KU LEUVEN

FACULTY OF ECONOMICS AND BUSINESS

A comprehensive evaluation of minimum wage policy



Dissertation presented to obtain the degree of Doctor in Business Economics

by

Jesse Wursten

Number 701 2020

Daar de proefschriften in de reeks van de Faculteit Economie en Bedrijfswetenschappen het persoonlijk werk zijn van hun auteurs, zijn alleen deze laatsten daarvoor verantwoordelijk.

Committee

Supervisor	
Prof. dr. Dirk Czarnitzki	KU Leuven
Doctoral Committee	
Prof. dr. Otto Toivanen	Aalto University
Prof. dr. Geert Dhaene	KU Leuven
Prof. dr. Mariana Zerpa	KU Leuven
Prof. dr. Michael Reich	IRLE, UC Berkeley
Chair	
Prof. dr. Peter Teirlinck	KU Leuven

I am grateful to all the members of the committee for their comments at the doctoral seminars, the pre-defense and in between.

Acknowledgements

It is somewhat intimidating how many people have contributed to this PhD one way or another. Maybe the beginning is as good a place to start my acknowledgements as any.

Therefore, I'd like to begin by thanking my parents. They tolerated my quirks and fostered my strengths. They supported me every step of the way and I could not have asked for a better start to life than they have given me. I mention this in particular, because one of the things that has become strikingly apparent to me is how much one's success is driven by their initial conditions. Yes, I wrote this doctoral thesis all by myself. But I was able to do so while distracted by relatively few worries. I have always been able to be myself, to explore what I find interesting. My parents pushed me to never accept the easy answer and to always keep the other person in mind. Combine the two, and it is perhaps no surprise that I ended up writing a dissertation about minimum wage policy, which at heart is about helping others help themselves.

I would also like to thank my brother and sister. Everything I said about my parents, also applies to them. My brother is the trailblazer. Without him, I'm not sure I would have dared move from comfy Antwerp to far away Leuven for my studies. But so is my sister. She started a PhD in physics a year before I started mine in Economics and thus made the prospect much less daunting. My family has always been the bedrock I could turn to even when my life sometimes felt like quicksand beneath my feet.

In the same spirit, I would like to thank everyone who took care of me as a child, from the friendly members of my church, to the numerous instructors in kindergarten, elementary school and high school. I am especially grateful to all my math and science teachers, who showed me what I am capable of and made my university career considerably less hard than it could have been. Never let anyone say that 'gemeenschapsonderwijs' is inferior.

I would also like to thank all the friends I made along the way. You have all played a part in shaping who I am and helped me get through the twenty years of education before I even started the PhD. I was lucky in that I continued to meet amazing people also after I started. I think of my colleagues at ECOOM and HOG, but also at ZEW (DE), the ECB (DE) and IRLE (US). The PhD life can easily become a very lonely one and you all stopped that from happening. This dissertation is already very long, so I cannot write a paragraph of praise for each of you, but some do deserve

special mention. Laura and Wouter, for making me feel welcome from the very first day at ECOOM and for the deep and impressively structured lunch time discussions. Maikel, Markus, Moritz and Bastian for doing the same at ZEW; Thomas, Dennis, Nuria and Jeroen at HOG; Tora, Jose, Agneta and Kjersti at IRLE and Cecilia, Samuel, Roma and Luca at the ECB. These acknowledgements would not be complete without mentioning Lara, who was always available for a chat when the negative aspects of the PhD got me down (e.g. the numerous paper rejections that are part of the process) or in those moments where life left me all confused.

Finally, there are two persons who have been instrumental beyond all others. First, Dirk Czarnitzki, my advisor. This PhD would literally not have been possible without you. You have kept me focused, read more about minimum wages than could be expected of any innovation professor, supported some of my crazy programming ideas and kept me afloat financially as I submitted both my papers and myself around the world.

Last, but definitely not least, my girlfriend. Freya, I will not embarrass you by writing too much here, but suffice to say that meeting you remains the best thing that's ever happened to me, that more than anyone else you have made the last years absolutely incredible and that because of you, I did not return to my apartment after work but to my home, which makes all the difference.

Jesse Wursten
Leuven, July 2020

Contents

Committee iii

Acknowledgements v

List of Figures xi

List of Tables xiv

Preface 1

1 Is Politics the Missing Piece of the Minimum Wage Puzzle? 5
- 1.1 Introduction . . . 5
- 1.2 Data . . . 7
- 1.3 Omitted Variable Hypothesis . . . 10
- 1.4 Results . . . 14
- 1.5 Relation to literature . . . 18
- 1.6 Robustness Checks . . . 26
- 1.7 Conclusion . . . 31

2 Estimating the earnings and employment effects of the minimum wage through differences in exposure across US counties 35
- 2.1 Introduction . . . 35
- 2.2 Vulnerability Measure . . . 38
- 2.3 Empirical Strategy . . . 42
- 2.4 The Data . . . 44
- 2.5 Results . . . 48
- 2.6 Robustness Checks . . . 51
- 2.7 Policy Simulations . . . 64
- 2.8 Conclusion . . . 67

3 Limits to the external validity of minimum wage research 69
- 3.1 Introduction . . . 69
- 3.2 External validity . . . 71
- 3.3 Data, methodology and context . . . 73

3.4 Shares over time . . . 74
3.5 Similarity by study type . . . 80
3.6 Evidence by sub group . . . 85
3.7 Summary of empiric coverage . . . 89
3.8 Edge between empirical and theoretical territory . . . 90
3.9 Conclusion . . . 91

Bibliography **93**

Appendix to Chapter 1 **101**
1 Endogenous control bias . . . 101
2 Tables and Figures . . . 103

Appendix to Chapter 2 **109**
1 Sources of measurement error . . . 109
2 Complete long differences . . . 111
3 QWI . . . 114
4 Policy simulations without unaffected group assumption . . . 114
5 Tables . . . 126
6 Figures . . . 135

Appendix to Chapter 3 **147**
1 Additional context . . . 147
2 Tables . . . 152
3 Figures . . . 154

List of Figures

1.1 Evolution of minimum wages per state. 8
1.2 Histogram of percentage minimum wage changes (1 percent bins). . . 9
1.3 Median political ideology over time 12
1.4 Restaurant employment by state, minimum wage change years highlighted 13
1.5 Coefficients of the state time trends. 21
1.6 Predicted probability of being bound by the federal minimum wage. . 23
1.7 Employment elasticity, different starting years. 25
1.8 Mean ideology values for bound and unbound states. 26
1.9 Elasticity with respect to the minimum wage. 32

2.1 Construction of the vulnerability measure. 39
2.2 Distribution of hypothetical shares, with base thresholds. 40
2.3 US Counties and their vulnerability status. 41
2.4 Evolution of state minimum wages between 1990 and 2017. 44
2.5 Size of state minimum wage changes between 1990 and 2017. 45
2.6 Minimum wage earner shares 46
2.7 Elasticity with respect to the minimum wage, high vs low vulnerability. 48
2.8 Elasticity with respect to the minimum wage, sparse model. 50
2.9 Distribution of hypothetical shares within the low vulnerability group. 50
2.10 Elasticity with respect to the minimum wage, saturated model, grey-zone vs the other groups. 51
2.11 Elasticity with respect to the minimum wage, saturated model, continuous interaction. 52
2.12 Elasticity with respect to the minimum wage, saturated model, bottom threshold variation, high vs low vulnerability. 53
2.13 Elasticities with respect to the minimum wage, joint regressions. . . . 55
2.14 Earnings and Employment elasticities with respect to the minimum wage, QWI dataset, teenagers (14-18) from all industries. 62
2.15 Elasticities with respect to the minimum wage, multiple sectors, industry-specific estimates after three quarters. 63
2.16 Elasticities with respect to the minimum wage, multiple sectors. . . . 65

3.1 Evolution of state minimum wages between 1990 and 2017. Copied from Wursten (2020). 73
3.2 Affected shares for the restaurant sector, teenagers and the overall working population. Historical and projected. 74
3.3 Share of affected workers by age group. Historical and projected. . . . 75
3.4 Share of affected workers by (modified) industry group. Historical and projected. 76
3.5 Share of affected workers by (modified) occupation group. Historical and projected. 76
3.6 Share of affected workers by job tenure. Historical and projected, two year moving averages. 77
3.7 Share of affected workers by education level. Historical and projected. 78
3.8 Share of affected workers by education level, college only. Historical and projected. 78
3.9 Share of affected workers by aggregated census division. Historical and projected. 79
3.10 Share of affected workers by distance to new minimum wage. Historical and projected. 80
3.11 Similarity By Age Group 80
3.12 Similarity By Industry 82
3.13 Similarity By Occupation 82
3.14 Similarity By Job Tenure 83
3.15 Similarity By Education 84
3.16 Similarity By geography 84
3.17 Similarity By Expected Pay Increase 85
3.18 Share of represented sub-groups, one dimension. 86
3.19 Coverage of represented sub-groups, three dimensions. 87
A1 Share of affected workers by age group. Historical and projected. . . . 104
A2 Example of one of the instruments. 104
A3 Loadings of the first factor (λ_1) averaged by state. 105
A4 Slopes of the trend in political ideology over time by state. 105
A5 Evolution of the first factor (F_1) over time. 106
B1 Deviations from mean percentage. Boxplot: box represents IQR, the whiskers the adjacent values, the dots the outside values. 109
B2 Elasticity with respect to the minimum wage, complete long differences (CLD). 111
B3 Earnings and Employment elasticities with respect to the minimum wage, QWI dataset. 118
B4 Hiring and separation elasticities with respect to the minimum wage, QWI dataset. 119
B5 Employment elasticities with respect to the minimum wage, QWI dataset, by age. 120
B6 Earnings elasticities with respect to the minimum wage, QWI dataset, by education. 121

B7 Employment elasticities with respect to the minimum wage, QWI dataset, by education. . . . 122
B8 Earnings elasticities with respect to the minimum wage, QWI dataset, by race. . . . 123
B9 Employment elasticities with respect to the minimum wage, QWI dataset, by race. . . . 124
B10 Earnings and Employment elasticities with respect to the minimum wage, QWI dataset, young adults (19-21) from all industries. . . . 125
B11 Elasticity with respect to the minimum wage, saturated model, continuous interaction, predicted minimum wage effects after three quarters. 135
B12 Elasticity with respect to the minimum wage, sparse model, bottom threshold variation, effect in low vulnerability group. . . . 136
B13 Elasticity with respect to the minimum wage, saturated model, mean instead of median over time. . . . 137
B14 Elasticities with respect to the minimum wage, variable group membership. . . . 138
B15 Elasticity with respect to the minimum wage, saturated model, raw shares. . . . 139
B16 Elasticity with respect to the minimum wage, saturated model, shares excluding MW episodes. . . . 140
B17 Elasticities with respect to the minimum wage, controls interacted by county. . . . 141
B18 Elasticity with respect to the minimum wage, saturated model, county by quarter of year controls. . . . 142
B19 Elasticity with respect to the minimum wage, saturated model, excluding 10% least densely populated counties. . . . 143
B20 Elasticity with respect to the minimum wage, saturated model, limited service restaurants (NAICS 7222). . . . 144
B21 Elasticity with respect to the minimum wage, saturated model, limited service restaurants (NAICS 7221). . . . 145
C1 Share of affected workers by (modified) minimum wage worker family type. Historical and projected. . . . 147
C2 Share of affected workers by income quantile (working families only). Historical and projected. . . . 148
C3 Share of affected workers by income quantile. Historical and projected. Effect of family size adjustment. . . . 149
C4 Share of affected workers by income quantile. Historical and projected. Effect of working family restriction. . . . 150
C5 Share of affected workers by food security. Historical and projected. . . . 151
C6 Similarity By Family Type 154
C7 Similarity By Income Quartile 154

List of Tables

1.1 Impact of Politics on Minimum Wage Laws 11
1.2 Employment elasticity of the minimum wage in the restaurant sector 15
1.3 Political Ideology Instruments 18
1.4 Employment elasticity of the minimum wage in the restaurant sector - Instrumented Results 19
1.5 Robustness checks (Earnings and Finance results) 27
1.6 Own Wage Employment Elasticities (restaurant sector) 28
1.7 Campaign Contribution based Political Ideology Instruments 29
1.8 First and Second Stage of Instrumented 2FE regressions - With Contribution Instruments 30

