesses. The sample was stratified by non-profit status and firm size, and the survey was targeted at benefits managers. Interviews were completed with 727 San Francisco firms.⁴⁷ The overall response rate was 19 percent among eligible phone numbers attempted.

The Institute for Women's Policy Research created post-stratification weights using Stata's raking procedure to represent the firm-size and industry distribution of San Francisco establishments in 2008 according to the U.S. Census Bureau's County Business Patterns data.

Appendix Table B: Sample Sizes and Weighted Distribution, Employer Survey

	Sample Size	Weighted Distribution
Offers Paid Sick Days		
Paid Sick Days	682	84.2%
No Paid Sick Days	36	15.8%
Total	718	100.0%
Number of Employees		
1 to 9	190	77.2%
10 to 24	206	13.9%
25 to 49	145	4.6%
50 or more	186	4.3%
Total	727	100.0%
Industry		
Accommodation and Food Service	80	12.0%
Construction	37	4.4%
Education, Health Care, and Social Services	86	11.3%
Finance, Insurance, and Real Estate	91	12.9%
Professional, Scientific, and Technical Services	197	19.9%
Retail	46	12.4%
Other	190	27.1%
Total	727	100.0%
Wage Levels		
Low-Wage Firms	167	19.6%
Other Firms	192	24.0%
High-Wage Firms	368	56.5%
Total	727	100.0%
Firms Work-Hours		
Low-Hours Firms	166	19.9%
Other Firms	239	33.1%
High-Hours Firms	300	47.0%
Total	705	100.0%
Hires Temporary Workers		
Hires Temporary Workers	138	14.9%
Does Not Hire Temporary Workers	412	85.1%
Total	550	100.0%
Offers Health Insurance		
Offers Health Insurance	630	60.8%
No Health Insurance	97	39.2%
Total	727	100.0%
Firm Has More Than 80 Percent Women?		
More Than 80 Percent Women	109	24.3%
Other Firms	596	75.7%
Total	705	100.0%
25% or More of Employees Are Workers of Color		
More Than 25 Percent Workers of Color	473	64.0%
Other Firms	254	36.0%
Total	727	100.0%

Source: IWPR analysis of employer survey data.

Appendix Table 1: Workers Who Can Use for Specified Purpose and Earn at the Required Rate

	Has PSD for Own Health			Has PSD for Family Care			Has PSD To See Doctor		
	Yes	No	Don't Know	Yes	No	Don't Know	Yes	No	Don't Know
All Workers	79.3%	15.4%	5.3%	63.2%	25.2%	11.6%	68.8%	24.9%	6.4%
Age									
Age 25 to 54 Years	80.8%	14.3%	5.0%	66.2%	22.8%	10.9%	70.0%	23.5%	6.5%
Age 55 or Over	78.1%	17.8%	4.1%	56.9%	31.8%	11.4%	66.2%	28.6%	5.3%
Sex									
Men	78.0%	16.9%	5.0%	60.1%	29.0%	10.9%	67.4%	27.3%	5.3%
Women	80.9%	13.5%	5.6%	67.0%	20.5%	12.5%	70.5%	21.9%	7.6%
Race/Ethnicity									
Black	87.3%	10.6%	2.1%	72.7%	20.1%	7.2%	69.5%	25.3%	5.3%
Latino	76.4%	20.2%	3.4%	57.8%	33.9%	8.3%	55.3%	36.8%	8.0%
Other	80.8%	12.7%	6.5%	66.2%	23.1%	10.7%	72.8%	21.9%	5.3%
White	78.6%	15.4%	6.0%	61.9%	24.2%	13.9%	70.1%	22.7%	7.1%
Parental Status									
Parents	75.2%	17.9%	7.0%	68.4%	23.5%	8.1%	71.4%	23.8%	4.8%
Mothers	77.0%	18.5%	4.5%	72.5%	21.4%	6.1%	77.1%	19.3%	3.6%
Single Mothers	67.5%	28.9%	3.6%	59.8%	34.4%	5.8%	53.9%	43.5%	2.6%
Chronically Ill	77.1%	19.6%	3.3%	56.0%	30.9%	13.1%	70.5%	24.5%	5.1%
Union Members	86.0%	11.0%	3.1%	62.1%	26.6%	11.2%	63.8%	30.8%	5.4%

Source: IWPR analysis of employer survey data.

Appendix Table 1 (cont.): Workers Who Can Use for Specified Purpose and Earn at the Required Rate

	Has All PSD Uses			Accrues at Least 1/30 Hours			Has All Uses and Accrues			N
	Yes	No	Don't Know	Yes	No	Don't Know	Yes	No	Don't Know	
All Workers	53.3%	31.2%	15.6%	64.3%	19.1%	16.6%	44.1%	29.0%	26.9%	1,172
Age										
Age 25 to 54 Years	56.2%	28.6%	15.1%	66.0%	17.2%	16.8%	46.4%	26.6%	27.0%	721
Age 55 or Over	47.1%	37.5%	15.4%	67.2%	20.8%	12.0%	43.2%	34.9%	21.9%	342
Sex										
Men	51.2%	33.7%	15.0%	61.2%	21.0%	17.8%	40.4%	31.4%	28.2%	623
Women	55.7%	28.1%	16.2%	68.2%	16.8%	15.1%	48.6%	26.1%	25.3%	549
Race/Ethnicity										
Black	52.3%	35.8%	11.9%	64.0%	15.5%	20.5%	40.1%	35.9%	24.1%	86
Latino	44.3%	41.8%	13.9%	60.8%	23.9%	15.3%	33.5%	42.0%	24.5%	108
Other	55.3%	31.0%	13.8%	66.1%	16.7%	17.2%	46.0%	25.3%	28.7%	194
White	54.6%	27.6%	17.8%	64.5%	19.1%	16.4%	46.9%	25.6%	27.5%	745
Parents	58.1%	30.7%	11.1%	63.6%	21.5%	14.8%	47.6%	30.0%	22.4%	305
Mothers	62.0%	29.6%	8.5%	69.9%	17.8%	12.2%	54.1%	28.9%	17.0%	149
Single Mothers	42.8%	51.3%	5.8%	47.5%	24.7%	27.8%	29.9%	40.0%	30.0%	35
Chronically Ill	47.8%	35.7%	16.5%	65.2%	20.9%	13.9%	41.7%	32.5%	25.8%	276
Union Members	50.1%	35.9%	14.1%	71.4%	13.0%	15.6%	45.3%	27.7%	27.0%	218

Appendix Table 2: How Firms Changed Their PSD Policies in Response to the PSLO

Firm Characteristics	Provides PSD	Enacted a New PSD Policy	Increased the Accrual Rate for an Existing Policy	Expanded an Existing Policy to Cover More Workers	Firm Made Changes?		
					One or More	No	Not Sure if Changes Were Made
All Firms	82.1%	17.0%	15.8%	17.1%	30.6%	63.7%	5.7%
Number of Employees							
1 to 9	78.4%	15.1%	11.9%	11.3%	24.3%	70.0%	5.8%
10 to 24	92.0%	25.9%	30.0%	32.2%	49.8%	45.6%	4.6%
25 to 49	97.5%	23.2%	27.6%	38.8%	49.9%	47.4%	2.7%
50 or More	99.4%	15.8%	25.8%	46.5%	58.4%	31.1%	10.5%
Industry							
Accommodation and Food Service	62.1%	38.5%	17.4%	23.3%	46.0%	30.3%	23.7%
Construction	69.3%	39.5%	50.0%	28.2%	66.3%	29.9%	3.8%
Education, Health Care, and Social Services	89.2%	21.5%	13.6%	14.2%	32.6%	66.3%	1.1%
Finance, Insurance, and Real Estate	93.2%	-6.2%	8.2%	8.4%	10.8%	87.3%	1.9%
Professional, Scientific, and Technical Services	85.3%	5.4%	10.9%	13.7%	20.7%	76.3%	3.0%
Retail and Wholesale Trade	77.9%	16.4%	15.6%	17.3%	32.4%	65.7%	1.9%
Other Services	78.5%	24.0%	33.6%	27.9%	49.7%	41.1%	9.2%
Other	91.6%	12.0%	9.4%	16.6%	24.3%	72.4%	3.3%
Wage Levels							
Low-Wage Firms	69.7%	18.6%	19.0%	17.4%	35.0%	57.7%	7.3%
High-Wage Firms	87.1%	15.7%	15.3%	14.3%	26.4%	70.3%	3.3%
Firms Work-Hours							
Low-Hours Firms	73.7%	21.1%	14.8%	16.7%	30.9%	62.8%	6.2%
High-Hours Firms	88.7%	10.1%	13.9%	15.2%	24.1%	72.1%	3.8%
Female Workforce							
More Than 80 Percent of Employees Are Women	82.3%	14.6%	16.2%	17.9%	30.1%	62.5%	7.5%
Other Firms	80.8%	24.0%	14.6%	15.4%	32.9%	66.9%	0.2%
Diverse Workforce							
50 Percent or More of Employees Are Non-White	84.5%	11.6%	16.4%	13.1%	24.7%	73.7%	1.6%
Other Firms	76.1%	27.4%	14.3%	23.1%	40.8%	48.3%	10.9%
Has Unionized Workers							
Some Employees Are Union Members	91.8%	11.4%	32.8%	48.8%	62.0%	37.1%	0.8%
No Union Members	81.7%	17.3%	14.9%	15.5%	29.1%	65.0%	5.9%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn at least \$15 per hour. More than 30 percent of employees in “low-hours firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.

Source: IWPR analysis of employer survey data.

Appendix Table 3: Firms Rarely Hire Replacement Workers

Firm Characteristics	Share of Firms Hiring Outside Replacements for Workers Using PSD:				
	Always	Frequently	Rarely	Never	Don't Know
All Firms	1.2%	7.2%	23.6%	65.6%	2.4%
Number of Employees					
1 to 9	0.7%	7.4%	21.0%	68.1%	2.8%
10 to 24	3.3%	7.7%	28.8%	59.6%	0.5%
25 to 49	3.3%	2.5%	35.8%	57.0%	1.3%
50 or More	1.5%	8.0%	38.9%	50.3%	1.3%
Industry					
Accommodation and Food Service	1.6%	30.8%	25.5%	41.9%	0.2%
Construction	0.0%	6.9%	22.7%	70.5%	0.0%
Education, Health Care, and Social Services	1.1%	4.3%	28.5%	65.9%	0.2%
Finance, Insurance, and Real Estate	0.0%	4.8%	36.6%	57.1%	1.5%
Professional, Scientific, and Technical Services	1.7%	2.0%	34.1%	57.5%	4.8%
Retail and Wholesale Trade	1.4%	1.4%	8.5%	88.6%	0.2%
Other Services	0.0%	10.0%	8.3%	72.8%	8.9%
Other	2.4%	4.4%	21.3%	69.8%	2.1%
Wage Levels					
Low-Wage Firms	1.1%	14.4%	12.5%	66.4%	5.6%
High-Wage Firms	1.0%	2.1%	24.1%	70.6%	2.1%
Firms Work-Hours					
Low-Hours Firms	2.6%	13.9%	19.2%	62.4%	2.0%
High-Hours Firms	0.7%	3.4%	25.3%	66.6%	4.0%
Female Workforce					
More Than 80 Percent of Employees Are Women	1.0%	9.1%	25.5%	62.5%	2.0%
Other Firms	2.0%	2.2%	16.4%	76.5%	3.0%
Diverse Workforce					
50 Percent or More of Employees Are Non-White	0.6%	6.5%	19.3%	72.1%	1.4%
Other Firms	2.3%	9.4%	26.8%	59.1%	2.5%
Has Unionized Workers					
Some Employees Are Union Members	5.1%	5.2%	27.1%	62.0%	0.5%
No Union Members	1.0%	7.3%	23.4%	65.8%	2.5%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-hours firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.