2.1 Industry composition of NAICS 722 46
2.2 Restaurant Worker Characteristics 47
2.3 Elasticity with respect to the minimum wage - Differences 57
2.4 Elasticity with respect to the minimum wage - Levels 59
2.5 Policy Simulations 66

3.1 Aggregation strategies considered in this paper. 72
3.2 Coverage at 100 observations per subgroup. 87
3.3 Share in $15 population by education, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample. 88
3.4 Share in $13 population by education, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample. 88
A1 Monte Carlo results 102
A2 Occupational distribution of the restaurant sector workforce in 2016 . 103
A3 Replication of Meer and West (2016) 106
A4 First and Second Stage of Instrumented 2FE regressions 107
B1 Example of our data 112
B2 Example of data in the style of Meer and West (2016) 113
B3 QWI: Summary Statistics 115
B4 QWI result summary table: Impact after three quarters 116
B5 QWI result summary table: Contemporaneous impact 116
B6 QWI result summary table: Impact three quarters before 117

B7 Policy Simulations 117
B8 Elasticity with respect to the minimum wage - High vs Low Vulnerability 126
B9 Elasticity with respect to the minimum wage - Sparse model 127
B10 Elasticity with respect to the Minimum Wage - Saturated model, grey-zone vs the other groups. 127
B11 Elasticity with respect to the minimum wage - Continuous interaction 128
B12 Elasticity with respect to the minimum wage - Controls interacted by county 129
B13 Elasticity with respect to the minimum wage - Controls interacted by county and quarter of year 130
B14 Elasticity with respect to the minimum wage - Variable Group Membership 131
B15 Elasticity with respect to the minimum wage - Joint Regressions . . . 132
B16 Sector Characteristics 132
B17 Elasticity with respect to the minimum wage - Multiple sectors 133
B18 Elasticity with respect to the minimum wage - Long Differences . . . 134
C1 Share in $15 population by age, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample. . 152
C2 Share in $13 population by age, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample. . 152
C3 Share in $15 population by tenure, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample. 153
C4 Share in $13 population by tenure, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample. 153

Preface

Les Représentans du peuple se rendront à leur destination, investis de la plus haute confiance et de pouvoirs illimités. Ils vont déployer un grand caractère. Ils doivent envisager qu'une grande responsabilité est la suite inséparable d'un grand pouvoir.

—the French National Convention, 1793

I believe in the dignity of labor, whether with head or hand; that the world owes no man a living but that it owes every man an opportunity to make a living.

—John D. Rockefeller, 1941

The French National Convention of 1793 observed that anyone representing the people holds great power, but that they should always wield it responsibly. Part of that responsibility consists of doing the due diligence before implementing or changing policies. The larger the potential impact of a policy, the more extensive this due diligence should be. This applies doubly so for policies that affect people's livelihoods or restrict their freedom of choice. As economists, we play an important role in this process, estimating treatment effects where we can and constructing structural or theoretical models where we cannot. It would be hubris to claim we can predict with certainty what the effects of any policy will be, but at least we can attempt to reduce the uncertainty to acceptable levels.

In this dissertation, I attempt to reduce the uncertainty surrounding the effects of minimum wage policy in the United States, a web of federal, state, county and city level legislation that affects between 15-50 million workers across the US. It could be a powerful tool against poverty, but only for those who remain employed. As a result, any gains in earnings at the bottom of the wage distribution have to be compared to the number of jobs that might be lost.

This would be straightforward if minimum wages were introduced randomly, for then we could simply compare localities with a minimum wage to those without and read out the effects. Instead, in Chapter 1 I show that the minimum wage is a political battleground and that there are major differences between places that increase it and those that do not. That makes it difficult to distinguish the impact of minimum wage policy from concurrent policy changes, as well as fundamental differences in the economic situation of the various US states. I find that once you factor in that

conservative governments are less likely to increase the minimum wage, then you still see meaningful earnings effects, but no losses to employment. This would suggest that higher minimum wages are beneficial to low wage workers, who, especially in the US, earn a very low wage indeed.

In Chapter 2, I propose a second approach to shield the minimum wages effect estimate from these differences between states. Instead of estimating the impact of minimum wages levels by comparing states, I move one level deeper and compare counties within states. Even though these have the same minimum wage (I exclude exceptions), they are not equally affected by minimum wage policy. Consider California. Average earnings of a restaurant employee in booming San Francisco in 2019 were $662 per week. In rural Humboldt County, weekly earnings are 40% lower, at $383. One could imagine then, that state level minimum wage policy is much more relevant in Humboldt than in San Francisco.[1] I find that after a minimum wage increase, earnings rise more in 'vulnerable' counties, whereas employment responses remain muted across the board, consistent with the results from Chapter 1.

In Chapter 3, I take a step back and analyse the limits of empirical minimum wage work. Imagine you are a policymaker and you wonder whether it would be a good idea to introduce a $15 dollar minimum wage in the US, twice the current federal rate. Available to you are all the minimum wage studies of the last decades. Can you base your decision on those studies? And if not, what kind of assumptions would you need to fill in the gaps? I find that past minimum wage changes are surprisingly representative in terms of the industries they affected. Moreover, historically affected minimum wage workers came from similar positions in the income distribution as those that would be affected by large minimum wage hikes. However, the composition of these groups does differ in terms of age, tenure, role in the household, education level, geographic location and the size of the required pay increase.

I show that it is not possible to predict the effects of a large minimum wage change using only empirical evidence. Instead, it is necessary to assume how those results generalise to groups with different characteristics and when they receive a larger pay increase than we have seen in the past three decades. The chapter concludes with a roadmap for future minimum wage research, based on three pillars. First, there is scope to estimate minimum wage effects for finer subgroups than is currently available. For example, we do not know whether workers with long tenure at their job are hit differently by minimum wage shocks, even though there is data available on these characteristics. Second, the subgroup-specific estimates we have are based mainly on worker characteristics. But the type of firm affected by minimum wage policy might also matter. E.g. it is not clear whether employment responses are likely to be stronger in small firms than in large ones. Finally, I propose adopting a reversed modelling approach. Traditional models attempt to convert fundamental behavioural assumptions into concrete policy outcomes. Instead, I suggest to start from the wealth of empirical evidence and then model explicitly how that extrapolates to other contexts.

[1] The difference is so large that San Francisco has decided to exceed the state level wage floor and introduced its own minimum wage in 2004. As such, it is one those exceptions excluded from the analysis.

Overall, this dissertation is cautiously optimistic about the effects of minimum wage policy. I find robust earnings effects in my empirical studies, which far exceed any negative employment effects. Moreover, the final chapter indicates that while it is not straightforward to extrapolate these results to large minimum wage changes, it does suggest that the gap is not as large as is commonly thought.

Chapter 1

Is Politics the Missing Piece of the Minimum Wage Puzzle?

The effect of minimum wages on US employment is highly disputed. We show that certain differences in the literature can be explained by heterogeneous shifts in political ideology across states. The results suggest minimum wages increase earnings in affected sectors without reducing employment. Unlike existing estimates, these new results remain consistent across specifications, even when we instrument political ideology by evolving cultural values or campaign contributions. The methodology used can be extended to analyses of other state level policies.

1.1 Introduction

The minimum wage and its effects is one of the most researched topics in labour economics. Yet, even after a century of research there is no sustained consensus on its desirability.[1] It can be a powerful tool against poverty, especially for the working poor.[2] At the same time, introducing a wage floor could lead to a decrease in the demand for labour, as certain jobs might no longer be profitable to employers, potentially reducing employment more than it raises earnings.

In this paper, we show that certain inconsistencies in the literature can be reconciled once we accept that minimum wage hikes are the result of political evolutions that also affect restaurant employment in ways unrelated to the minimum wage.[3] We include a bi-yearly measure of each state legislature's political orientation and

[1] In the 19th century, Macrosty (1898) describes two unions fighting for sector-specific minimum wages: the London dock workers who demand a wage which "would secure decency, if not comfort" and forward-looking miners who want their wages tied to the evolution of coal prices.

[2] In 2013, 7% of workers in the US were classified as poor despite working at least 27 weeks per year.

[3] We focus on the restaurant sector as it is the only industry to employ a meaningful amount of workers who could plausibly be affected by the minimum wage. We chose not to look at teenagers as they have made up a decreasing share of plausibly affected workers, which applies doubly to the higher minimum

find little to no evidence of disemployment effects of the minimum wage. This result holds when we instrument the political ideology measure with cultural values or campaign contributions which are expected to affect restaurant employment only through politics (conditional on county and time fixed effects and total private sector employment controls). Moreover, our specification leads to more credible estimates when applied to the de facto unaffected finance sector.

Our findings link to a large and varied body of literature. The political ideology angle taken in this paper provides an explanation for the negative effects found by the year and state fixed effects models which imply that a 10% increase in the minimum wage leads to a 1-3% drop in employment (e.g. Neumark and Wascher, 1992). We provide theoretical and empirical arguments that this does not reflect a causal link, but rather that states which become more conservative tend to have lower minimum wages and higher restaurant employment.[4]

We show that richer models, e.g. the state time trends model of Allegretto, Dube and Reich (2011) and the cross border county comparison model of Dube, Lester and Reich (2010) implicitly filter out most of the confounding effects of political ideology. In that regard, our paper provides an explanation for the long debated difference between these models and traditional twoway fixed effects (see e.g. Allegretto et al., 2013 and Neumark and Wascher, 2017). Additionally, we illustrate how our findings can add to the interpretation of the results of less traditional models, such as the interactive fixed effects model of Totty (2015), the instrumental variables approach of Baskaya and Rubinstein (2015) and the federal minimum wage driven results of Clemens and Wither (2019).

Overall, we find that factoring in the political angle reduces or even eliminates existing evidence of negative employment effects. The positive earnings effects of the minimum wage in affected groups persists. Combined, this implies that minimum wage policy has the capacity to reduce working poverty without major detrimental effects to the labour market.[5]

The remainder of this paper is structured as follows. Section 1.2 describes the data and institutional framework. In Section 1.3 we show why political ideology could be an important omitted variable. Then, in Section 1.4 we illustrate how the main estimates in the literature change once one adds the political ideology control. In Section 1.5 we link these results to a wider set of models. Section 1.6 provides robustness checks and Section 1.7 concludes.

wages currently proposed (Appendix Figure A1). Moreover, the welfare implications of fewer teenagers working depend strongly on their outside options, whereas an increase in the number of jobs available in the restaurant sector is almost universally positive for low wage workers.

[4]Note for example that according to the CNBC's Top States for Business ranking, conservative states are rated more business friendly and have a lower cost of doing business. At the same time, the Republican party has consistently lobbied against minimum wages, whereas the Democrats added a commitment to introducing a $15 an hour minimum wage in their party platform in 2016.

[5]This is consistent with certain theoretical models, e.g. the search models described in Flinn (2006) and Dube, Lester and Reich (2016).

1.2 Data

1.2.1 Employment

We follow Dube, Lester and Reich (2010) in turning to the Quarterly Census of Employment and Wages (QCEW)[6], extending the sample to 2013. Its earnings and employment records are based on administrative records related to unemployment insurance and cover ~97% of salaried workers in the United States.

In line with the literature, we focus on employment in the restaurant service sector[7] as it employs a disproportionate share of minimum wage workers, with 30% earning within 10% of the minimum wage in 2006 (DLR). Additionally, the sector is present in every county, although the data are at times not available for confidentiality reasons.[8] It covers both traditional (full service) restaurants as well as (limited service) fastfood and takeout joints. The sector provided employment to 10 million people in 2016, of which 30% were fastfood and counter workers (average hourly wage: $9.57), 20% were waiters ($11.57), 16% cooks ($11.28) and 7% supervisors ($16.66). Table A2 in the Appendix provides a more complete breakdown.

Our full sample runs from 1990-2013, representing 288 months of employment information.[9] We follow the literature in using a balanced panel, retaining the 1293 out of 3142 counties that have data for the full time period. These counties represented 88% of total non-farm employment in December 2013.[10] This leaves us with 300 000 observations (out of more than half a million). The QCEW does not provide hours-worked information, only a headcount of filled positions. As a result, we are limited to discussing effects along the extensive margin, although the impact on weekly earnings suggests any reductions in hours worked are more than compensated by wage increases. In our regressions, we will use total restaurant sector employment and average weekly earnings as dependent variables and total private sector employment and earnings as controls, all in logs.

1.2.2 Minimum Wages

Structured, regression-ready data on minimum wage levels was obtained from the personal website of Ian Salas (Neumark, Salas and Wascher, 2014*a*) and cross-checked with official data from the BLS and law (amendment) texts.[11] In total, we observe 426 minimum wage changes (counted at the state level). States are free to set the minimum wage at any level, as long as it equals or exceeds the federal minimum

[6] Use of this dataset in minimum wage research goes back to at least Card and Krueger (2000), when it was still known as the ES-202 program.

[7] NAICS 7221 + 7222, based on the 2002 classification.

[8] The Bureau of Labor Statistics (BLS) suppresses industry-county specific values if the industry is so small in that county the numbers might allow observations to be made on individual establishments.

[9] DLR use quarterly employment data. We find that the results are qualitatively similar regardless of the time unit chosen (results available on request).

[10] Own calculations based on BLS data

[11] The original file can be found at `https://sites.google.com/site/jmisalas/data-and-code`.

wage. Many have made use of this freedom: 238 of the observed changes are the result of decisions by the state legislatures, the remaining 188 are adjustments to stay in line with the federal minimum wage. Figure 1.1 shows the evolution of minimum wages over time.

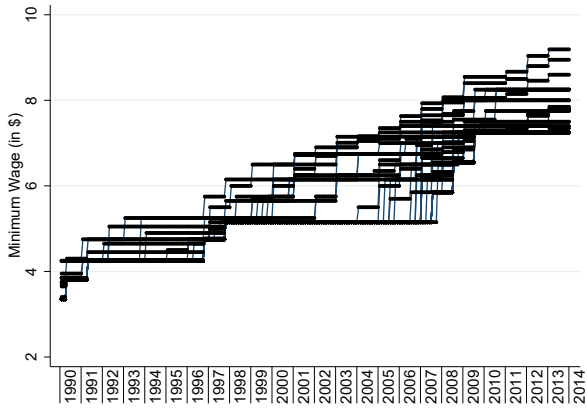


Figure 1.1: Evolution of minimum wages per state.

The average increase of the mandated minimum wage was 9.5%, but this masks a wide variation in the size of the changes as depicted in Figure 1.2. About a quarter were smaller than 5%, often reflecting cost of living adjustments (in 2017, 18 states have their minimum wage pegged to some price index, usually the CPI).[12] A handful of adjustments exceed 20%. Michigan spans the crown with their October 2006 increase of 35%. Colorado is the only state to have ever reduced the minimum wage (by 0.5% in 2010, following a decline of the consumer price index the year before). The minimum wage enters the regressions in logs.[13]

1.2.3 Political measure

We use the political measure established in Shor and McCarty (2011) to quantify the political situation in each state. It tries to determine each legislator's 'ideal point', which is their location on a liberal-conservative axis. This ideal point is construed based on both roll-call data (who votes on what) and the NPAT survey on political beliefs.[14] The first characterises each legislator within its legislative body (e.g. the Alabama House), but does not allow for consistent comparison across states. The survey on the other hand poses the same questions across states and years, but has

[12] 70% of those small changes can be accounted for by indexation policies. 75% of indexation policies in our sample were enacted before 2007.

[13] This log-log setup implies its coefficient can be interpreted as an elasticity (percentage change in X corresponds to percentage change in Y).

[14] The National Political Awareness Test (NPAT) is administered by Project Vote Smart and poses a wide range of political questions to legislators.

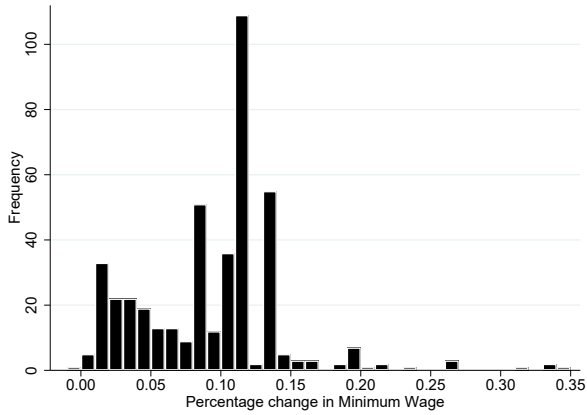


Figure 1.2: Histogram of percentage minimum wage changes (1 percent bins).

only a limited response rate. Shor and McCarty (2011) show that the survey answers of the respondents can be used to rescale the roll-call based ideal points into the common NPAT space (which is consistent over states and years).[15]

The median of these individual ideal points per state chamber is then used as an indicator of the prevailing political sentiment and can be compared over time and space. Note that for many states this measure only changes every two years (the election cycle in most states). The bottom part of Figure 1.5 shows the evolution of this measure for the state lower chambers ('the House'). Positive (negative) values indicate a gradual shift towards more conservative (liberal) notions. There is great heterogeneity across the US, e.g. the south has become considerably more conservative, whereas the west and north-east have shifted left. The changes can be considerable, e.g. Georgia was mildly liberal in 1996, but turned into one of the most conservative states by 2012.[16] More generally, the Kendall rank correlation between a state house's median ideal point in 1996 and 2000 is only 0.59, gradually dropping to 0.31 by 2012. In the regressions, we will use the median ideology of the state's *lower* chamber, as it tends to have the most legislative power, especially when it comes to economic decision making.[17] There are some gaps in the availability of this measure, which reduces our sample to 221 340 observations.

[15] In other words, you know from the roll call data how conservative everyone is within his chamber. From the survey, you know for some how conservative they are relative to respondents in the rest of the country (and across years). You can then translate everyone's ideal point to this common scale (the NPAT space) using the survey respondents as indicator for how 'conservative within New York House' corresponds to 'conservative within the US'.

[16] Anecdotally, it might be interesting to note that Georgia never exceeded the federal minimum wage in our sample, whereas Colorado has done so since 2007.

[17] Results are nonetheless very similar when one uses the upper chamber ('the Senate') instead.

1.3 Omitted Variable Hypothesis

In this section we discuss the four conditions that need to be met for political ideology to be an important omitted variable. First, political ideology affects the level of the minimum wage. There is surprisingly little research that looks into the determinants of the minimum wage (two exceptions are Leigh, 2008 and Baskaya and Rubinstein, 2015). To fill this gap, we trawled through two decades of state legislation to determine which legislature passed which minimum wage legislation and under which political conditions.

$$MW_law_passed_{st} = \delta * POL_{st}^{0to1} + \gamma_1 * e_{st}^{TOT} + \gamma_2 * pop_{st} + \alpha_s + \tau_t + \epsilon_{st} \tag{1.1}$$

Equation 1.1 summarises how we use that information to determine whether ideology matters in minimum wage policy. $MW_law_passed_{st}$ is a binary variable indicating whether state s passed a minimum wage law in year t. POL_{st}^{0to1} is the political ideology measure, rescaled such that a value of one refers to the most conservative legislature in our sample (0: most liberal). We add total private employment (e_{st}^{TOT}) and population (pop_{st}) as controls. In order to filter out confounding state-level factors and changing nationwide effects we include state and year fixed effects (in line with e.g. Besley and Case, 2003; Leigh, 2008).

Results are shown in Table 1.1. In Column (1), we omit the control variables, including only the state and year fixed effects. The point estimate of -0.35 (significant at 5%) indicates that going from the most liberal to the most conservative legislature reduces the probability of introducing a minimum wage law by 35 percentage points.[18]

The estimated relation does not change if we add the total employment and population controls in Column 2 (-0.36, 5%). It is reassuring to see that those controls appear uncorrelated to the minimum wage. Finally, in Column (3) we interact the political ideology variable with a dummy that indicates whether the same parties control both chambers (state House and Senate). One would expect ideology in the House to matter less if the Senate is controlled by the opposing party. Indeed, we only see a significant effect under unified government (-0.42, 10%). When government is divided, the coefficient drops to -0.04 with a large standard error (0.31).

The higher propensity of liberal states to increase state minimum wages also reduces the impact of *federal* minimum wage changes in those states, which are often no longer binding. For example, Clemens and Wither (2019) document that only 27 states were fully bound by the federal minimum wage increases of 2008 and 2009.

Second, liberal governments behave differently in ways that could affect restaurant employment. This is directly visible in the main regressions in Section 1.4 and supported by previous literature. For example, liberal governments spend more on

[18] In terms of predicted probabilities, this results in a 32% chance of introducing minimum wage legislation in the most liberal legislature versus -3% in the most conservative one. We consider the negative predicted probability close enough to zero to stick to a linear model (and thus avoid the complications that arise from including fixed effects in probit/logit models).

Table 1.1: Impact of Politics on Minimum Wage Laws

	(1)	(2)	(3)
Rescaled Political Ideology	-0.35**	-0.36**	
	(0.17)	(0.17)	
Unified government ×			-0.42*
Rescaled Political Ideology			(0.21)
Divided government ×			-0.04
Rescaled Political Ideology			(0.31)
Divided government			-0.31
			(0.22)
Total Private Sector		-0.27	-0.16
Employment		(0.53)	(0.52)
Population		0.06	0.01
		(0.49)	(0.49)
State FE	✓	✓	✓
Year FE	✓	✓	✓
N	564	564	560

Standard errors statistics in parentheses, errors are clustered at the state level. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

education and family assistance (Beland and Oloomi, 2017; Hill and Jones, 2017; Besley and Case, 2003; Leigh, 2008; Soss et al., 2001) and they raise more state revenues (Besley and Case, 2003; Reed, 2006; Warren, 2009; Alt and Lowry, 1994; Bjørnskov and Potrafke, 2013; Chang, Kim and hsiang Ying, 2009; Gu et al., 2017; Pickering and Rockey, 2013). Note that it is not strictly required that state policies affect restaurant employment directly. One could imagine a scenario where a sufficient set of agents believe liberal government is bad for the economy, depressing confidence, investment and local demand, creating a self-fulfilling prophecy.[19]

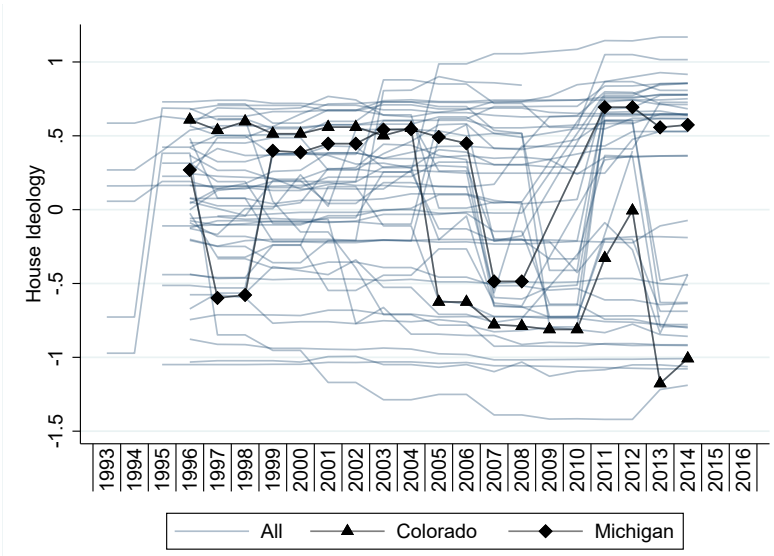


Figure 1.3: Median political ideology over time

Third, the median ideology of state governments changes heterogeneously over time, which prevents state and time fixed effects from filtering out its effect. This was partly covered in Section 1.2 and is highlighted further in Figure 1.3, which shows our political variable for each state over time. There is considerable variation over the years and no overall trend. Note e.g. Colorado which went from being one of the most conservative states to one of the most liberal, or Michigan which has been very conservative throughout the sample, except for a short liberal stint in 2006/08. In this liberal legislative session, they raised the minimum wage by 35%, increased the state income tax rate, invested $2bn in a jobs fund, cut government services and introduced a "No Worker Left Behind" act which provides two years of free training/community college to the unemployed.