Source: IWPR analysis of employer survey data.

Appendix Table 4: Effect of the PSLO on Business Operations

Firm Characteristics	Impact on Employee Morale				Impact on Customer Service				Impact on Presenteeism			
	Better	About the Same	Worse	Don't Know	Better	About the Same	Worse	Don't Know	Better	About the Same	Worse	Don't Know
All Firms	3.2%	82.9%	0.9%	13.0%	1.2%	81.9%	2.6%	14.3%	3.3%	80.4%	3.4%	12.9%
Number of Employees												
1 to 9	2.3%	81.6%	0.2%	15.9%	0.9%	81.1%	0.9%	17.1%	2.1%	81.4%	1.5%	15.0%
10 to 24	4.4%	89.1%	3.0%	3.5%	1.9%	82.4%	10.3%	5.4%	6.6%	77.0%	9.4%	7.0%
25 to 49	8.6%	83.2%	3.3%	4.9%	4.2%	86.0%	5.6%	4.2%	9.4%	77.1%	9.9%	3.7%
50 or More	7.5%	83.5%	3.5%	5.5%	0.3%	89.0%	1.9%	8.9%	5.5%	77.5%	7.8%	9.1%
Industry												
Accommodation and Food Service	2.8%	76.6%	0.8%	19.7%	2.4%	73.8%	4.1%	19.7%	4.9%	69.9%	4.6%	20.5%
Construction	5.6%	73.6%	8.6%	12.2%	0.0%	83.1%	11.2%	5.6%	6.1%	77.3%	10.0%	6.6%
Education, Health Care, and Social Services	3.1%	77.8%	1.1%	17.9%	1.4%	80.4%	0.7%	17.5%	2.2%	78.1%	2.8%	16.9%
Finance, Insurance, and Real Estate	6.7%	84.6%	0.1%	8.6%	2.4%	87.6%	1.4%	8.6%	0.6%	88.7%	2.8%	7.9%
Professional, Scientific, and Technical Services	2.0%	87.0%	0.7%	10.2%	0.9%	87.3%	0.7%	11.2%	2.9%	86.0%	0.6%	10.5%
Retail and Wholesale Trade	2.1%	82.5%	0.6%	14.8%	0.0%	84.4%	1.4%	14.2%	1.3%	82.9%	4.7%	11.0%
Other Services	1.6%	80.1%	0.0%	18.3%	0.8%	68.1%	2.1%	29.1%	10.7%	68.8%	0.6%	19.9%
Other	4.2%	91.9%	1.2%	2.8%	1.3%	85.2%	7.3%	6.1%	1.8%	82.6%	7.0%	8.7%
Wage Levels												
Low-Wage Firms	3.0%	85.9%	1.6%	9.4%	2.0%	82.2%	2.5%	13.3%	2.4%	84.8%	3.6%	9.3%
High-Wage Firms	2.7%	82.6%	0.7%	14.0%	0.7%	83.8%	1.2%	14.3%	4.2%	80.9%	1.8%	13.1%
Firms Work-Hours												
Low-Hours Firms	6.3%	79.5%	1.1%	13.1%	0.8%	82.2%	3.7%	13.4%	3.0%	77.3%	5.5%	14.2%
High-Hours Firms	1.7%	85.8%	1.3%	11.3%	1.3%	84.5%	2.6%	11.6%	4.9%	82.3%	2.0%	10.8%
Female Workforce												
More Than 80 Percent of Employees Are Women	3.5%	82.4%	1.2%	12.9%	1.6%	81.0%	3.3%	14.1%	2.7%	80.6%	4.1%	12.6%
Other Firms	2.4%	84.4%	0.2%	13.0%	0.1%	84.5%	0.5%	14.9%	5.3%	79.5%	1.3%	13.9%
Diverse Workforce												
50 Percent or More of Employees Are Non-White	3.8%	82.9%	0.7%	12.5%	0.7%	83.0%	1.3%	15.0%	4.0%	82.4%	1.1%	12.5%
Other Firms	2.8%	81.7%	1.0%	14.5%	2.1%	79.1%	4.5%	14.3%	2.5%	77.2%	6.5%	13.8%
Has Unionized Workers												
Some Employees Are Union Members	2.6%	84.7%	2.8%	9.9%	0.3%	91.9%	4.0%	3.9%	2.3%	89.4%	4.7%	3.6%
No Union Members	3.3%	82.8%	0.8%	13.1%	1.3%	81.4%	2.5%	14.8%	3.4%	79.9%	3.4%	13.4%

Note: At least one worker in “low-wage firms” earns less than \$10 per hour; all employees in “high-wage firms” earn more than \$15 per hour. More than 30 percent of employees in “low-wage firms” work less than 10 hours per week; in “high-hours firms,” all employees work at least 30 hours per week.
 Source: IWPR analysis of employer survey data.

NOTES

- ¹ National Partnership for Women & Families. “Current Paid Sick Days Laws.” <http://www.nationalpartnership.org/site/Page-Server?page-name=psd_campaigns>. (modified July 2010).
- ² Employees who perform work in the City/County of San Francisco are covered by the PSLO beginning 90 calendar days after the date of hire, regardless of where their employer or usual workplace is located. Note that federal, state, and city/county government workers in San Francisco had paid sick days before the PSLO was adopted.
- ³ San Francisco Administrative Code Chapter 12W.
- ⁴ DeBare, Ilana. 2008. “S.F. Sick Leave Law Celebrates 1 Year,” *The San Francisco Chronicle*, February 6. Interviews with 26 employers in March 2008 found that “most employers were able to implement [the PSLO] with minimal impacts on their business in the first year.” Waters Boots, Shelley, Karin Martinson, and Anna Danziger. 2009. *Employers’ Perspectives on San Francisco’s Paid Sick Leave Policy*. Washington, DC: The Urban Institute, 12.
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- ⁷ Li, Jiehui, Guthrie S. Birkhead, David S. Strogatz, and R. Bruce Coles. 1996. “Impact of Institution Size, Staffing Patterns, and Infection Control Practices on Communicable Disease Outbreaks in New York State Nursing Homes.” *American Journal of Epidemiology* 143 (May): 1047.
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- ¹⁷ Drago, Robert and Kevin Miller. 2010. *Sick at Work: Infected Employees in the Workplace During the H1N1 Pandemic*. IWPR Publication #B264. Washington, DC: Institute for Women’s Policy Research.
- ¹⁸ Harter, James K., Frank L. Schmidt, and Theodore L. Hayes. 2002. “Business-Unit-Level Relationship Between Employee Satisfaction, Employee Engagement, and Business Outcomes: A Meta-Analysis.” *Journal of Applied Psychology* 87(2): 273; Artz, Benjamin. 2010. “Fringe Benefits and Job Satisfaction.” *International Journal of Manpower* 31(6): 626-27.
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- ²⁰ Cooper, Philip F. and Alan C. Monheit. 1993. “Does Employment-Related Health Insurance Inhibit Job Mobility?” *Inquiry* 30 (Winter):409; Earle, Alison, John Z. Ayanian, and Jody Heymann. 2006. “Work Resumption After Newly Diagnosed Coronary Heart Disease: Findings on the Importance of Paid Leave.” *Journal of Women’s Health* 15(4): 436.
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- ²³ “Chronic health conditions” are conditions like heart disease, asthma, or diabetes that require continuous treatment, are not curable, and may last for a lifetime.
- ²⁴ The PSLO provides that workers accrue leave at the rate of one hour of leave for every 30 hours worked, to a maximum of five days for workers in firms with fewer than 10 employees and nine days for workers in larger firms. If a worker uses some of that accrued time, accrual begins again, with the same caps on the amount that can be accrued. San Francisco Office of Labor Standards Enforcement. 2007. *San Francisco Paid Sick Leave Ordinance, Administrative Code Chapter 12W, Frequently Asked Questions* (September 28), 5–6. An individual working 40 hours a week throughout the year would accrue just under nine days (40 hours per week x 52 weeks per year, divided by 30 hours worked per paid sick days earned, equals 8.7 days). Workers use a median of three paid sick days per year regardless of establishment size. We estimate that 28 percent of workers are in small establishments where they earn an average of five days (leaving two days unused), while the remaining 72 percent of workers earn nine days (leaving six days unused). Weighting unused days by the relevant proportions of workers yields an average of 4.9 unused days each year, compared with three days used.