Finally, many of the channels in our omitted variable hypothesis are indirect,

[19] President Trump's election provides an extreme example. Before his election, 44% of Republicans felt able to make a large purchase. Four months later, this number jumped to 71%. For Democrats, the share remained stable at roughly 55%. Source: `https://www.theatlantic.com/business/archive/2017/03/trump-economy/520533/`.

with potentially only small effects on the employment elasticity estimate. If the direct minimum wage effect on employment were expected to be large, we might be able to ignore the political aspects. However, this is not the case, as even the more extreme estimates suggest only a drop of 1-2% in total restaurant employment after a 10% increase in the minimum wage. In Figure 1.4 we show aggregated restaurant employment in our sample per state over time, with minimum wage change years highlighted in red to illustrate how fine a comb is required to detect the minimum wage effect. Note that most of these arguments apply to other state policies as well, suggesting our methodology would also be of use in other fields.

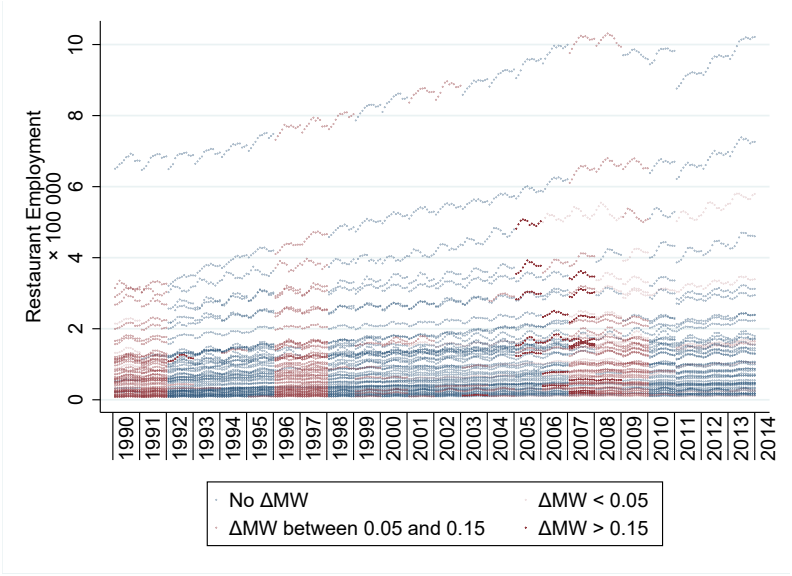


Figure 1.4: Restaurant employment by state, minimum wage change years highlighted

It is difficult to predict the direction of the bias ex ante as it depends on the sign of two correlations, only one of which is known with some certainty. I.e. conservative governments are less likely to increase their local minimum wage. The correlation between political ideology and restaurant employment on the other hand is more ambiguous. On one hand, conservatism is associated with more business friendly policies, which would lead to a negative bias.[20,21] On the other hand, it is less clear which ideology's policies lead to more product demand and thus potentially more labour demand. The social welfare policies of liberal governments might be better at supporting and maintaining demand at the lower end of the market, whereas the lower tendency to tax at conservative legislatures might boost the middle and upper

[20] Phrased from the other direction: liberal governments are more likely to increase the minimum wage and are associated with lower employment. Omitting ideology would then transfer this negative correlation to the minimum wage coefficient.

[21] See also footnote 4.

segments.[22]

1.4 Results

We start our analysis by re-estimating the most common methods in the literature and then add the political ideology variable to determine whether it is a meaningful omitted variable in practice. Equation (1.2) shows the basic regression structure as inspired by Neumark and Wascher (1992) and DLR. e_{it}^{R} represents the log of restaurant employment in county i during month t. mw_{it} is the log minimum wage, β is then the employment elasticity we are interested in. The common controls e_{it}^{TOT} and pop_{it} refer to the logs of total private sector employment and population (at the county level). The term between large brackets is the median ideology of the state lower chamber. Table 1.2 shows results both with and without this variable. Finally, $controls_{it}$ represents the particular statistical setup of the various papers as detailed in the set of Equations (1.3). We chose not to weight the regressions by population as we consider each state equally informative about the impact of minimum wages.

$$e_{it}^{R} = \beta * mw_{it} + \gamma_1 * e_{it}^{TOT} + \gamma_2 * pop_{it} + \text{controls}_{it}\left[+ \delta * POL_{it} \right] + \epsilon_{it} \tag{1.2}$$

$$\begin{aligned} &\text{(1) 2FE} && \text{controls}_{it} = \alpha_i + \tau_t \\ &\text{(2) Trends} && \text{controls}_{it} = \alpha_i + \tau_t + \psi_s * t \\ &\text{(3) CBCP} && \text{controls}_{ipt} = \alpha_i + \tau_{pt} \\ &\text{(4) CD} && \text{controls}_{it} = \alpha_i + \tau_{cd,t} \end{aligned} \tag{1.3}$$

The simplest model listed is the twoway fixed effect model (2FE), which includes month dummies as well as county fixed effects and represents the side of the debate that finds negative employment effects. The remaining models then represent the null effect side. The trends model (Trends), as popularised by Allegretto, Dube and Reich (2010) adds state-specific linear time trends (ψ_s identifies the state). The contiguous border county pair model (CBCP), introduced by DLR adds separate time dummies per county pair that straddles a state border, with p identifying the pair.[23] Model 4 (CD) includes Census Division by time fixed effects $\tau_{cd,t}$ which eliminate all variation between the nine census divisions (see Allegretto, Dube and Reich, 2010 and Dube, Lester and Reich, 2010).

[22] Moreover, these effects are likely to be very context-driven. Regulatory burden might outweigh other concerns in tourist driven locations, whereas welfare systems and taxation are more relevant when local demand is key. Social support systems might sustain demand during recessions, whereas small government ideologies amplify the gains from boom periods.

[23] We surrendered some accuracy here for ease of exposition. The CBCP method involves a modified sample, where each county appears once per cross-border pair it belongs to. This way, we only retain 281 unique counties represented in 289 pairs. As a result, the error term should be e_{ipt} to indicate a particular county might be represented multiple times per time period if it borders multiple states. In the regressions, errors are clustered both on state and state border segment. See DLR for more details.

Table 1.2: Employment elasticity of the minimum wage in the restaurant sector

		(1) 2FE	(2) Trends	(3) CBCP	(4) CD
A	Employment elasticity	-0.112**	-0.016	-0.045	0.002
		(0.049)	(0.020)	(0.053)	(0.032)
	N	221 340	221 340	90 552	221 340
B	Employment elasticity	-0.049	-0.016	-0.052	0.011
		(0.033)	(0.020)	(0.054)	(0.032)
	Median political ideology of house	0.042***	0.001	-0.011	0.010
		(0.011)	(0.004)	(0.011)	(0.007)
	N	221 340	221 340	90 552	221 340

Panel A shows results without controlling for political ideology. Panel B shows results with the political ideology variable included.
Standard errors in parentheses, errors are clustered at the state level. All regressions include total private sector employment and population controls, as well as monthly time dummies. Model (4) uses three factors, results are robust to using a different number of factors. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The results in the top panel (A) of Table 1.2 summarise the existing conflict in the literature. The traditional twoway fixed effects models (column 1) predicts economically[24] and statistically significant negative employment elasticities, suggesting that employment goes down by 1.1% after a 10% increase in the minimum wage, akin to roughly 0.1 million food service jobs lost nationwide.[25] By contrast, the richer models do not support the disemployment effect story. Simply adding state-specific trends already reduces the coefficient to zero (column 2). Likewise, the cross border county pair method (CBCP, column 3) and the census division-specific time fixed effects model (CD, column 4) both lead to a null effect.

We include the political ideology measure in panel B. Now, none of the point estimates suggest economically or statistically significant disemployment effects, from the twoway fixed effects model (column 1) up to the census division-specific time dummies model (column 4).[26] The 95% confidence interval of the employment elasticity in the 2FE model (column 2) equals [-0.11, 0.016], overlapping only ever so slightly with the -0.1 to -0.3 consensus band of the 90s. The political measure is highly significant in the twoway fixed effects model which initially predicted strong disemployment effects (column 1), providing empirical backing for the omitted variable hypothesis.[27]

Including the political ideology measure removes the average effect of political ideology on restaurant employment. The CBCP and CD models are considerably more restrictive as they are identified purely by variation in minimum wages within county pairs and census divisions respectively. As such, they run the risk of "throwing out the identifying baby along with, or worse yet instead of, the contaminated bathwater" (Neumark, Salas and Wascher, 2014*b*).[28] This is not the case in our politically augmented models. The R^2 of our regressions in Table 1.1 is only 0.4, indicating political ideology and minimum wage effects are separately identified. Moreover, multicollinearity would exhibit itself through inflated standard errors, whereas ours decrease once you add the political ideology variable.

One concern is that our efforts to reduce omitted variable bias have exposed us to a problem of endogenous controls. If political ideology is endogenous and minimum wage levels are correlated with political ideology, then part of the endogeneity bias originating from political ideology leaks out to the coefficient on minimum wages. In Equation 1.4 we show the analytical expression for this asymptotic bias, as derived by

[24] We consider results economically significant if they are more negative then -0.1, inspired by the old consensus band of the 90s, also repeated in Neumark (2019).

[25] Based on 2013 employment levels in the restaurant sector (8 730 219).

[26] Results for the interactive fixed effects model, the BEA economic area-specific time dummies model (Liu, Hyclak and Regmi, 2016), as well as dynamic versions of most models are available upon request - none of them suggest economically and statistically significant disemployment effects.

[27] These point estimate decreases in absolute value from -0.049 to -0.038 when we interact our political ideology variable with a dummy indicating whether the same parties controlled the house and senate. Reassuringly, the impact of political ideology on restaurant employment is smaller under divided governments. Table available on request.

[28] Both Neumark, Salas and Wascher (2014*b*) and Neumark and Wascher (2017) suggest directly including omitted variables as an alternative to saturating the model with fixed effects.

Frölich (2006).[29] We also illustrate this bias through a simple Monte Carlo exercise in Appendix 1.

$$\operatorname*{plim}_{N\to\infty} \hat{\beta} - \beta = \frac{\sigma^2_{Pol}\sigma_{\epsilon,MW} - \sigma_{MW,Pol}\sigma_{\epsilon,Pol}}{\sigma^2_{Pol}\sigma^2_{MW} - \sigma^2_{MW,Pol}} \tag{1.4}$$

$\sigma^2_A = var(A)$ and $\sigma_{AB} = Cov(A, B)$ for all $A, B \in \{MW, Pol, \epsilon\}$. The two conditions for the bias to occur can be seen in the second term of the numerator. If either $\sigma_{MW,Pol}$ (variables uncorrelated) or $\sigma_{\epsilon,Pol}$ (politics not endogenous) is zero, the bias converges to zero.[30] We address this potential problem in two ways. First, although the process determining political ideology in each state is complex, we see few ex ante reasons to assume this leads to a systematic bias given our controls. Any confounder would have to affect restaurant employment conditional on total private sector employment, population, county fixed effects and time fixed effects. Second, we instrument the political ideology variable using social values which affect whether the Democrat or Republican candidate enters the state house, but which are not plausibly correlated with restaurant employment. Then $E[\hat{\beta}] = \beta + (Z'X)^{-1}Z'\epsilon$, which converges to β if the minimum wage and the instruments Z are truly exogenous given the controls.

We obtain instruments from the American National Election Studies (ANES), which provides publicly available state-year level polling data (with gaps). We use the combination of plausibly exogenous opinions that maximises the first stage predictive power. This includes the share of atheists in the population, opinions on gays, the military, abortion, big business, the prevalence of racial discrimination and whether new lifestyles are hurting society (see Table 1.3). Take abortion stances as an example. The 2016 Democratic party platform calls for universal access to abortion whereas the 2016 Republican party platform opposes all use of public funds to perform abortions, including through subsidized healthcare (DNC, 2016; RNC, 2016). A shift towards more favourable attitudes of abortion would then be expected to attract more voters to the Democratic party but is unlikely to be correlated with restaurant employment once you control for total employment, population levels plus state and month fixed effects.