- ²⁵ Institute for Women’s Policy Research. 2010. Analysis of data from the 2008 National Health Interview Survey.
- ²⁶ Older workers are nearly twice as likely to have a chronic health condition (30.5 percent do) as prime-working-age workers (17.4 percent), but other research shows that older workers are less likely to have acute illnesses, injuries, or accidents than younger workers. Edelstein, Barry A., Ronald R. Martin, and Lesley P. Koven. 2003. “Psychological Assessment in Geriatric Settings,” in *Handbook of Psychology, Volume 10: Assessment Psychology*, ed. John R. Graham, Jack A. Naglieri, and Irving B. Weiner. Hoboken, NJ: John Wiley & Sons, Inc., 390. These facts may explain why older workers were more likely than younger workers to report not using any PSD, but, at the same time, to report using more days, if they did take PSD.
- ²⁷ For example, single parents and low-income families are associated with low rates of job tenure. See Holzer, Harry J. and Karin Martinson. 2005. *Can We Improve Job Retention and Advancement Among Low-Income Working Parents?* Washington, DC: The Urban Institute, 3.
- ²⁸ Survey respondents were asked to report a reason for every leave they took, so the percentages taking different types of leave sum to more than 100 percent.
- ²⁹ These visits could have been for either the respondent or a family member.
- ³⁰ The PSLO does not mandate paid leave for these uses, but they may have been authorized by the policies for these particular workers. For instance, paid sick days may be provided under a general paid time off or PTO plan, and some employers may allow their workers to use paid sick days for vacation time.
- ³¹ Some parents may try not to use PSD for themselves, saving their paid time off for when their children will need care.
- ³² To some extent, workers in San Francisco had that flexibility before the PSLO was passed. Under a California law passed before the PSLO, employers are required to allow workers to use up to half of their accrued sick leave to care for sick family members. Labor Project for Working Families. 2008. *Use of Sick Leave for Family Care/Kin Care* (March). <http://www.working-families.org/learnmore/paid-familyleave/e_sick.pdf> (accessed January 4, 2011).
- ³³ Preventive care has been found to be associated with fewer employee days of unplanned absences, lower productivity loss due to absenteeism, and higher returns to shareholders. See National Business Group on Health and Towers Watson. 2010. *2009/2010 Staying@Work Report: The Health and Productivity Advantage*. New York: Watson Wyatt Worldwide.
- ³⁴ Data in this section are for workers who had the same employer before and after the PSLO went into effect.
- ³⁵ Labor Project for Working Families. May 10, 2008. “Working Moms Speak Out for Paid Sick Days.” Press Release. <http://www.paid-sickdaysca.org/media/press/press_release5_08.pdf> (accessed January 25, 2011).
- ³⁶ Close to half (45.9 percent) of workers who interacted with the public were employed in the education or leisure and hospitality industries. In contrast, workers without public contact were primarily employed in the information and financial activities industries (59.7 percent).
- ³⁷ Perhaps this result is explained by employees who are reliant upon customers’ tips needing that income, and so come to work while sick.
- ³⁸ Low-wage workers were those in the bottom wage quartile, who earned less than \$15 per hour (2010 dollars). There were 118,000 low-wage workers in San Francisco in 2009.
- ³⁹ This estimate assumes that all employers starting new PSD policies provided PSD to all their employees. It does not reflect the effect of employers extending their existing PSD policies to cover more workers.
- ⁴⁰ Among the two-thirds of hospitality firms that actually provided paid sick days, just 3.6 percent reported hiring replacements “frequently” or “always” for workers using sick leave, implying that employers who had not implemented the PSLO were those hiring replacement workers most frequently.
- ⁴¹ Note that many employers in the construction industry (just under one-quarter) reported “don’t know” in terms of support, whereas 51.0 percent of employers in the other services industry were supportive of the PSLO.
- ⁴² 17.9 percent of employers said that they did not have a paid sick days policy at the time of the survey, 18 months after the ordinance went into effect.
- ⁴³ Partnership for New York City. 2010. *Impact of Paid Sick Leave on NYC Businesses: A Survey of New York City Employers* (September), 6.
- ⁴⁴ For example, the San Francisco Office of Labor Standards Enforcement (OLSE) conducted a public rulemaking process for the PSLO; the OLSE and San Francisco’s Mayor’s Office of Economic and Workforce Development distributed informational brochures and employee fact sheets to employers and advertised the PSLO in local newspapers and in bus shelters; the OLSE and the San Francisco Department of Public Health and the OLSE worked with community-based organizations to educate workers about the PSLO. New York City Council Committee on Civil Service and Labor. 2009. *Transcript of the Minutes of the Committee on Civil Service and Labor: Provision of Paid Sick Time Earned by Employees* (November 17), 4.
- ⁴⁵ For example, a study conducted by the Partnership for New York City was anything but supportive of a proposed New York City law that was similar to the PSLO, and the Society for Human Resource Management has opposed similar legislation at the national level. See Partnership for New York City. 2010. *Impact of Paid Sick Leave on NYC Businesses: A Survey of New York City Employers* (September); and Society for Human Resource Management. 2009. *SHRM to Seek Alternative to Healthy Families Act to Encourage Paid Leave* (May 18). <<http://www.shrm.org/about/pressroom/PressReleases/Pages/AlternativetoHealthyFamiliesAct.aspx>> (accessed January 1, 2011).
- ⁴⁶ For more information about the BAEHBS, see Dow, William H., Arindrajit Dube, and Carrie Hoverman Colla. 2010. *2009 Bay Area Employer Health Benefits Survey: Health Benefits Report 2009*. Berkeley: University of California, Berkeley School of Public Health (May).
- ⁴⁷ In addition, 283 non-San Francisco firms completed surveys.

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By LeaAnne DeRigne, Patricia Stoddard-Dare, and Linda Quinn

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Workers Without Paid Sick Leave Less Likely To Take Time Off For Illness Or Injury Compared To Those With Paid Sick Leave

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ABSTRACT Paid sick leave is an important employer-provided benefit that helps people obtain health care for themselves and their dependents. But paid sick leave is not universally available to US workers. Little is known about paid sick leave and its relationship to health behaviors. Contrary to public health goals to reduce the spread of illness, our findings indicate that in 2013 both full- and part-time working adults without paid sick leave were more likely than workers with that benefit to attend work when ill. Those without paid sick leave were 3.0 times more likely to forgo medical care for themselves and 1.6 times more likely to forgo medical care for their family compared to working adults with paid sick leave benefits. Moreover, the lowest-income group of workers without paid sick leave were at the highest risk of delaying and forgoing medical care for themselves and their family members. Policy makers should consider the potential public health implications of their decisions when contemplating guaranteed sick leave benefits.

The decision of when to seek medical care is a complex one made only more complicated by whether or not a person has access to paid sick leave benefits through an employer. Paid sick leave allows employees to leave work to seek care or recuperate at home without losing wages. Of twenty-two countries ranked highly in terms of economic and human development, the United States is the only country that does not mandate employers to provide paid sick leave.¹ Workers in as many as 145 countries have some paid sick days through either employer-mandated sick leave or national social insurance plans.² For many Americans, a day off work translates into lost wages or jeopardized employment.

Seventy percent of the US civilian population working full time have paid sick leave benefits, while only 19 percent of part-time workers have this benefit. That leaves nearly forty-nine million workers without access to paid sick leave.³ Employees in higher-wage jobs or at larger employ-

ers have higher rates of paid sick leave than do their counterparts in lower-wage jobs or employed at smaller companies.³ The Family and Medical Leave Act (FMLA) of 1993 mandates that companies with more than fifty employees offer workers up to twelve weeks of unpaid job-protected leave. However, the FMLA does not address short-term sick leave or leave for routine or preventive care.⁴ Given these restrictions, the FMLA should not be considered a substitute or proxy for paid sick leave.

Access to paid sick leave varies by race, with Hispanic workers having the lowest rates of coverage.³ This health-related employment benefit is also less common among those who are younger, less educated, low income, in fair or poor health, and uninsured.⁵ Only three out of ten low-income workers with a child in fair or poor health have paid sick leave benefits.⁶

Workers use paid sick leave benefits to care for their own health needs or the health needs of their family members. This is particularly impor-

tant to the nearly 50 percent of US adults who have one or more chronic health conditions⁷ and the more than half of working Americans who provide care to a child or family member.⁸ Paid sick leave allows workers (and presumably their dependent family members) to receive prompt preventive or acute medical care, recuperate from illness faster, and avert more serious illness.²

The American Public Health Association endorses and advocates for paid sick leave benefits as a public health policy.⁹ Although peer-reviewed studies on this topic are limited, evidence suggests that benefits of paid sick leave include increased job stability and employee retention following illness, injury, or birth of a child;¹⁰ increased worker productivity;¹¹ decreased accidents or injuries on the job;¹³ and increased mental and physical health of caregivers.¹⁴ Additionally, when used to augment maternity leave, paid leave increases well-baby visits, maternal health, and the duration of breastfeeding, while also decreasing infant mortality.¹⁵

This study extended the limited body of existing research and examined the relationship between paid sick leave benefits and delays in care and forgone care for both working adults and their family members. It also examined the risk of emergency department (ED) use and the risk of missing work because of illness or injury by paid sick leave status. Finally, we analyzed interaction effects between paid sick leave and family income and health insurance.

Study Data And Methods

SAMPLING AND DATA COLLECTION Our study used data from the National Health Interview Survey (NHIS), an ongoing data collection initiative that began in 1957 and was designed to provide data about a broad range of health topics. The 2013 NHIS cross-sectional sample used multi-stage area probability sampling to derive a representative sample of the civilian noninstitutionalized US population. Black, Hispanic, and Asian people were oversampled in the adult sample.

NHIS survey interviews were conducted at the household, family, and individual levels. One randomly selected adult from each family was interviewed on a complementary set of questions. Interviews were conducted on an ongoing basis by trained Census Bureau employees either in person or by phone.

MEASURES The thirteen control variables included in the analyses were sex of adult respondent, highest level of education attained (seven indicators ranging from eighth grade or less to doctorate), race and ethnicity (Hispanic, non-

Hispanic white, non-Hispanic black, non-Hispanic other), marital status (yes/no), family size, occupation of adult respondent (five categories based on the Standard Occupational Classification system), obesity in adult respondent (body mass index greater than 30 kg/m²), full-time work status of adult respondent (yes indicated worked thirty-five hours or more previous week, no indicated worked fewer than thirty-five hours in past week), health insurance coverage status of adult respondent (insured or uninsured, meaning that no health insurance under private health insurance, Medicare, Medicaid, State Children's Health Insurance Program, a state-sponsored health plan, other government programs, or military health plan was reported), health status of adult respondent (excellent, very good, or good versus fair or poor), presence of a limiting condition in adult respondent (yes meaning limited in any way, no meaning not limited in any way), total annual family income (less than \$35,000, \$35,000 to less than \$75,000, \$75,000 to less than \$100,000, \$100,000 or more), and age of adult respondent (in years).¹⁶ The indicator measured for all analyses was self-reported paid sick leave status (yes/no). These control variables were selected based on theory and past empirical findings as being related to the outcome variables. An interaction between paid sick leave and family income was added since losing a few days of wages might be less relevant in deciding whether to visit a doctor or not for high-income respondents, but for low-income respondents, family income and the absence of paid sick leave would likely be much more significant.

The six outcome variables examined were as follows: respondent delayed medical care in past twelve months because of cost (yes/no), family member delayed medical care in past twelve months because of cost (yes/no), respondent needed medical care but did not get it in past twelve months because of cost (yes/no), family member needed medical care but did not get it in the past twelve months because of cost (yes/no), respondent had an ED visit in past twelve months (yes/no), and number of days respondent missed work at job or business because of injury or illness (excluding maternity leave). We examined these variables because of their important implications for public health.

Measurement limitations included the reliance on self-reported data for all measures, so the question of accuracy was raised. We found a similar percentage of the workforce with paid sick leave as the Bureau of Labor Statistics found. An additional limitation was that some variables, such as ED usage, pertained to only the respondent, not all household members. Finally, the

70%

Have paid sick leave
Among the US civilian population working full time, 70 percent have paid sick leave benefits. Only 19 percent of part-time workers have this benefit.

data we used were cross-sectional; therefore, causation cannot be established.

ANALYTIC SAMPLE Three 2013 NHIS core questionnaires were used: family core (all related family members in the same household), person core (all individuals within a family), and sample adult (age eighteen or older randomly selected from family). The analytic sample included 18,655 adults ages 18–64 with current paid employment selected from the sample adult file. Those working without pay, working in a family business, self-employed, looking for work, or not working were excluded.

DATA ANALYSIS We estimated five multivariable logistic regression equations and one multivariable regression equation. We used the same control variables (sex, education, race/ethnicity, marital status, family size, occupation, full-time work status, health insurance status, health status, obesity, limiting health condition, family income, and age) and one predictor variable (paid sick leave status) in all six equations. The outcome variables are described above, in the Measures section. Interaction effects were added between paid sick leave status and family income and between paid sick leave status and insurance coverage.

Study Results

We identified 10,586 working adults (57.3 percent) with paid sick leave benefits and 7,879 (42.7 percent) without. Their full demographic profile is shown in online Appendix Exhibit 1.¹⁷ Those without paid sick leave were more likely to be male, unmarried, less educated, and Hispanic; hold service occupations; work part time; be uninsured; have fair or poor health; have a limiting health condition; and have lower incomes (Exhibit 1). Nearly 65 percent of families with incomes below \$35,000 had no paid sick leave, compared to 25 percent of families who earned more than \$100,000 a year. This disparity left the most economically vulnerable without the protective benefit of paid sick leave.

When considering the respondent's delaying medical care for themselves or a family member in the past twelve months, we found that those who did not receive a paid sick leave benefit had a significant ($p < 0.05$) increase in predicted risk of delaying medical care (0.3 percent versus 0.9 percent) for themselves and a family member (0.8 percent versus 1.6 percent) compared to those with paid sick leave benefits (Exhibit 2). (See Appendix Exhibits 2 and 3 for full regression results.)¹⁷ While predicted risks were significantly increased when the respondent was uninsured, there was no significant interaction effect between paid sick leave benefits and insur-

For many Americans, a day off work translates into lost wages or jeopardized employment.

ance status. Predicted risks were highest for the lowest family annual income category and significantly lower as family income increased ($p < 0.05$). There was also a significant interaction between family income and paid sick leave benefits ($p < 0.05$). The gap between predicted risks of delaying medical care was significantly smaller for those with less than \$35,000 family income (11.1 percent without paid sick leave versus 9.8 percent with paid sick leave for those without insurance coverage; 2.5 percent without paid sick leave versus 2.4 percent with paid sick leave for those with insurance coverage) compared to all other income levels ($p < 0.05$). As a depiction of the interactive effects among paid sick leave benefits, income, and insurance status, the predicted risks (from Exhibit 2) of the respondent's delaying medical care are shown in Exhibit 3. The significance of these findings was true not only for delaying medical care but also for the respondent's forgoing needed care (see Appendix Exhibit 4 for full regression results and Appendix Exhibit 9 for a depiction of the interactive effects).¹⁷ While paid sick leave was important, having insurance also had a major impact on the respondent's forgoing needed care, especially for low-income respondents. This was true for those with and without paid sick leave benefits and across income groups. The risk of forgoing needed care because of costs dropped from 16.5 percent for those without insurance to 3.1 percent for those with insurance among low-income adults without paid sick leave and 13.5 percent for those without insurance to 2.8 percent for those with insurance among low-income adults with sick leave.

When considering the risk of a family member's delaying medical care or forgoing medical care, we observed the same pattern and significance of risks as for the respondent, with the exception that the interaction between family income and paid sick leave benefits was not significant ($p < 0.05$) when we analyzed the variable whether to forgo medical care. (See Appendix Exhibits 3 and 5 for full regression results

and Appendix Exhibits 8 and 10 for a depiction of the interactive effects.)¹⁷

Paid sick leave benefits and family income had a significant interaction effect ($p < 0.05$) when we considered the risk of an ED visit in the past twelve months. (See Appendix Exhibit 6 for full regression results.)¹⁷ However, insurance status and its interaction with paid sick leave benefits were not statistically significant. We found that respondents with family incomes below \$35,000 had the highest predicted risk of an ED visit (10.7 percent) regardless whether or not they had paid sick leave or insurance coverage. When family income was \$35,000 or above and the family had paid sick leave, the predicted risk was nearly constant between 9.1 percent and 9.5 percent. Yet when there was no paid sick leave, the predicted risk of an ED visit decreased from 9.9 percent to 7.6 percent as family income level increased. (See Appendix Exhibit 11 for a depiction of the interactive effects.)¹⁷ For all people in the sample with incomes below \$35,000, the risk of an ED visit was higher than in any other income bracket. For people with incomes of \$35,000 to <\$75,000, the predicted risk of an ED visit depended on both paid sick leave and insurance status. For people with incomes of \$75,000 or above, the predicted risk of having an ED visit was higher if they had paid sick leave than if they did not. Properly interpreting these findings requires additional research, which further examines factors that may influence ED usage, such as severity of health crisis and ability to access treatment in primary care settings.

Finally, insured working adults with paid sick leave benefits missed 1.5 days more of work because of illness or injury compared to workers without paid sick leave. Uninsured working adults with paid sick leave missed one day more of work because of illness or injury compared to workers without paid sick leave. Predicted mean days lost because of illness or injury are also presented in Exhibit 2. (See Appendix Exhibit 7 for full regression results and Appendix Exhibit 12 for the depiction of the interactive effects.)¹⁷

Discussion

This research greatly enhances our understanding of the relationship between paid sick leave benefits and health care-seeking behaviors. Both full- and part-time workers without paid sick leave were less likely than those with paid sick leave to take time off work when ill or injured and more likely to either forgo or delay treatment for themselves or a family member. These findings hold true after we controlled for individual- and family-level variables (including income, education level, and health status),

EXHIBIT 1

Bivariate analyses of control variables in a sample of NHIS respondents, by paid sick leave status, 2013

Variable	Paid sick leave		No paid sick leave	
	Number ^a	Percent	Number ^b	Percent
Sex				
Male	5,096	55.7%	4,060	44.3%
Female	5,490	59.0	3,819	41.0
Marital status				
Married	5,861	60.3	3,855	39.7
Not married	4,692	54.0	4,001	46.0
Education				
0-8 years	188	28.8	465	71.2
9 to less than 12 years	388	33.2	779	66.8
High school graduate/GED	1,969	47.5	2,179	52.5
Some college	3,266	54.7	2,703	45.3
Bachelor's degree	3,023	71.8	1,186	28.2
Master's degree	1,299	77.3	381	22.7
Doctorate	427	74.3	148	25.7
Race and ethnicity				
Hispanic	1,557	45.0	1,905	55.0
Non-Hispanic white	6,573	60.0	4,391	40.0
Non-Hispanic black	1,551	58.9	1,081	41.1
Non-Hispanic other	792	65.0	426	35.0
Occupation				
Management	1,162	71.7	459	28.3
Professional	3,917	75.0	1,304	25.0
Service	1,182	35.4	2,156	64.6
Sales	2,473	60.2	1,634	39.8
Production	1,628	42.5	2,200	57.5
Work status				
Fewer than 35 hours per week	9,606	66.4	4,857	33.6
More than 35 hours per week	948	24.3	2,954	75.7
Uninsured				
Yes	766	21.0	2,881	79.0
No	9,793	66.4	4,957	33.6
Health status				
Poor/fair	538	46.3	625	53.7
Good/very good/excellent	10,043	58.1	7,253	41.9
Limiting health condition				
Yes	437	47.7	480	52.3
No	10,145	57.8	7,395	42.2
Obesity				
Yes	3,000	58.9	2,092	41.1
No	7,238	56.6	5,554	43.4
Family income				
Less than \$35,000	1,922	35.2	3,533	64.8
\$35,000 to less than \$75,000	3,683	61.4	2,319	38.6
\$75,000 to less than \$100,000	1,565	69.2	695	30.8
\$100,000 or more	2,871	75.0	955	25.0
	Mean	SD	Mean	SD
Age (years)	42.0	11.8	39.1	13.0
Family size (number of individuals)	2.5	1.4	2.6	1.6

SOURCE Authors' analysis of data from the National Health Interview Survey, 2013. **NOTES** All analyses are statistically significant at the 0.005 level. GED is general educational development. SD is standard deviation. ^aN = 10,586. ^bN = 7,879.

which might otherwise influence delays in care and forgone care. Moreover, interactions between income and paid sick leave status indicate

EXHIBIT 2

Predicted risks in percentages or predicted mean days by respondents' insurance status, annual family income, and paid sick leave benefits, 2013

Outcome	Paid sick leave benefits?	Uninsured, by annual family income				Insured, by annual family income				
		All	<\$35k	\$35k to <\$75k	\$75k to <\$100k	\$100k+	<\$35k	\$35k to <\$75k	\$75k to <\$100k	\$100k+
Respondent delayed care in past 12 months because of cost ^a	No	0.9%	11.1%	9.7%	7.2%	4.4%	2.5%	2.1%	1.5%	0.9%
	Yes	0.3	9.8	6.3	3.5	1.3	2.4	1.5	0.8	0.3
Family member delayed care in past 12 months because of cost ^a	No	1.6%	13.5%	12.6%	9.3%	5.8%	3.9%	3.6%	2.6%	1.6%
	Yes	0.8	11.7	7.9	4.8	2.6	3.9	2.6	1.5	0.8
Respondent needed care but did not get it in past 12 months because of cost ^a	No	0.9%	16.5%	12.4%	8.1%	5.4%	3.1%	2.2%	1.4%	0.9%
	Yes	0.3	13.5	8.8	4.5	1.4	2.8	1.8	0.9	0.3
Family member needed care but did not get it in past 12 months because of cost ^a	No	1.3%	18.1%	14.0%	8.4%	5.5%	4.6%	3.4%	1.9%	1.3%
	Yes	0.8	14.6	10.4	6.4	3.2	4.1	2.8	1.7	0.8
Respondent had an ED visit in past 12 months ^c	No	7.6%	10.0%	8.3%	7.2%	6.3%	11.9%	9.9%	8.5%	7.6%
	Yes	9.1	10.0	8.8	8.7	8.5	10.7	9.5	9.4	9.1
No. of days respondent missed work because of illness or injury ^{d,e}	No	3.6	4.2	3.7	3.8	2.9	4.9	4.4	4.5	3.6
	Yes	5.1	4.1	4.5	4.3	3.9	5.3	5.8	5.6	5.1

SOURCE Authors' analysis of data from the National Health Interview Survey, 2013. **NOTES** Control variables include age, sex, marital status, educational level, race and ethnicity, occupation, family size, work status, health status, limiting health condition, and obesity. ED is emergency department. ^aSignificance for interaction of paid sick leave benefits and family income, main effect of paid sick leave benefits, insurance coverage, and family income ($p < 0.05$). ^bSignificance for main effect of paid sick leave benefits, insurance coverage, and family income ($p < 0.05$). ^cSignificance for interaction of paid sick leave benefits and family income ($p < 0.05$). ^dSignificance for main effect of paid sick leave benefits and insurance coverage ($p < 0.05$). ^eExcludes maternity leave.

that the lowest-income group of workers without paid sick leave are at the highest risk of delaying and forgoing medical care for themselves and their family members—making the most financially vulnerable workers the least likely to be able to address health care concerns in a timely manner.