The instruments were chosen to have the opposite characteristics of the variables preferred in cultural economics (e.g. Alesina and Giuliano, 2015). There, the focus is on variables that are relatively persistent and are likely to affect institutions and economic development. Instead, we select opinions that shifted heterogeneously across states and are unlikely to be affected by the same unobservables as restaurant employment. As an example, Figure A2 in the Appendix shows how opinions on the link between new lifestyles and society's health evolved across states. These instruments will lead to biased results if they are correlated with unobservable trends that also affect restaurant employment. For example, if older populations have different restaurant spending habits and different social values. However, these confounders would have to persist in the presence of total private sector employment and population

[29] The expression follows directly from the derivation of the OLS estimator.

[30] Of course, this only holds if the minimum wage is exogenous conditional on political ideology and the other controls.

Table 1.3: Political Ideology Instruments

Instrument	N	Mean	SD	Min	Max
Women should always be able to get an abortion	614	0.29	0.16	0.00	0.69
Atheist	614	0.35	0.13	0.00	0.79
Federal crime fighting spending should be higher	614	0.64	0.12	0.20	1.00
Negative feelings towards the military	614	0.07	0.05	0.00	0.39
Negative feelings towards gays	614	0.35	0.14	0.07	0.80
We have gone too far pushing equal rights	614	0.41	0.12	0.11	0.90
Positive feelings towards big business	614	0.44	0.13	0.00	0.94
Gov't should provide only little support	614	0.48	0.12	0.07	1.00
Blacks have not been discriminated against	614	0.53	0.12	0.06	1.00
New lifestyles contribute to society's breakdown	614	0.64	0.10	0.32	0.95

controls as well as county and month fixed effects.

Results are shown in Table 1.4. In Panel A we repeat the estimates from the literature for the sample that takes into account the gaps in the polling data. We still see the same pattern: a significant disemployment effect for the 2FE model (-0.12, column 1) and none for the others (columns 2-4). In Panel B we add our instrumented political ideology measure. As before, the richer models remain unaffected, producing essentially identical point estimates (columns 2-4).[31] Most interesting is what happens to the 2FE estimate (0.04, column 1). Again, we see that any evidence for a significant disemployment effect of the minimum wage disappears. The 95% confidence interval is [-0.09, 0.18], which no longer encompasses the -0.1 to -0.3 band of consensus that used to exist in the literature.[32] The SW F- and χ^2 tests reject respectively their nulls of weak and underidentification (Sanderson and Windmeijer, 2016). We never reject the Hansen J-test. First stage results for the instrumented 2FE model are shown in column (1) of Table A4 in the Appendix.

1.5 Relation to literature

In this section we discuss how our results relate to existing research. Where feasible, we evaluate directly how adding the political angle affects estimates. For other studies, we are limited to discussing how the political angle can expand the interpretation of their results.

1.5.1 Traditional 2FE studies

The most direct comparison is to the traditional twoway fixed effects models spearheaded by Neumark and Wascher (1992). These tend to find negative employment

[31] The biggest change occurs for the CD model, where the estimate becomes more positive.

[32] The estimate is also significantly different from the 2FE coefficient at 1%.

Table 1.4: Employment elasticity of the minimum wage in the restaurant sector - Instrumented Results

		(1) 2FE	(2) Trends	(3) CBCP	(4) CD
A	Employment elasticity	-0.118**	-0.022	-0.064	0.006
		(0.046)	(0.018)	(0.055)	(0.026)
	N	187 092	187 092	67 488	187 092
B	Employment elasticity	0.044	-0.018	-0.068	0.054
		(0.067)	(0.032)	(0.060)	(0.046)
	Median political ideology of house	0.100***	0.005	-0.005	0.063
		(0.031)	(0.031)	(0.026)	(0.038)
	N	187 092	187 092	67 488	187 092
	SW F-test pvalue	0.001	0.064	0.000	0.006
	SW χ^2-test pvalue	0.000	0.031	0.000	0.001
	J-test pvalue	0.930	0.247	0.695	0.451
	95% CI on MW	[-0.09, 0.18]	[-0.08, 0.04]	[-0.17, 0.06]	[-0.03, 0.14]

Panel A shows results without controlling for political ideology. Panel B shows results with the instrumented political ideology variable.
Standard errors in parentheses, errors are clustered at the state level. All regressions include total private sector employment and population controls, as well as monthly time dummies. Model (4) uses three factors, results are robust to using a different number of factors. The pvalues for the SW F- and χ^2 tests represent the Sanderson and Windmeijer (2016) tests for weak identification and underidentification respectively. The null is that the endogenous regressor is respectively weakly or underidentified. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

elasticities, in the order of -0.1. The combination of theoretical arguments, the significance of the political ideology variable in the 2FE model and the impact its inclusion has on the minimum wage effect estimate suggest that political ideology is an important omitted variable in these models. Rather than picking up a causal link from minimum wage to lower employment, they pick up that as state legislatures become more liberal, they are more likely to increase the minimum wage, but also tend to correlate with lower employment in the restaurant sector. The latter is not necessarily the result of their policies - the omitted variable argument remains if the causality runs in the opposite direction.

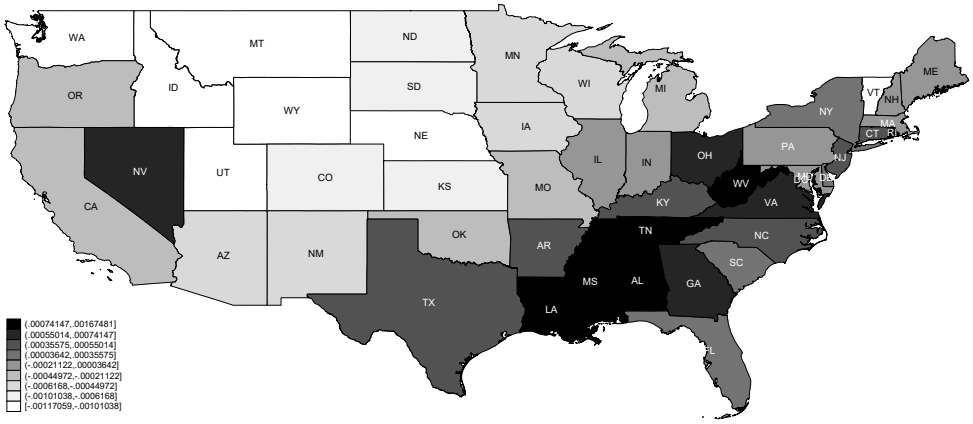
1.5.2 Enriched 2FE studies

The second largest strand of literature takes the traditional twoway fixed effects model and then adds a twist. For example, Allegretto, Dube and Reich (2011) show that adding state-specific time trends or census division fixed effects eliminate any evidence of significant disemployment effects. In our main regressions we show that the trend models were unaffected by the addition of the political ideology variable (see the Trends columns). In Figure 1.5 we show the coefficients of these state trends and below it the slope of the trend in political ideology over time for each state (positive [darker] values indicate a shift towards conservative values). The two slope coefficients are moderately correlated at 0.35, suggesting that the shift in the political landscape is at least one of the major aspects filtered out by the trends model. In that sense, our estimates provide an economic explanation for the relevance of state time trends. Likewise, the clear regional clustering of the ideology slope coefficients could explain why the census division approach and the cross border county pair approach (Dube, Lester and Reich, 2010; CBCP columns in the regression tables) led to similar results.[33] In Figures A3 and A5 in the appendix we show that the same applies to the IFE model (Totty, 2015) as the first factor it estimates strongly represents a seasonally adjusted linear time trend.

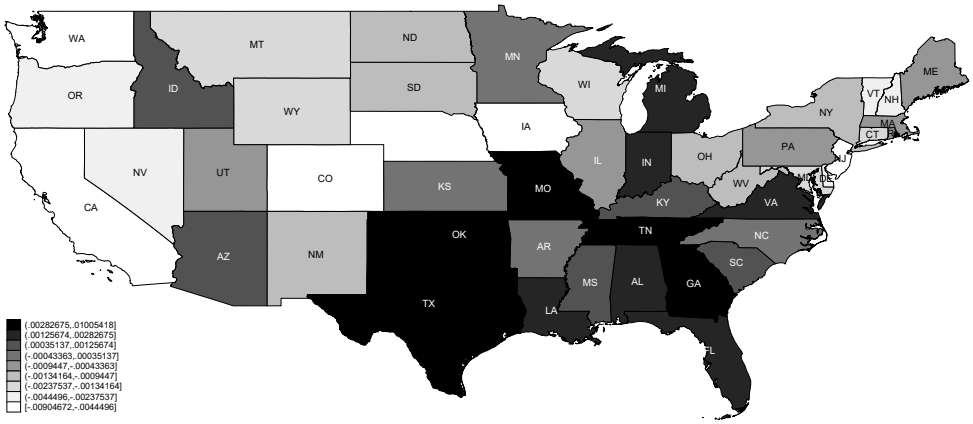
A similar argument appears to be relevant in Meer and West (2016). They argue that minimum wage changes only affect employment over the long run and use a long differences approach to identify how estimates of minimum wage effects differ as you consider different horizons. They find that disemployment effects are only visible in horizons of multiple years and that the inclusion of time trends washes out this effect. We present replicated results in Table A3 in the Appendix. Panel A shows the initial estimates as reported by Meer and West (2016), with significant disemployment effects from two years onwards. In Panel B we restrict the sample to states and years for which we have information on the political ideology of states. Roughly speaking, this reduces the timeframe from 1977-2011 to 1993-2011. This sample restriction already removes most evidence of disemployment effects, with the only significant coefficients found after five years. Adding the political ideology

[33] A variance decomposition using multi-level modeling suggests 20% of variation in the political measure is explained by the census division. 52% is due to 'fixed' state differences and the remaining 28% is time variation.

Figure 1.5: Coefficients of the state time trends.



Slopes of the trend in political ideology over time by state.



variable (Panel C) further reduces the significance of the employment effects, which is now only significant at 10% or less in six and seven year windows, although the difference is rather small.[34] Overall, it appears that the long difference framework also filters out most of the impact of political ideology and that the different outcome is due to the timeframe examined (i.e. their negative effects of the minimum wage appear to be driven by the 70s and 80s).

1.5.3 Other studies with political angle

Perhaps closest to this paper in terms of methodology is Baskaya and Rubinstein (2015). They instrument effective state level minimum wages with federal minimum wages multiplied by an estimate of the probability that they will actually be binding. This probability is derived from a probit regression of actual binding status (1977-2007) on income per capita (1968), political ideology (1960-68) and past binding status (1968-76). They find employment elasticities with regard to the minimum wage of -0.2 to -0.5. Their identification assumption is that the actual bindingness of the federal minimum wage is endogenous, but not the part predicted by historic political ideology, income per capita and bindingness in 1968-76.

There are two arguments against this assumption. First, there is relatively little variation in the predicted bindingness as we show in Figure 1.6. Half the states have a predicted probability of over 92%, only one in four has a predicted probability below 82%.[35] If the effect of the federal minimum wage itself is endogenous, then it is not clear how multiplying it by this limited range of predicted bindingness probabilities solves that issue.

Second, these predicted probabilities themselves could very well be endogenous. Indeed, when we regress the predicted probabilities on 2007 values of the political ideology measure and income per capita, we get an R^2 of 0.41. We show in this paper that political ideology is a meaningful omitted variable and the same plausibly applies to income per capita. That these variables have strong predictive power of the predicted probabilities during a federal minimum wage change implies that the instrument proposed in Baskaya and Rubinstein (2015) is the multiplication of two endogenous variables and thus does not satisfy the exclusion restriction.