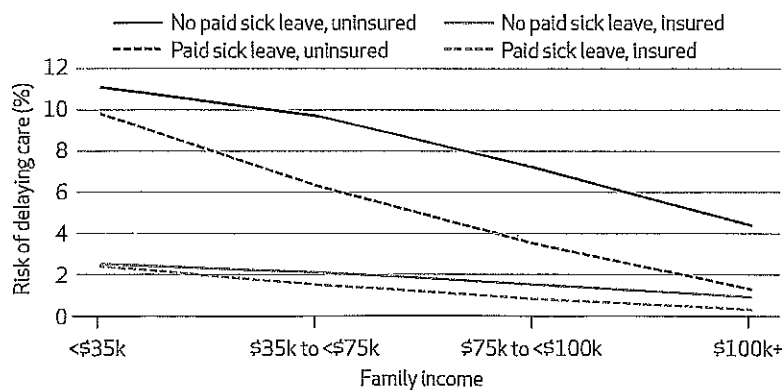
DELAYED AND FORGONE CARE While increasing the number of people who are able to obtain timely and needed medical care is an important national health care objective,¹⁸ existing research regarding paid sick leave's relationship

to delayed and forgone care is limited. Roy Penchansky and J. William Thomas's model of access to care asserts that affordability, accommodation, availability, accessibility, and acceptability are important determinants of health care access.¹⁹ Previous research identifies nearly 30 percent of US adults as having experienced delays in care or unmet health care needs; these respondents identified worry about the cost of care (affordability) and being "too busy with work or other commitments to take the time off" (accommodation) as the most prevalent reasons.²⁰ Access to paid sick leave benefits cuts across several of these determinants to improve access to care. It prevents wage loss (affordability), provides workers with the ability to take time off without risking losing their job (accommodation), and also increases workers' ability to seek treatment during daytime work hours (availability).

Findings from the research presented here are consistent with Penchansky and Thomas's theoretical framework. Our findings indicate that needed care is three times more likely to be delayed or put off entirely because of cost for the adult worker without paid sick leave. Family members are two times more likely to delay needed medical care and 1.6 times more likely to forgo needed care when paid sick leave is not present. Of note, these delays are attributed to cost. It is possible that the cost concerns identified reflect not only the direct cost of health care and per-

EXHIBIT 3

Predicted risk of a respondent's delaying medical care in the past twelve months, 2013



SOURCE Authors' analysis of data from the National Health Interview Survey, 2013.

The ability to stay home from work because of illness allows workers and their dependent children to self-quarantine when necessary.

haps more expensive urgent care but also the indirect costs associated with wage loss for those who do not have paid sick leave benefits. The risk for delaying medical care was significantly lower for higher-income respondents, which supports the idea that lost wages may be easier to handle when income is higher. The personal health care consequences of delaying or forgoing needed medical care can lead to more complicated and expensive health care conditions.

STAYING HOME FROM WORK WHEN SICK OR INJURED Consistent with previous research, this analysis found that US workers with paid sick leave were more likely to miss work because of being sick or injured compared to workers without paid sick leave. Put another way, they were more likely to take time off work to care for self or family when needed. This is important since increased work absences are associated with shorter recovery times and reduced complications.²¹ Additionally, the ability to stay home from work because of illness also allows workers and their dependent children to self-quarantine when necessary, without concerns about income or job loss. The importance of having this option is underscored by experience during the 2009 H1N1 influenza outbreak. The Centers for Disease Control and Prevention recommended that people stay home if they were sick; yet estimates suggest that employees who did not stay home infected an additional seven million people.²² Lack of paid sick leave is estimated to have resulted in 1,500 additional deaths during this outbreak.²³ Supriya Kumar and colleagues estimated that paid sick leave benefits could reduce influenza in the United States by as much as 6 percent.²⁴

More recently, in early 2016 the Chipotle restaurant chain, in response to its struggle with *E.*

coli and norovirus, announced a new paid sick leave benefit for all employees to ensure that employees stay home when they are ill, thus avoiding the possibility of making customers sick.²⁵

Policy Implications

In addition to illuminating the potential value of offering noncompulsory paid sick leave, findings from this research can inform the discussion about mandatory paid sick leave policy. Globally, paid sick leave benefits are designed either as a mandated private employer benefit or as part of a national health system. Four US states (Connecticut, California, Massachusetts, and Oregon) along with a few dozen municipalities, now mandate paid sick leave as an employee benefit.²⁶ In September 2015 President Barack Obama signed an executive order requiring that federal contractors allow workers to earn one hour of sick leave for every thirty hours worked, accumulating up to fifty-six hours or seven days of sick leave per year.²⁷ Comprehensive, long-term, peer-reviewed analyses of outcomes in the US regions that have passed sick leave legislation do not exist, but some research does exist that describes some health and business outcomes pertaining to paid sick leave in these regions.^{28,29} Our findings add to the body of research that policy planners can consider when weighing the issue of mandatory paid sick leave.

Reducing health disparities and improving access to medical care are national aspirations as documented in the Healthy People 2020 agenda.³⁰ When one takes into consideration the differential access to paid sick leave by race and ethnicity, income, and health status, access to paid sick leave can be viewed as a modifiable health disparity. Understanding the public health impact of employment policy is an important step toward implementing sound workplace regulations that may lessen the longstanding health care disparities between higher- and lower-wage workers.

In addition to those weighing the value of mandatory paid sick leave, this research is of interest to stakeholders such as health planners, human resources managers, and employers who aim to voluntarily plan their benefit packages in a way that optimizes the health and productivity of employees, while also boosting their business performance. Although further research is needed, findings from this study suggest benefits associated with paid sick leave, which these stakeholders may want to consider.

Health system policies, which support expanded access to affordable after-hours and weekend health services, should also be considered so that

those without paid sick leave are able to get preventive and routine treatment in nonemergency settings. Similarly, policy makers should also consider expanded access to health clinics in schools and work settings and the use of telemedicine appointments that could occur while at work or school. When workers report cost as the reason for delayed or no receipt of medical care, they are often counting the indirect costs, which include loss of wages in addition to the cost of the care itself. Being able to seek health care services after work hours would reduce the loss of wages.³¹

Implications For Future Research

Future research should assess whether urgent

care and ED use is significantly related to paid sick leave, considering the entire family unit and not just the adult worker. Future research should also examine whether family medical care costs are higher because of delayed or forgone care among those without paid sick leave. The relationship between paid sick leave and specific health outcomes for individuals, families, coworkers, and the patrons they come in contact with should be further investigated as well. Finally, gaps in insurance coverage and the quality of insurance coverage should be further examined in relation to these variables. Such research would help in the effort to relieve the burden of health care seeking for the most vulnerable members of the work force: those with lower incomes who lack paid sick leave. ■

All analyses, interpretations, and conclusions from this research are attributable to the article's authors and not to the National Center for Health Statistics, which is responsible for the initial data only.

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RESEARCH ARTICLE

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The lack of paid sick leave as a barrier to cancer screening and medical care-seeking: results from the National Health Interview Survey

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Abstract

Background: Preventive health care services, such as cancer screening can be particularly vulnerable to a lack of paid leave from work since care is not being sought for illness or symptoms. We first describe the prevalence of paid sick leave by broad occupational categories and then examine the association between access to paid sick leave and cancer testing and medical care-seeking in the U.S. workforce.

Methods: Data from the 2008 National Health Interview survey were analyzed by using paid sick leave status and other health-related factors to describe the proportion of U.S. workers undergoing mammography, Pap testing, endoscopy, fecal occult blood test (FOBT), and medical-care seeking.

Results: More than 48 million individuals (38%) in an estimated U.S. working population of 127 million did not have paid sick leave in 2008. The percentage of workers who underwent mammography, Pap test, endoscopy at recommended intervals, had seen a doctor during the previous 12 months or had at least one visit to a health care provider during the previous 12 months was significantly higher among those with paid sick leave compared with those without sick leave after controlling for sociodemographic and health-care-related factors.

Conclusions: Lack of paid sick leave appears to be a potential barrier to obtaining preventive medical care and is a societal benefit that is potentially amenable to change.

Keywords: Cancer screening, Pap test, Mammography, FOBT, Colonoscopy, Paid sick leave, Health benefits

Background

Paid sick leave is paid time taken off from work by individuals to attend to their own or their family member's illness or other medical needs without loss of pay or job loss. Paid sick leave in the United States is a provision by the employer and not an insurance option. Currently in the United States there are no federal legal requirements for paid sick leave [1]. The Federal Family and Medical Leave Act (FMLA) provides up to 12 weeks of unpaid leave for specified medical conditions for employees of companies with 50 or more employees [2], but FMLA does not apply to workers who need time off for routine or preventive medical care. Both San Francisco and Washington, DC have passed legislation guaranteeing paid sick leave to workers in their cities. In addition,

measures providing sick leave have passed in Milwaukee, WI and Seattle, WA but have not yet been enacted [3].

Concern about the lack of paid sick leave was heightened during the 2009 H1N1 influenza outbreak when the Centers for Disease Control and Prevention recommended that workers remain at home if they were sick with flu-like symptoms to control the spread of infection [4], and emergency legislation guaranteeing temporary sick leave was introduced in the House of Representatives [5]. In addition to the potential for reducing the spread of infection, the ability to take sick leave is likely to have an effect on a much wider range of health conditions and care-seeking both for workers and their families.

Preventive health services, including cancer screening, can be particularly vulnerable to a lack of paid leave since, by definition, preventive care is not sought for illness or symptoms. The United States Preventive Services Task Force (USPSTF) and the American Cancer Society recommend regular screening for the prevention of

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breast, colorectal and cervical cancers for early detection or removal of precancerous lesions [6,7]. However, screening rates for breast, cervical, and colorectal cancer in the US remain lower for people with lower income and education, without health insurance, and Hispanic ethnicity [7-10]. The USPSTF also recommends screening for high blood pressure and further screening for diabetes for those with high blood pressure [11]. This screening is typically part of a medical care visit.

Although the lack of health insurance coverage and access to preventive care have been broadly examined, [12-15] we are not aware of research to assess the effect of paid sick leave on the use of cancer screening services. In 2008, the Task Force on Community Preventive Services completed a systematic review of research on client-directed interventions to increase cancer screening [16]. The research examined did not address paid sick leave but did include other efforts to reduce out-of-pocket expenses. The Task Force concluded that there was sufficient evidence to show that reducing out-of-pocket costs increased the use of mammography but the evidence was judged insufficient to determine the effectiveness of similar interventions for cervical or colorectal cancer screenings. The aims of this analysis are to (1) describe the prevalence of paid sick leave by broad occupational categories and other occupationally-related groupings and (2) examine the association between

access to paid sick leave and cancer testing and medical care-seeking in the U.S. working population.

Methods

Study population

We used data from the 2008 National Health Interview Survey (NHIS), a multi-purpose health survey of a probability-based sample of the U.S. civilian noninstitutionalized population conducted by the CDC's National Center for Health Statistics (NCHS). The majority of the interviews were conducted in person by trained interviewers from the U.S. Census Bureau, and 25% were completed by telephone. The interviewed sample for 2008 consisted of 74,236 persons in 29,421 families from 28,790 households yielding a household response rate of approximately 85%, a conditional sample adult response rate of 74%, and a final adult sample size of 11,826 with a sample adult response rate of 63% [17].

The focus of this analysis was currently employed adults who were 18 years of age and older. This group included adults currently working for pay at a job or business in the prior week or adults working at a job or business but not at work in the prior week. We excluded workers who were self-employed, working without pay, working in a family business, looking for work, or not working (Figure 1).

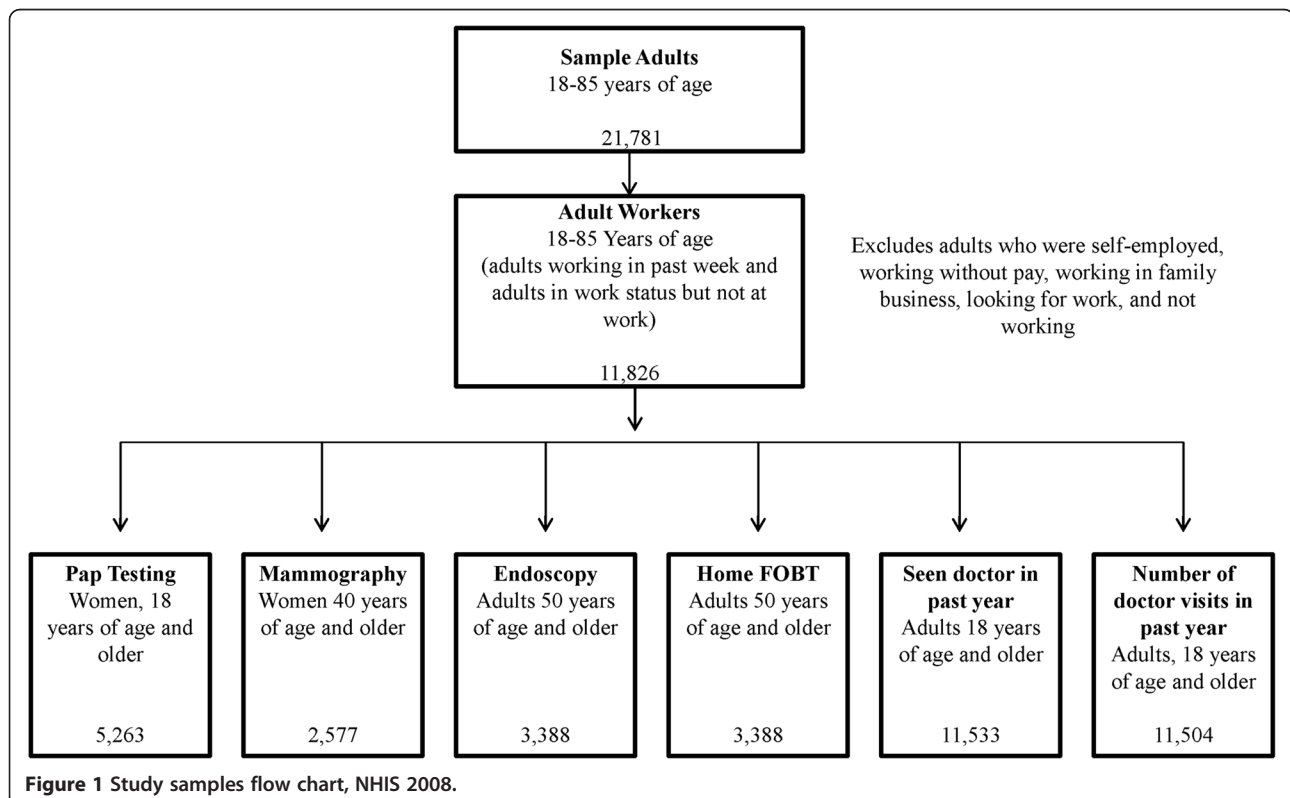


Figure 1 Study samples flow chart, NHIS 2008.

Occupational characteristics

Respondents were asked about the kind of work they did (occupation) and the current job or work situation (employed by a private company or federal, state or local government). Two-digit codes based on the Standard Occupational Classification [18] were assigned to each verbatim response by NCHS [19]. We collapsed the occupations into 5 general categories that included management occupations (codes 01–04), professional/technical occupations (codes 05–31), service occupations (codes 32–52), sales and office administrative support occupations (codes 53–64), and a general production category that included construction, production, transportation and maintenance occupations as well as farming, forestry and fishing occupations (codes 65–93).

Information was obtained on the number of people who worked at the respondent's current job location. The possible response categories of 1–9 employees, 10–24 employees, 25–49 employees, 50–99 employees, 100–249 employees, 250–499 employees, 500–999 employees and 1,000 or more employees were collapsed into 4 groups (Table 1). Currently working respondents reported how

many years they had worked at a main job or business. Years at work were categorized as 0–1, 2–5, 6–15 and 16 or more. Respondents answered 'yes' or 'no' to the question, "Do you have paid sick leave on this main job or business?"

Cancer tests and medical care seeking

Respondents were asked if they had ever had a colorectal exam, the type of exam, when they had the exam and the reasons for the exam. We classified respondents who reported having had a colonoscopy during the past 10 years or sigmoidoscopy during the past 5 years for any reason as having had an endoscopy within recommended screening guidelines. Although FOBT is currently recommended with sigmoidoscopy [6], the use of sigmoidoscopy represents only a small fraction of endoscopic screening procedures, and this recommendation in 2008 may not be reflected in the data used for this analysis. We used the definition of screening by sigmoidoscopy during the past 5 years to permit comparisons with other published estimates. In addition, respondents were

Table 1 Percent of U.S. workforce with paid sick leave by occupational characteristics, NHIS, 2008

Characteristics	NHIS sample	Estimated # of U.S. workers	% with paid sick leave*	95% CI	Estimated # of workers with no sick leave
All workers	11,826	127,067,000	61.9	60.7-63.1	48,352,000
Occupation					
Management	874	10,120,800	81.1	77.5-84.2	1,915,200
Professional/Technical	3,281	35,349,700	79.0	77.1-80.7	7,442,100
Services	1,997	21,028,300	41.2	38.3-44.1	12,373,000
Sales/Office	2,873	30,406,100	64.3	62.3-66.3	10,843,800
Production**	2,423	27,492,200	53.9	51.4-56.4	14,818,500
Class of worker					
Private	9,577	104,319,000	57.0	55.7-58.3	44,858,100
Federal	306	3,063,100	91.5	86.6-94.7	260,200
State	858	8,967,500	81.5	77.5-85.0	1,655,200
Local	975	10,717,600	85.3	82.2-87.9	1,578,500
Years on the job					
0-1	3,250	35,737,500	45.1	42.9-47.3	19,632,200
2-5	3,542	38,651,600	60.3	58.1-62.4	15,353,000
6-15	3,115	32,914,500	71.9	69.8-73.9	9,236,480
16+	1,704	18,662,600	79.4	76.8-81.7	3,854,130
Number of employees					
< 10	2,073	22,388,100	41.6	39.0-44.1	13,086,000
10-49	3,084	34,408,900	53.1	50.8-55.4	16,136,000
50-249	2,878	31,806,300	69.4	67.0-71.7	9,737,850
250+	2,485	32,820,100	79.4	77.4-81.3	6,760,420

* Percentages are weighted to the population of workers.

** Production category includes Production, Agricultural, Forest and Fishery workers.

CI indicates confidence interval.

Totals in categories may not sum to all worker totals because of missing and unknown values.

asked if they had ever used an FOBT home kit, and the date of their most recent test. Respondents, who had never had this test or had not had one during the prior year as recommended by national guidelines, were classified as not having the test. Women were asked if they had had a mammography and a Pap smear or Pap test, when they had the tests and the reasons for the test. Women who reported having had a mammogram during the prior 2 years or a Pap test during the prior 3 years as part of a routine exam were classified as having had a mammogram or Pap test respectively [6].

Respondents were asked if they had seen or talked to a general practice, internal medicine or family doctor during the prior 12 months and how many times during the prior 12 months they had seen a doctor or other health care professional in a doctor's office, clinic or location other than a hospital, emergency room, or dental office or spoken to one by telephone. For this analysis we dichotomized the number of doctor visits as no visits versus one or more visits during the prior 12 months.

Age groups and gender

For analyses of cancer testing, we included working women 40 years and older in the analysis of mammography. During the time of this survey, recommendations for mammography included women from age 40 to 49 years [20]. All adult working women (18 years of age or older) were included in analyses of Pap testing. Colorectal cancer analyses (endoscopy and FOBT) focused on adults 50 years of age or older. Analyses of the outcomes of those individuals who had seen or spoken with a doctor and the number of visits included all working adults 18 years of age and older. Figure 1 presents a chart of population sub-groups for analyses. We assumed that most adults who were healthy enough to work could potentially benefit from early cancer detection, regardless of age, and therefore we did not apply an upper age limit for the use of any cancer screening test.

Other covariates

All variables were self-reported. These included age (classified by 10 year age groups), education (less than high school, high school or GED, some college and college graduates), race/ethnicity (Hispanic, non-Hispanic white, non-Hispanic black, and non-Hispanic other), poverty ratio (<100%, 100% to <200%, 200% to <400%, 400% or more), insurance status (private, public only, private and public, not covered and unknown), usual source of medical care (yes, no, and only emergency room care) and marital status (never married, married/partnered, and widowed/divorced). Missing data for income was imputed by using multiple imputation [17].

Statistical analysis

We used descriptive statistics to examine the distribution of occupational characteristics of the U.S. workforce with and without paid sick leave. In addition, we used the chi-square test to examine the association of having paid sick leave with the uptake of various cancer tests, the number of physician visits and whether members of this population have been seen by a doctor during the prior year. We used six multivariate logistic regression models that show the association between sick leave status and various socio-demographic characteristics with each of the cancer tests, number of physician visits, and whether members saw a doctor during the prior year. To enable easy interpretation of the models' results, we computed and presented adjusted percentages (predicted margins), which are derived from the logistic regression model [21]. Overall associations were assessed with the Wald F statistic, and differences between categories within each adjusted variable were tested using general linear contrasts of the percentages.