The role of political ideology is also mentioned in Cengiz et al. (2019), who point out that the negative employment effects of traditional twoway fixed effects models might be driven by the 1990-92 recession. This recession hit Democratic-leaning states particularly hard. Given that these states were also more likely to increase minimum wages later on, there is a spurious correlation between negative employment effects and higher minimum wages. This might have an outsized effect on our

[34] Adding state fixed effects pushes all coefficients to zero. The original notes in Meer and West (2016) mention that the regressions already include state fixed effects, but this is not the case. The regression specification they mention (Equation 2, page 513) also does not contain state fixed effects. One reason for this might be that fixed effects in a long difference setting are very similar to time trends in levels settings.

[35] Caveat. We were not able to find these statistics in the original paper and thus cannot guarantee that we replicated their results perfectly.

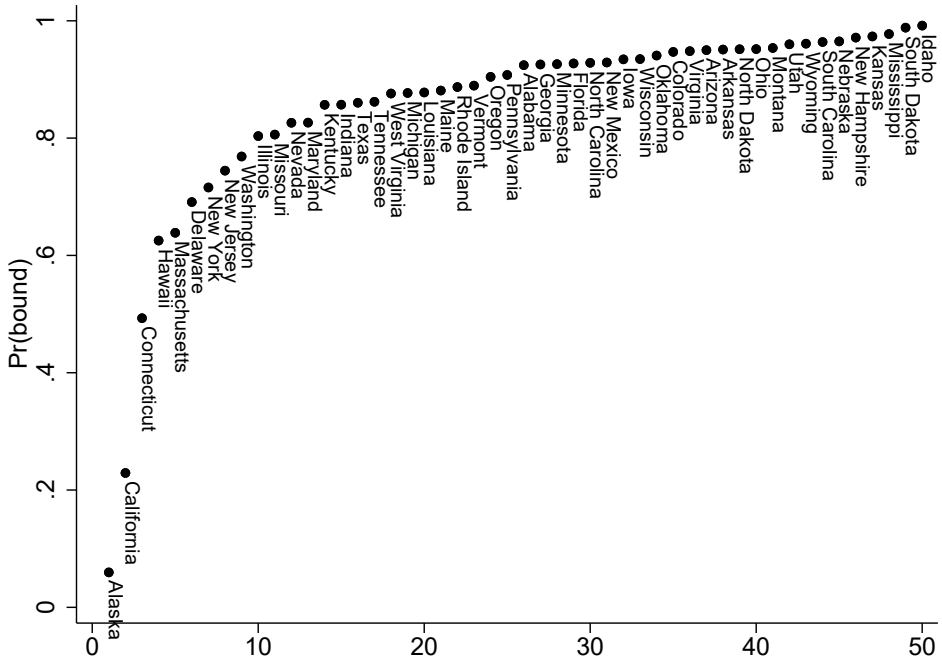


Figure 1.6: Predicted probability of being bound by the federal minimum wage.

estimate of the political ideology effect. The ideology variable is only available from 1993, so there cannot be any direct impact, but there might still be delayed effects.

We test this in Figure 1.7, where we re-estimate our models for different starting years (x-axis). The y-axis shows the elasticity of employment to the minimum wage (Panel A) and political ideology (Panel B). If anything, the graph reinforces the notion that employment was not affected by the minimum wage. The 2FE model is still the only model with significantly negative employment elasticities, but the effect becomes smaller and even insignificant as we delay the start of our sample. From 1998 onwards the 95% CI no longer includes -0.1, the traditional lower bound of minimum wage elasticities. The elasticities estimated by the politically augmented models (POL and POLIV) are never significant at 5% and their 95% CI never includes -0.1. The coefficient estimates of the ideology variable on the other hand remain significant over all starting years (except POLIV if we start in 1999 due to enlarged standard errors), suggesting our estimates are not driven by the 1990-92 recession or its aftermath.

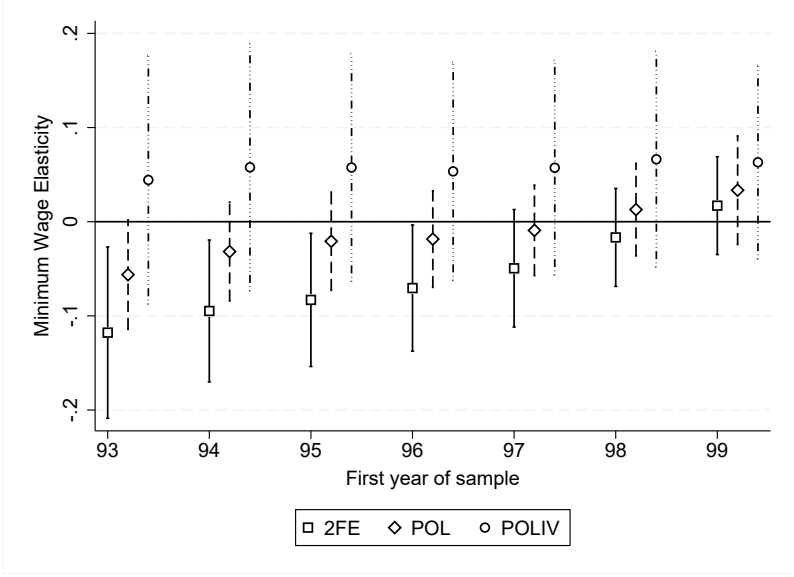
1.5.4 Difference in differences studies

Finally, we demonstrate how evaluating the political ideology of states can be a relevant exercise in difference in differences studies, using Clemens and Wither (2019) as an example.[36] They find that low wage workers were less likely to retain their employment in states bound by the 2008-2009 federal minimum wage increase, but include as caveat that they cannot truly verify to which extent treated and control states differ. Zipperer (2016) shows that differences in industry composition account for 63% of the negative impact in the original paper. We argue that these states also differed substantially in terms of political ideology.

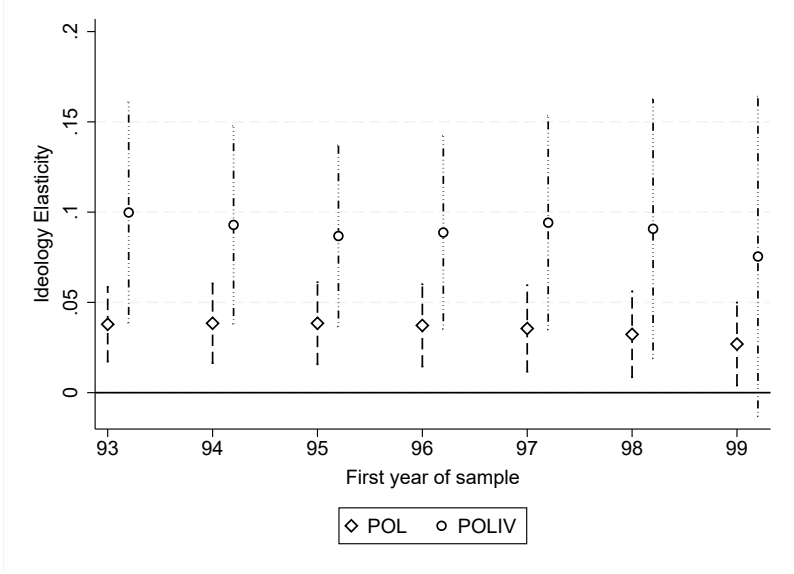
In Figure 1.8 we show that bound and control states are opposites in terms of political ideology and that these differences widened in 2009. The gap is particularly large when we use population weights (dashed lines), which are implicitly present in their regressions. At -0.81 and +0.15 in 2009, the difference between the two group means is almost a full standard deviation, covering half the range between the most liberal and most conservative state at the time. Due to the unequal distribution of population over US states, there are six states in each group that make up over 50% of their respective population. The gap between these states is even more pronounced (dash-dot lines), covering fully three quarters of the range of the political ideology variable in 2009.

This indicates we could use essentially the same state split as Clemens and Wither (2019) to contrast how low wage workers in liberal and conservative states weathered the Great Recession. These states differ in their industrial composition (see Zipperer, 2016), but also in terms of unemployment insurance, disability benefits, welfare schemes and labour protection laws. Moreover, the prevailing ideology in the

[36] Note that Clemens and Wither (2019) also perform a triple diff-in-diff where they use higher wage workers within the same state as control group. As such, the exposition below applies only to the between-state aspect of their analysis.



(a) Minimum Wage



(b) Ideology

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for total private sector employment and population. Includes county dummies and month dummies.

Figure 1.7: Employment elasticity, different starting years.

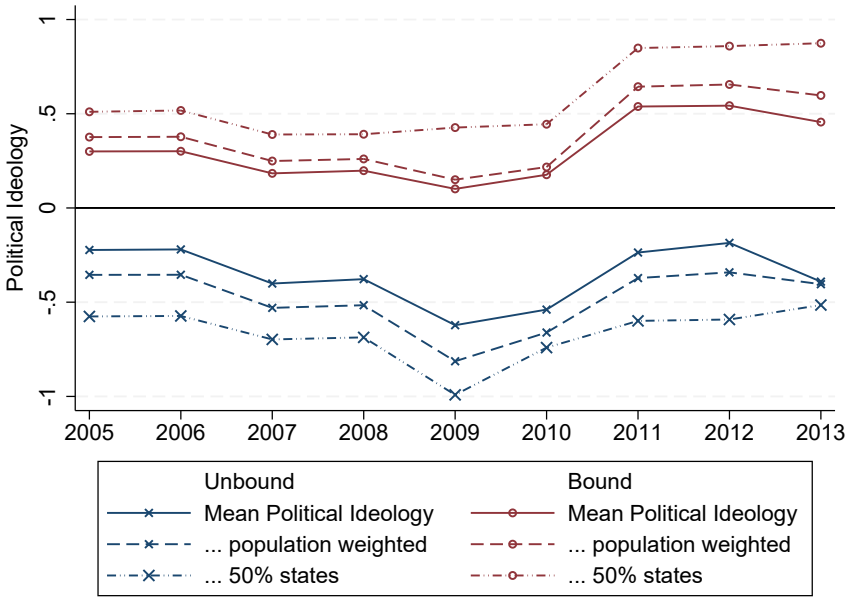


Figure 1.8: Mean ideology values for bound and unbound states.

state legislature affects how each of these change in response to the Great Recession (Campbell and Sances, 2013).

The heterogeneity in political ideology does not necessarily invalidate the results from Clemens and Wither (2019), who devote considerable attention to discussing the confounding effect of the Great Recession. Indeed, we find that over our sample conservative states tend to have higher restaurant employment which would suggest that omitting political ideology leads to an upward bias in their estimates.[37] In that sense, this paper suggests the true effect lies somewhere between the significantly negative estimates of Clemens and Wither (2019) and the insignificant ones of Zipperer (2016). Unfortunately, as the study is based on a very short timeframe, it is not feasible to replicate the study factoring in the effect of political ideology.

1.6 Robustness Checks

In this section, we compare the performance of the various existing estimators to that of the politically augmented twoway fixed effects estimator, as well as its instrumented version.[38] As is common in the literature, we start by checking whether

[37] Intuitively, the reason for this is that the states that normally raise their minimum wages are the liberal states (leading to a downward bias in panel estimates). However, this also implies that federal minimum wage hikes mainly affect the conservative states, switching the roles and thus also the direction of the bias.

[38] Results for the other methods augmented with the political measure, as well their dynamic versions are available on request.

the models lead to plausible earnings estimates ('find an effect when they should'). Then, we move to the finance sector to see if there we find a significant disemployment effect when we should not. Next, we add data on campaign contributions to the set of instruments for the political ideology variable. Finally, we show that none of our estimates exhibit pre-existing trends up to two years before a minimum wage introduction.