To generalize the results to the population, each respondent was assigned a sampling weight. The weights accounted for selection probability and non-response. A P-value of less than 0.05 was considered statistically significant. We considered an estimate to be unstable and recommend caution in interpretation if the relative standard error, (calculated as [standard error/estimated percentage] x 100), was more than 30%. All statistical analyses were performed by using SAS 9.2 with SUDAAN release 10 (Research Triangle Institute, Research Triangle Park, NC) to adjust for the complex sampling design of the NHIS.

Results

More than 48 million currently employed U.S. workers reported having no paid sick leave at their main job or business (Table 1). The percentage of workers with paid sick leave varied by class of worker, years on the job and number of employees at the respondent's location of work. Service occupations had the lowest percentage of workers with paid sick leave (41%), and management workers had the highest percentage (81%) among occupational categories. The percentage of workers with paid sick leave was lower among private vs. all levels of the public sector. As years on the job and number of employees in a work location increased, the percentage of workers with paid sick leave increased.

Table 2 presents the relationship between having paid sick leave and cancer testing and medical care seeking. The percentage of workers who underwent mammography, Pap test, endoscopy at recommended intervals, had seen a doctor during the prior 12 months or had at least one visit to a health care provider during the prior 12 months was significantly higher among those with

Table 2 Percentages and 95% CIs of U.S. workers undergoing cancer tests and medical care-seeking by paid sick leave, NHIS, 2008

Cancer test	n	Paid Sick Leave				p*
		Has sick leave		Doesn't have sick leave		
		%	95% CI	%	95% CI	
Mammography	2,555	83.6	81.5-85.5	75.8	72.1-79.2	<0.001
Pap Test	5,218	89.9	88.7-91.0	86.4	84.5-88.1	<0.001
Endoscopy	3,224	52.7	50.1-55.3	43.1	39.7-46.5	<0.001
Home FOBT	3,208	9.2	7.8-10.8	9.7	7.9-11.9	0.68
# physician visits in past year	11,504	84.0	82.8-85.2	72.0	70.3-73.7	<0.001
Seen doctor in past year	11,533	69.1	67.6-70.4	57.9	56.1-59.8	<0.001

FOBT, fecal occult blood test; CI, confidence interval;
 Mammography within past 2 years, women age >39.
 Endoscopy for adult workers within past 5 years for flexible sigmoidoscopy or 10 years for colonoscopy.
 Home FOBT for adult workers within past year age > 49.
 # physician visits and seen doctor in past 12 months for all workers.
 p* values are based on an chi square test for association.

paid sick leave as compared with those without sick leave. The percentage of workers who reported having an FOBT within the prior year was less than 10% and did not vary by sick leave status.

After adjusting for sociodemographic and health related characteristics (Table 3), the associations between paid sick leave and mammography, Pap test and endoscopy remained statistically significant. The unadjusted and adjusted proportions of cancer tests by sick leave were quite similar. Working women 40 years of age and older with sick leave were more likely to have had a mammogram within the prior 2 years (83.3%; 95% CI, 81.2–85.2) than those without sick leave (77.0%; 95% CI, 72.9–79.9). No associations found between age, education, poverty ratio, health insurance status, race/ethnicity and mammography use. However, associations still remained for marital status and having a usual source for medical care. Married or partnered workers were more likely to have had a mammography than those who were widowed or divorced (83.2% vs 77.1%, $p < 0.01$) adjusting for covariates. Workers without a usual source of care were less likely to report a mammogram (57.1%; 95% CI, 47.5–66.2) than workers who had a usual source of care (82.7%; 95% CI, 81.0–84.3).

Among working women, we saw a small but statistically significant difference in Pap test reporting by paid sick leave status (91.9% vs. 89%, $P < 0.04$). A significant difference in reported Pap testing was also seen for age with the highest proportion of Pap tests being reported by the youngest workers (aged 18–29 years) when compared with all other age groups ($p < 0.001$). Widowed or divorced workers were less likely to report having had a

Pap test (88.2%) than married or partnered workers (92.0%), ($P < 0.01$). In addition, workers with a usual source of care (93.5%; 95% CI, 92.6–94.3) were more likely to have reported a Pap test compared with workers without a usual source of care (68.8%; 95% CI, 63.8–73.5). No associations with Pap testing were seen for education, poverty ratio, health insurance, or race/ethnicity.

A similar pattern was observed for endoscopy reporting. A larger proportion of workers with paid sick leave reported having had an endoscopy (52.5%; 95% CI, 49.9–55.0) than workers who lacked sick leave (43.5%; 95% CI 40.1–47.1). Higher proportions of workers aged 60–69 years or older reported endoscopy compared with those aged 50–59 years ($P < 0.001$). Married workers were significantly more likely to have had an endoscopy (51.7%; 95% CI, 49.0–54.3) than workers who were widowed or divorced (44.8%; 95% CI, 41.4–48.2). Having a usual source of care was significantly associated with reporting an endoscopy ($P < 0.001$). No association was found between education, poverty ratio, health insurance or race/ethnicity and endoscopy. Only age and marital status were statistically and significantly associated with home FOBT. Workers aged 50–59 years were less likely to have reported an FOBT home test than workers aged 60–69 years ($P < 0.001$) or workers 70 years of age and older ($P = 0.02$). Contrary to results for other cancer testing, widowed or divorced workers were more likely to report a home FOBT test ($P = 0.01$). Finally, workers with a usual source of care were more likely than to report a home FOBT than workers without a usual source of care (10.0%; 95% CI, 8.9–11.4 vs. 2.0%; 95% CI, 0.75–5.2).

Table 4 presents results for medical-care-seeking among working men and women. The overall proportion of workers reporting having seen a doctor during the prior year in any setting was higher than the proportion of workers who had at least one physician visit in an office or clinic setting. Only sick leave, age, and marital status significantly predicted having seen a doctor during the prior year or having had at least one physician visit. Workers with sick leave were more likely to have had at least one physician visit in an office or clinic (68.4%; 95% CI, 66.9–69.8) than those without sick leave (59.2%; 95% CI 57.3–61.0). A similar relationship was observed for workers seeking medical care in any setting including an emergency room. As expected, older workers (60–69 years of age and 70 years of age or older) were more likely to report having seen a physician at least once in a clinic or office than workers 50–59 years of age (83.4%; 95% CI, 80.4–86.1 and 83.2%; 95%CI 76.4–88.4 vs. 72.1%; 95% CI, 69.8–74.4). This relationship was also observed for having seen a doctor during the prior year in any setting. Widowed or divorced workers reported

Table 3 Adjusted population percentages and 95% CIs of U.S. workers undergoing cancer tests, NHIS, 2008

Characteristics	Mammography (n = 2,545)			Pap Test (n = 4,505)			Endoscopy (n = 3,210)			Home FOBT (n = 3,194)		
	PM*	95% CI	P**	PM*	95% CI	P**	PM*	95% CI	P**	PM*	95% CI	P**
Sick leave												
Yes	83.3	81.2-85.2	<0.001	91.9	90.6-93.0	<0.04	52.5	49.9-55.0	<0.001	9.3	8.0-10.9	0.83
No	77.0	72.9-79.9		89.9	88.1-91.4		43.5	40.1-47.1		9.6	7.7-11.8	
Age years												
18-29	-		0.29	97.0	95.5-98.0	<0.001	-		<0.001	-		<0.001
30-39	-			91.6	89.7-93.1		-			-		
40-49	79.6	76.6-82.3		88.4	86.2-90.3		-			-		
50-59	81.5	78.7-83.1		87.3	84.3-89.8		46.0	43.5-48.6		7.8	6.6-9.2	
60-69	84.4	81.2-88.0		83.1	78.2-87.0		56.9	53.0-60.6		12.7	10.4-15.3	
70+	84.0	74.2-90.4		70.1	55.5-81.5		60.7	52.7-68.2		13.7	9.5-19.4	
Race/ethnicity												
Hispanic	80.7	77.0-84.0	0.48	90.4	88.0-92.3	0.90	49.2	44.9-53.6	0.79	9.4	7.3-12.2	0.69
Non-Hispanic White	80.8	78.3-83.1		92.2	90.9-93.4		50.5	47.6-53.4		9.6	8.1-11.3	
Non-Hispanic Black	81.2	76.1-85.4		88.0	84.6-90.7		46.5	40.9-52.2		7.9	5.5-11.3	
Non-Hispanic Asian	87.3	80.3-92.1		90.9	86.7-93.8		51.3	42.9-59.6		10.4	6.0-17.4	
Non-Hispanic Other	81.9	64.6-91.9		94.1	85.5-97.7		48.2	31.9-64.8		14.8	6.6-30.0	
Marital status												
Never married	77.7	71.6-82.8	0.00	91.5	89.1-93.5	0.01	42.5	36.0-49.2	0.002	9.1	6.1-13.3	0.02
Married/partnered	83.2	80.9-85.3		92.0	90.6-93.1		51.7	49.0-54.3		8.5	7.2-10.1	
Widowed/Divorced	77.1	74.0-80.0		88.2	85.8-90.2		44.8	41.4-48.2		12.2	10.2-14.7	
Education												
< high school	80.5	77.5-83.2	0.08	91.8	90.0-93.3	0.70	49.8	46.1-53.5	0.51	10.2	8.3-12.6	0.80
High school/GED	79.8	75.7-83.4		90.9	88.7-92.7		50.3	46.1-54.5		8.9	6.7-11.8	
Some college	84.3	80.5-87.6		91.4	89.1-93.2		49.4	44.9-53.9		8.6	6.5-11.3	
College graduate	77.8	72.2-82.5		89.7	87.1-91.9		46.0	41.0-51.2		8.9	6.5-12.0	
missing	86.5	80.5-91.0		91.4	88.0-94.0		53.6	46.9-60.2		10.4	7.2-14.9	
Poverty ratio												
< 100%	84.0	78.3-88.4	0.20	92.1	88.8-94.5	0.71	43.3	37.3-49.4	0.12	6.7	4.4-10.1	0.14
100% to <200%	76.7	71.6-81.1		89.9	87.3-92.0		52.1	46.5-57.6		11.5	8.5-15.3	
200% to <400%	82.2	78.9-85.1		91.3	89.3-93.0		52.0	47.7-56.3		10.4	8.1-13.4	
400% or more	82.4	79.1-85.3		90.8	89.0-92.3		49.8	46.1-53.4		9.7	7.6-12.2	
Unknown	80.5	76.5-84.1		91.7	89.5-93.5		48.2	43.5-52.9		7.9	5.9-10.6	
Health insurance												
Private	80.4	76.9-83.5	0.25	90.4	88.3-92.1	0.2	48.2	44.6-51.8	0.44	9.9	7.9-12.2	0.76
Public only	79.4	75.1-83.2		91.8	89.6-93.6		51.6	47.1-55.9		8.9	7.0-11.2	
Public and Private	84.4	81.4-87.0		90.7	88.6-92.4		51.1	47.1-55.1		10.1	7.8-12.9	
Not covered	80.0	75.6-83.9		91.9	89.6-93.7		46.7	41.8-51.6		8.2	6.0-11.1	
Unknown	78.1	50.6-92.5		97.3	91.9-99.1		51.0	32.5-69.2		6.9	2.0-20.8	***
Usual Source of Care												
			<0.001			<0.001			<0.001			<0.001
Yes	82.7	81.0-84.3		93.5	92.6-94.3		51.7	49.5-53.9		10.0	8.9-11.4	
No	57.1	47.5-66.2		68.8	63.8-73.5		20.6	15.5-26.8		2.0	0.75-5.2	***

FOBT, fecal occult blood test; CI, confidence interval;

Mammography in past 2 years, women age > 39 home FOBT, adult workers in past year, age > 49 screening endoscopy, adult workers within past 5 years for flexible sigmoidoscopy or 10 years for colonoscopy.