Table 1.5: Robustness checks (Earnings and Finance results)

		(1) 2FE	(2) Trends	(3) CBCP	(4) CD	(5) POL2FE	(6) IVPOL2FE
A Earnings	Wage elasticity	0.231***	0.179***	0.140***	0.207***	0.222***	0.169***
		(0.028)	(0.025)	(0.031)	(0.028)	(0.022)	(0.038)
	Median political ideology of house					-0.005	-0.038***
						(0.005)	(0.012)
	SW F-test pvalue						0.001
	SW χ^2-test pvalue						0.000
	N	62 364	62 364	22 496	62 364	62 364	62 364
B Finance	Employment elasticity	-0.091*	0.035	-0.118	-0.006	-0.067	-0.019
		(0.046)	(0.035)	(0.093)	(0.065)	(0.043)	(0.051)
	Median political ideology of house					0.016	0.047**
						(0.012)	(0.018)
	SW F-test pvalue						0.000
	SW χ^2-test pvalue						0.000
	N	357 552	357 552	173 856	357 552	357 552	357 552

See notes of Table 1.4. The earnings results are based on quarterly data and thus include quarterly time dummies. First stage results for the POLIV2FE results are available in columns (2) and (3) of Table A4 in the Appendix. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

1.6.1 Restaurant Earnings

Panel A of Table 1.5 shows the estimated earnings elasticities based on average weekly earnings from the QCEW (see Section 1.2).[39] All models lead to statistically significant positive results, though there is noticeable variation in effect size, ranging from 0.140 (CBCP, 3) to 0.231 (2FE, 1). None of the models lead to implausibly small or insignificant elasticities, suggesting there is enough minimum wage variation left to identify its effects. If anything, the estimates seem somewhat on the high side: A back of the envelope calculation based on the Current Population Survey Merged

[39] These include tips and other gratuities.

Outgoing Rotation Groups data (CPS-MORG) suggests a wage elasticity of 0.10, assuming everyone below the new MW had their wage topped up and those above were unaffected (and no endogeneity).[40] Estimates from the politically augmented twoway fixed effects model (5) sit between the results from both sides of the debate, implying that a 10% increase in the minimum wage raises average weekly earnings by 2.22%. Its instrumented version suggest a rather smaller raise (1.69%, column 6). Note that these positive *weekly* earnings effects suggest that any unobserved reductions in hours worked are bound to be limited in size.

Table 1.6: Own Wage Employment Elasticities (restaurant sector)

	2FE	Trends	CBCP	CD	POL2FE	IVPOL2FE
Own Wage Elasticities	-0.51	-0.12	-0.46	0.03	-0.22	0.26

Wage elasticities from Table 1.5, employment elasticities from Tables 1.4 and 1.2. Note that only the employment elasticity of the 2FE model was statistically significant (at 5%).

We show own wage employment elasticities in Table 1.6. Even for the 2FE model we find an own wage employment elasticity of only -0.51, indicating that the negative employment effect we found in this model is still smaller than the increase in average earnings. This also holds for the political ideology models, which find own wage employment elasticities of -0.22 (POL2FE) and +0.26 (IVPOL2FE). These are solidly in the small effect group if we follow the classification of Dube (2019) and very close to the median literature-wide estimate of -0.17 (Dube, 2019).

1.6.2 Finance Employment

Next, we perform a placebo test by applying the various models to the finance sector (NAICS 52). Only 2-3% of employees in that industry earn up to 1.25 times the minimum wage (the corresponding number for the restaurant sector is 30%).[41] Panel B of Table 1.5 shows the relevant employment elasticities. The 2FE model predicts a remarkably similar disemployment effect (-0.09, column 1) in this plausibly unaffected sector as in the restaurant sector, with the 95% CI spanning [-0.18, 0.00]. The median wage of tellers, the lowest paid occupation in the finance sector, was $12.69 in 2015, almost twice the federal minimum wage and more than $2 above the highest state minimum wage at the time.[42] As a result, it would take severe ripple-on effects for the minimum wage to affect employment (through wages) that far up the distribution. The CBCP model (column 3) also produces a negative point estimate,

[40] The existence of wage spillovers above the wage floor is a subject of ongoing debate, see e.g. Grossman (1983), Autor, Manning and Smith (2016).

[41] If we compare current earnings to the upcoming minimum wage, we get respectively 4% and 40%. Calculations based on CPS-MORG data.

[42] Occupation level earnings data come from the Occupational Employment Statistics database.

but it is very imprecisely estimated (the 95% CI is [-0.29,0.07]).[43] Indeed, one advantage of the politically augmented models is that they retain all variation in minimum wages, avoiding issues of reduced power. The other models (Trends, CD, POL2FE and IVPOL2FE) are more consistent in the sense that they never point to economically and statistically significant disemployment effects.

Table 1.7: Campaign Contribution based Political Ideology Instruments

Instrument	N	Mean	SD	Min	Max
Republican donations (count)	928	12673.38	17548.22	0	246094
Democrat donations (count)	928	14076.60	19516.20	17	272162
Party contribution (R, millions)	928	1.38	2.41	0	14.73
Party contribution (D, millions)	928	1.35	2.67	0	23.41
Total amount donated (R, millions)	928	15.20	23.74	0	248.22
Total amount donated (D, millions)	928	15.02	23.58	0	203.50
Individual vs PAC donations (share)	926	0.55	0.21	0.05	0.99

1.6.3 Campaign Contribution Instruments

Finally, we expand our set of instruments by adding campaign contribution data from the Database on Ideology, Money in Politics, and Elections (DIME; Bonica, 2015). The dataset provides the number of donations to each candidate in state elections as well as their total value. Donors are split between individuals, Political Action Committees (PAC) and party committees. Table 1.7 lists some summary statistics for the seven extra instruments we derive from DIME. These are the number of donations per party (per election cycle per state), contributions by the respective party committees, total amount of donations per party and the share of contributions derived from individual donations relative to PAC donations.

All of these are plausibly exogenous to restaurant employment levels and provide an alternative source of variation, which is also tied more directly to the election cycle. However, relative to the social values, they are a lot closer to the world of economics and there is thus more chance of some convoluted link between the contribution instruments and our dependent variable. For this reason, we excluded them in the main regressions but still evaluate the conclusions they entail. Note however that issues can only arise if this convoluted link persists in the context of county and month fixed effects as well as the total private sector control.

Table 1.8 shows the first and second stage regression results for restaurant employment, restaurant weekly earnings and finance employment. The coefficients in the first stage appear plausible: more Democrat donations are associated with more

[43] This is partly due to the balanced panel requirement, which is particularly binding for the CBCP model as it requires county pairs to be fully available. If we drop that requirement, the 95% CI narrows somewhat to [-0.24, 0.06].

Table 1.8: First and Second Stage of Instrumented 2FE regressions - With Contribution Instruments

		(1) Ideology	(2) Ideology	(3) Ideology
1^{st} Stage	Abortion always allowed	0.110	0.112	0.037
		(0.284)	(0.286)	(0.339)
	Atheist	0.788***	0.795***	0.795***
		(0.285)	(0.285)	(0.272)
	Increase federal crime spending	0.371	0.374	0.290
		(0.345)	(0.345)	(0.332)
	Dislike military†	0.624	0.631	1.049*
		(0.579)	(0.580)	(0.604)
	Dislike gays†	-0.538	-0.543	-0.463
		(0.398)	(0.397)	(0.380)
	Equal Rights gone too far	0.002	0.004	0.181
		(0.455)	(0.454)	(0.448)
	Like Big Business†	0.440*	0.445*	0.458*
		(0.257)	(0.257)	(0.252)
	Govt should offer little support	0.585**	0.591**	0.550***
		(0.237)	(0.236)	(0.209)
	Blacks have not been discriminated	-0.183	-0.182	-0.012
		(0.320)	(0.321)	(0.308)
	New Lifestyles hurt society	0.588	0.591	0.646**
		0.306	0.309	0.381
	# of Republican donations (log)	0.102	0.102	0.120*
		(0.074)	(0.074)	(0.072)
	# of Democrat donations (log)	-0.175**	-0.175**	-0.174**
		(0.070)	(0.070)	(0.076)
	Total amount donated (R, log)	0.112**	0.112**	0.105**
		(0.056)	(0.056)	(0.052)
	Total amount donated (D, log)	-0.017	-0.017	-0.015
		(0.060)	(0.061)	(0.057)
	Republican Party contributions (log)	-0.011	-0.011	-0.019
		(0.030)	(0.030)	(0.029)
	Democrat Party contributions (log)	0.027**	0.027**	0.026*
		(0.012)	(0.012)	(0.015)
	Individual vs PAC donations (share)	-0.426*	-0.426*	-0.381*
		(0.223)	(0.224)	(0.219)
	Minimum Wage (log)	-0.811*	-0.819*	-0.764*
		(0.460)	(0.463)	(0.423)
	Population (log)	0.035**	0.037**	0.037**
		(0.016)	(0.015)	(0.017)
	Total Private Employment (log)	0.043		-0.014
		(0.129)		(0.144)
	Total Private Earnings (weekly, log)		0.150	
			(0.168)	
		Emp Rest	Earnings Rest	Emp Finance
2^{nd} Stage	Median political ideology of house	0.102***	-0.025***	0.037*
		(0.024)	(0.009)	(0.019)
	Minimum Wage (log)	0.056	0.165***	0.039
		(0.057)	(0.027)	(0.064)
	Population (log)	0.046***	0.006***	0.053***
		(0.006)	(0.002)	(0.005)
	Total Private Employment (log)	0.574***		0.379***
		(0.056)		(0.044)
	Total Private Earnings (weekly, log)		0.186***	
			(0.028)	
	N	171576	57192	324744
	SW F-test pvalue	0.000	0.000	0.000
	SW χ^2-test pvalue	0.000	0.000	0.000

See notes of Tables 1.4, 1.5 and A4.

liberal governments (-0.175***), higher Republican contributions with more conservative ones (0.105 to 0.112**) and elections where individual donations are more important (relative to PAC money) produce more liberal legislatures (-0.381 to -0.426*). In the second stage, we find the same results as in the main regressions: conservative governments are associated with higher restaurant (+0.10***) and finance sector employment (+0.037*), but lower restaurant earnings (-0.025***). The minimum wage has no effect on employment, neither in the restaurant nor in the finance sector, but does raise earnings in the restaurant sector (a 10% increase leads to a 1.65% increase in average weekly earnings).

1.6.4 Pre-existing trends

Equation 1.5 illustrates how we test for pre-existing trends in employment and earnings.[44] Apart from the summation term, the equation is identical to the main regression equation (Equation 1.2). The δ_j coefficients measure in half year increments whether the dependent variable behaved differently before changes in the minimum wage. The indicator function ensures that $\Delta mw_{i,t+6j}$ is non-zero only when it refers to the upcoming minimum wage. That is, if by $t+6j$ the minimum wage has changed more than once, the difference term is set to zero.