*PM, predicted marginals from multivariate logistic models including all variables in Table 3.

p** values are based on an overall Wald F Chi Square test for association from multivariate logistic regression models.

*** relative standard error is greater than 30%, interpret estimate with caution.

Table 4 Adjusted population percentages and 95% CIs of U.S. workers visiting a physician, NHIS 2008

Characteristics	# Physician Visits (n=11,504) (in clinic or office)			Seen doctor in past year (n=11,533) (in any setting including ER)		
	PM*	95% CI	P**	PM*	95% CI	P**
Sick leave						
Yes	68.4	66.9-69.8	<0.001	84.6	82.4-84.9	<0.001
No	59.2	57.3-61.0		72.8	71.2-74.4	
Age years						
18-29	55.9	53.1-58.7	<0.001	75.0	72.5-77.4	<0.001
30-39	59.9	57.7-62.0		74.8	72.9-76.6	
40-49	66.7	64.5-69.0		80.1	78.1-82.0	
50-59	72.1	69.8-74.4		84.2	82.2-86.1	
60-69	83.5	80.4-86.1		93.4	91.5-94.9	
70+	83.2	76.4-88.4		93.5	89.2-96.2	
Race/ethnicity						
Hispanic	63.8	61.4-66.1	0.54	79.2	77.1-81.2	0.99
Non-Hispanic White	65.1	63.5-66.7		79.5	78.2-80.2	
Non-Hispanic Black	64.2	61.3-67.0		79.4	76.8-81.8	
Non-Hispanic Asian	67.9	63.4-72.0		79.9	76.1-83.2	
Non-Hispanic Other	63.4	51.7-73.6		77.9	66.9-86.1	
Marital status						
Never married	64.0	61.8-66.2	0.01	78.1	76.0-80.0	<0.001
Married/partnered	65.8	64.2-67.4		80.1	79.3-82.0	
Widowed/Divorced	61.4	58.5-64.2		75.9	73.4-78.2	
Education						
			0.48			0.21
< high school	66.2	64.2-68.2		80.9	79.2-82.5	
High school/GED	64.3	62.0-66.6		79.2	77.1-81.2	
Some college	64.4	61.9-66.8		79.1	76.8-81.3	
College graduate	63.5	60.9-66.0		78.5	76.2-80.6	
missing	64.7	60.9-68.4		77.7	74.2-80.8	
Poverty ratio						
< 100%	64.1	61.1-67.1	0.91	80.6	77.6-83.4	0.11
100% to <200%	65.6	62.8-68.4		80.0	77.6-82.2	
200% to <400%	64.2	62.0-66.3		80.8	78.9-82.6	
400% or more	65.1	63.0-67.0		77.7	76.0-79.4	
Unknown	65.1	62.5-67.6		79.2	77.0-81.3	
Health insurance						
Private	66.2	64.3-68.0	0.26	80.6	78.6-82.4	0.12
Public only	63.6	61.3-65.8		79.2	77.1-81.2	
Public and Private	64.2	62.1-66.3		79.1	76.3-79.9	
Not covered	65.5	62.6-68.2		79.5	77.1-81.8	
Unknown	58.6	49.2-67.2		86.1	78.8-91.1	

*PM, predicted marginal from logistic regression model using all variables in Table 4.

p** values are based on an overall Wald Chi Square test for association from multivariate logistic regression models.

ER is Emergency Room.

the lowest proportions of medical care-seeking as compared with married or partnered workers. No significant relationship was seen for education, poverty level, race/ethnicity or poverty level and medical care seeking.

Conclusions

Out of an estimated U.S. working population of 127 million in 2008, more than 48 million (38%) lack paid sick leave. Approximately 60% of private-sector workers and

more than 80% of state and local government workers had paid sick leave. Our analysis shows that it was workers in service or production occupations, those in the private sector, and those in smaller firms with fewer years on the job who were less likely to report having sick leave. Furthermore, our results from this nationally representative sample demonstrate that sick leave could be a significant barrier to cancer testing and medical care seeking.

Both unadjusted and adjusted proportion of workers undergoing mammography, Pap test, endoscopy and medical care-seeking were significantly higher for those with paid sick leave than those who lacked paid sick leave. It was only for home FOBT that we did not see an association with paid sick leave. Compared with endoscopy which requires contact with a physician and time away from work, testing for blood in the stool with an FOBT test kit is performed at home. In addition, the proportion reporting home FOBT was much smaller than the proportion reporting endoscopy.

Screening behavior is affected by a myriad of factors that vary within different populations. We adjusted for sociodemographic factors that have been shown to be barriers or facilitators of cancer testing or medical care-seeking in the United States. Race/ethnicity, education, age, household income, marital status, usual source of care and health-care coverage have been associated with colorectal cancer screening [10,12,13], mammography [9,13], and Pap testing [13] in population-based surveys, including the NHIS and a random sample of Medicare beneficiaries [22]. Our study population, which included only U.S. working men and women, is likely to differ in important ways from the U.S. population as a whole or the Medicare population. Consistent with previous research, we also reported a significant contribution of age and marital status to models of cancer screening or medical care-seeking as outcomes, but saw no significant differences in cancer screening by health insurance status or poverty. This could be due to a population of working men and women having less variability in insurance status and poverty level than a general population. Among working adults, lack of paid sick leave may pose a more significant barrier to cancer testing and medical care-seeking than lack of insurance or poverty.

This analysis has some limitations. For example, data are based on self-report and respondents may have incorrectly reported their screening use and the timing of that screening. A recent meta-analysis of the accuracy of self-reports of cancer screening concluded that national survey data overestimate the prevalence of screening and mask disparities by race and ethnicity because of differences in reporting accuracy [23]. In addition, the survey seeks information only on paid sick leave and no other leave such as paid personal or annual leave, and

the survey does not capture any restrictions on the use of sick leave for preventive health care. Workers may have personal leave or vacation leave but may not consider or report these categories as paid sick leave. Thus, we may have underestimated the proportion of workers with leave that could be counted for cancer screening. However, our estimates of worker's access to paid sick leave were similar to the Bureau of Labor Statistics (BLS) estimates of 61% for private-industry workers and 89% of state and local workers during 2008 [24]. Differences are primarily due to differences in survey design. Whereas the NHIS is a survey of randomly chosen individuals from households who are representative of the noninstitutionalized U.S. population, the BLS estimates are obtained from the National Compensation Survey, an employer-based survey representing a random selection of establishments chosen from state unemployment insurance records [25].

Barriers to cancer screening and routine medical care-seeking involve a complex web of individual, community, health care system and societal characteristics. In the working population, a person's occupation is the source of his or her income and medical insurance coverage, and of other benefits such as paid sick leave, worker's compensation, paid vacation, and retirement benefits [26]. In short, a person's occupation is the source of some of the most critical elements determining their health and well-being. And in the United States, access to these benefits is largely determined by the type of occupation. The percentage of workers with access to paid sick leave is lowest among service workers, workers in construction and maintenance, transportation workers, and part-time workers, and highest among managers and professional workers. This occupational structure disproportionately affects women who are more likely to be low-wage and part-time workers [27].

Lack of paid sick leave can be considered within the category of out-of-pocket costs for medical care. Those without sick leave who take work time off for preventive services may lose pay. High deductibles and other forms of cost sharing have been associated with underuse of preventive services [28,29], specifically colorectal cancer screening [30] and mammography [31,32]. Lack of paid sick leave appears to be a potential barrier to obtaining needed medical care and a societal benefit that is potentially amenable to change.

Abbreviations

NHIS: National Health Interview Survey; CI: Confidence interval; FOBT: Fecal occult blood test; NCHS: National Center for Health Statistics; FMLA: Family and Medical Leave Act; USPSTF: United States Preventive Services Task Force.

Competing interests

The authors declare that they have no competing interests.

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Authors' contributions

LAP, ZB, AS and MCW designed the study. AS and ZB conducted the statistical analyses. LAP, AS, ZB and MCW contributed to the interpretation of the data. LAP drafted the manuscript with contributions from AS, ZB and MCW. LAP, AS, ZB and MCW read and approved the final manuscript. The findings and conclusions in this report are those of the authors and do not necessarily represent the official position of the Centers for Disease Control and Prevention.

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Arise Chicago



Worker Center Members on Why Chicago Needs #PaidSickDays



I have a special needs child. She needs specialized care. But her childcare doesn't take sick kids. I need paid sick days to take care of her. I've also missed doctor appointments because I couldn't take a day off.



I started working third shift when my daughter was 12. I can't count the number of times I had to leave her home sick alone overnight. I never missed a day in 8 years. After taking 1 sick day in 8 years, I was fired.



I never felt comfortable calling in to work sick. If you don't have someone else lined up to cover your shift, don't bother calling. You just go in sick.



As a worker and mother, one of the hardest things I experienced was not being able to be with kids when they were sick because I didn't have paid sick days.



I work at a factory. We need to be healthy to focus on our work on not get hurt. Paying for paid sick days will cost an employer less than paying for an accident and workers compensation.



My husband got sick and had an operation. He felt pressured to go back to work before he was ready. Some time after that, I got sick. Later we were both fired after 13 years.

"82% of voters in Chicago voted for paid sick days. That speaks to the desire and the need that exists for paid sick days." ~Martina

Arise Chicago



Worker Center Members on Why Chicago Needs #FairScheduling



We need to be seen not just as workers, but as people, as parents. I can't predict when my daughter might have an emergency—like when she swallowed a coin. I need flexibility to care for her without repercussions.



I was expected to work at a moment's notice. My schedule changed day to day. My boss would even come to my home at 1 or 2am pleading with me to come to work.



My husband's gets his schedule the day of his shift, and his hours change week to week. His hours even change during work—come in early, leave early, stay late. And if you don't stay late, they punish you by taking you off the schedule for weeks.



I worked with many single mothers. Not having set days or hours of work made it difficult for all of us to pick up our kids from school or plan for their care.



Arise Chicago Worker Center members representing car wash, restaurant, retail, and cleaning industries participated in the city's first Working Families Task Force Focus Group discussing the need for paid sick days and fair scheduling. They were joined by representatives from partner organizations sitting on the Task Force and representatives from the city of Chicago.

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