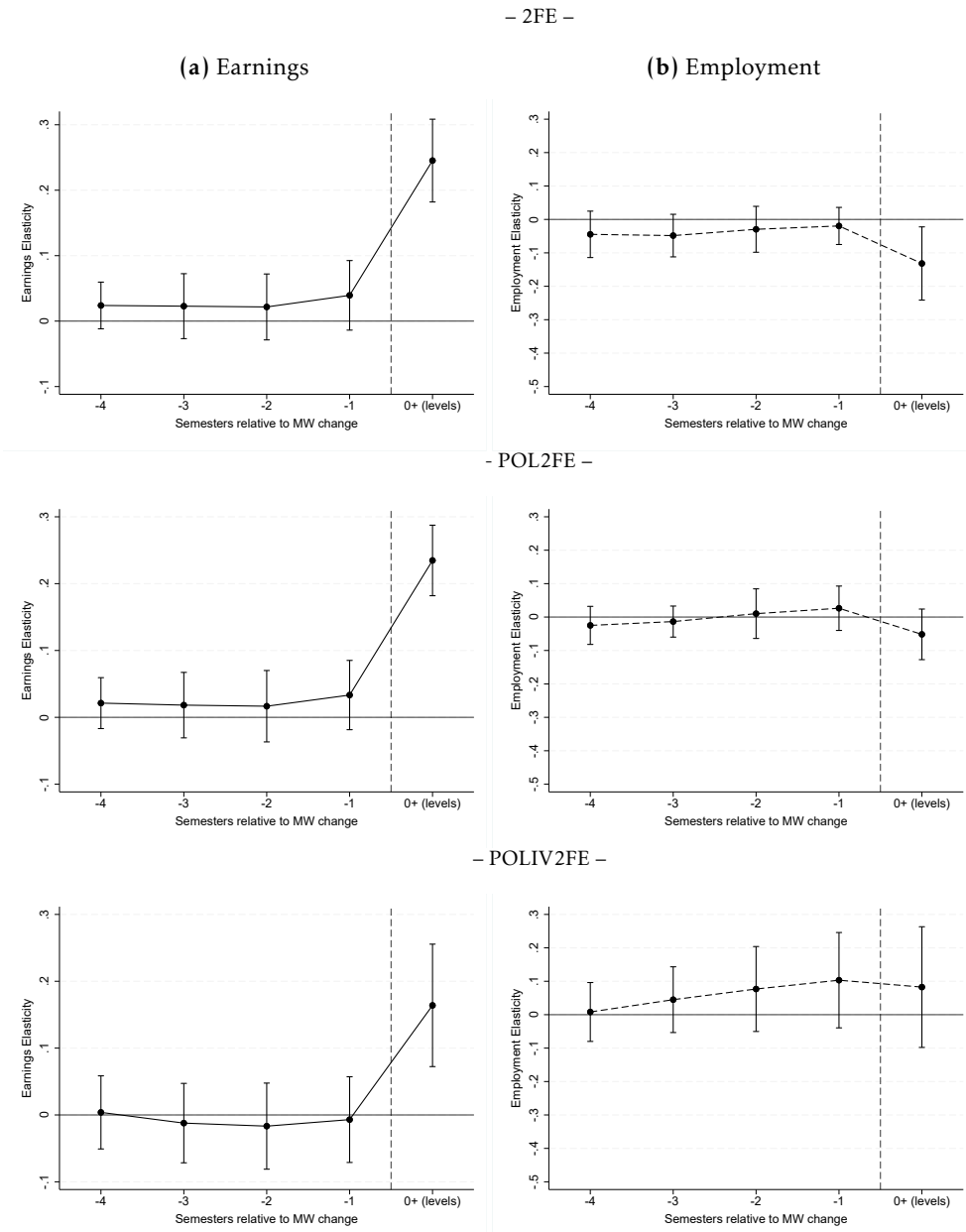
$$e_{it}^{R} = \beta * mw_{it} + \sum_{j=1}^{4} \delta_j * \Delta mw_{i,t+6j} * \mathrm{I}(mw_{i,t+6j} \text{ is next MW}) + \gamma_1 * e_{it}^{TOT} + \gamma_2 * pop_{it} + \alpha_i + \tau_t \left[+ \delta * POL_{it} \right] + \epsilon_{it} \tag{1.5}$$

The results are shown in Figure 1.9, with earnings in the left panel and employment elasticities on the right. We do not find evidence of pre-existing trends for any method, with the estimates for the 2FE, POL2FE and POLIV2FE models averaging to respectively -0.03 (se 0.02), 0.02 (se 0.02) and -0.01 (se 0.03) for earnings and -0.04 (se 0.03), -0.00 (se 0.03) and 0.06 (se 0.05) for employment. The levels estimate of the minimum wage effect does not change qualitatively after the addition of the pre-trend variables.

1.7 Conclusion

The impact of minimum wage policy on employment levels is hotly debated, with elasticity estimates very sensitive to the exact specification. These conflicting findings can be reconciled once we account for the impact changes in the state-level political situation have on both employment and the level of the minimum wage. In particular, we show that introducing a measure of political sentiment to the twoway fixed effects specification, the only to consistently point to negative employment effects, causes

[44] e_{it} refers respectively to employment or earnings.



Estimates from a regression with the log minimum wage in levels as well as its 1-4 semester leads. Leads set to zero if they do not refer to the upcoming minimum wage (but the one thereafter).

Figure 1.9: Elasticity with respect to the minimum wage.

any evidence of a statistically and economically significant disemployment effect in the restaurant sector to vanish. It also provides an alternative explanation for the negative effects found in several other studies. At the same time, we provide arguments to suggest that it is indeed these political shifts that are filtered out by the statistical methods espoused by e.g. Dube, Lester and Reich (2010) and Allegretto, Dube and Reich (2010).

Our preferred specification, the twoway fixed effects estimator augmented with an instrumented political measure as control variable, leads to credible wage elasticities, no disemployment effects in the restaurant sector and unlike the traditional 2FE estimator, none in the de facto unaffected finance sector either. As a result, we conclude based on current data and modelling techniques that there is very little credible evidence to suggest minimum wages adversely affect employment opportunities. Given their positive impact on bottom decile earnings, this implies minimum wage policy can be a valuable tool against (working) poverty.

There are two major caveats. First, the impact of a large minimum wage shock (e.g. the Living Wage Adjustment, from $7.25 to $15) might be very different from the moderate changes observed so far. Second, minimum wages may have different effects in different contexts. We find no average effect, but a high federal minimum wage could still be detrimental in places with low cost of living and wages, which appears an interesting venue for future research (see e.g. Thompson, 2009 and Wursten, 2020).

Chapter 2

Estimating the earnings and employment effects of the minimum wage through differences in exposure across US counties

We assess the impact of minimum wage changes on employment and earnings by exploiting wage variation within US states. We split counties into low, medium and high vulnerability groups. Our method is a hybrid of difference in differences and event study models, allowing us to pool all minimum wage events between 1990-2017. Applied to the food services sector, we find that earnings effects increase strongly in vulnerability, whereas differences in employment effects are small and fragile. The earnings results persist when evaluating other low wage sectors or teenagers, whereas any evidence of disemployment effects disappears.

2.1 Introduction

The steady barrage of minimum wage changes in the US offers an almost unique opportunity to learn more about the structure of labour markets and how they respond to outside shocks. Card and Krueger (1994) concluding that the minimum wage hike in New Jersey did not reduce employment led to a wealth of new and refined theories on labour market structures (e.g. Flinn, 2006; Aaronson, French and MacDonald, 2008; Sorkin, 2015). The implications and value of these models depends strongly on the actual response of employment and earnings to minimum wage policy. Yet, the findings in Card and Krueger (1994) are still disputed two decades later (e.g. Neumark and Wascher, 2000 and Ropponen, 2011) and while the empirical literature

has expanded greatly, it has only become more fractured, to the extent that even the conclusions of meta studies are disputed (e.g. Neumark and Wascher, 2006; Neumark, 2019; Wolfson and Belman, 2019 vs Belman and Wolfson, 2014; Dube, 2019). The discussion is of particular relevance because many states are considering the implementation of a $15/hour 'living wage', a policy even advocated at the federal level. The range of existing minimum wage studies implies this doubling could cost between no jobs and 30% of all employment in the restaurant sector and amongst teenagers (resp. Cengiz et al., 2019 and Thompson, 2009). At the lower end of losses, minimum wage policy could be seen as a transfer to employees from employers and customers. At the higher end, the only winners are those who retain their job.

In this paper we evaluate whether minimum wage shocks lead to stronger employment and earnings effects in vulnerable counties. We define vulnerability as the share of food services workers affected by a hypothetical 10% increase in the minimum wage and use this share to divide counties into high, low and medium vulnerability groups. We use local projection methods (Jorda, 2005) to pool all minimum wage events and include state-time dummies to ensure we compare counties within states. As such, our method combines the best of the three main strands of the literature: a) the difference in differences characteristics insulate our results from state level endogeneity, b) the local projection aspects increase external validity by allowing us to consider multiple minimum wage events simultaneously and c) the panel structure provides a flexible way of controlling for county characteristics.

Reassuringly, we find that earnings effects in the food services sector increase monotonically with our vulnerability measure. That is, earnings react strongly in counties we classify as highly vulnerable, moderately in the 'greyzone' middle and barely in the low vulnerability group. On the contrary, we find small and non-robust differences in employment elasticities. When we expand the analysis to cover all plausibly affected sectors, we still find positive earnings elasticities of the expected pattern, whereas the employment effects are both statistically and economically insignificant. Moreover, we find qualitatively similar results when we look at teenagers across all industries. We do not detect any pretreatment trends and the results are robust to different definitions of the vulnerability measure, a host of specification tweaks and changing to a different dataset.

Our conservative estimate of a 10% increase in all minimum wages suggests it would reduce nationwide food services employment by 0.1% whereas earnings would increase by 0.2%.[1] The respective numbers for a $15 federal minimum wage are -0.7 and +1.4 percent, however this increases to -2.9 and +7.9 percent if we calculate vulnerability relative to a $15 minimum wage.

The novelty of this paper lies in its combination of existing research designs. For example, the tradition of using variation in wage levels to identify minimum wage

[1] The estimate is conservative in the sense that it assumes both earnings and employment effects to be zero in the low vulnerability group, even though we find some evidence to suggest those counties saw some earnings gains but no disemployment effects whatsoever. If we factor in those effects, the earnings gains jump to +0.9 whereas the employment losses increase only slightly to -0.2.

effects goes back at least to Kaitz (1970) and Card (1992).[2] However, where their variation is on respectively the time series and state level, we move down a level of granularity by looking at counties within states. We also build on earlier studies that do look at county level variation and expand their scope, e.g. Even and Macpherson (2019) uses variation in the share of affected employees between counties and industries and finds employment elasticities that range between -0.24 and -0.38, but only considers the state of California.[3] Similarly, Thompson (2009) uses a difference in differences approach to compare counties where the federal minimum wage changes in 1996-1997 were likely to be binding to those where it was not. He too finds strong negative effects, with employment elasticities for teenagers between -0.26 and -0.57.[4]

A second strand of the literature does include an expanded set of minimum wage changes, but remains susceptible to state-level endogeneity. E.g. Addison, Blackburn and Cotti (2012) includes an interaction between the minimum wage and average weekly wages in one of their standard panel regressions. They find that earnings increased less in counties with relatively high wages. However, these weekly wages are not scaled relative to the minimum wage, nor do they include state-time dummies to filter out state level endogeneity. Our work is complementary to Godoy and Reich (2019) who use the American Community Survey to differentiate coumas by their median wage to minimum wage ratio.[5] They find strong wage but no employment effects for teenagers and individuals with only high school education or less based on a study of 51 minimum wage events. We study different population groups, use a more expansive set of minimum wage changes, use a different measure to identify vulnerable areas and employ a different estimation strategy.

Our study is also closely related to the cross border county pair method of Dube, Lester and Reich (2010), who compares counties directly across state borders with different minimum wages and only finds an impact on earnings. Their approach assumes that close counties are more alike, whereas legislative borders do not matter. In this paper, we make the opposite assumption: we compare counties from all corners of a state, but always stay within the same jurisdiction. Additionally, where they work in levels of earnings, employment and the minimum wage, we focus on a differenced setup to explicitly restrict the temporal distance between cause and effect. This prevents us from estimating longterm effects, but in turn strongly limits our susceptibility to slow-moving confounding factors. This aspect of our method is related to the long differences in Meer and West (2016) which we discuss in Appendix 2.

The contribution of this paper is threefold. First, to our knowledge we are the first comprehensive study of minimum wage effects that is robust to state level endogeneity. This is particularly relevant when minimum wage hikes arise together with

[2] It is also commonly used to study national shocks to the minimum wage, e.g. Caliendo et al. (2018) in Germany.

[3] Because they only consider California, they implicitly control for state level endogeneity.

[4] Clemens and Wither (2019) do a similar exercise on the individual level for the 2007-2009 federal minimum wage episode and find negative employment effects for low wage workers.

[5] A couma is a combination of a county and a Census public use micro area, see Case and Deaton (2017).

other policy changes (see e.g. Wursten, 2019), which does not show up in pretreatment trends and thus cannot be detected by cross-state studies. Second, although we believe the heterogeneity of minimum wage effects is interesting by itself, it would also be a valuable validation exercise for any minimum wage study. I.e. a method that finds equally large earnings effects in highly vulnerable and barely affected counties might not be picking up the causal effect of the minimum wage. Third, we demonstrate that the local projection method can also be applied outside macroeconomics. It is more transparent in the context of overlapping treatments than event studies because the local projection method separately estimates effects at each horizon. If there are no anticipation effects, any bias arising from overlapping horizons shows up only in the pretreatment period (and is thus visible) whereas the post treatment estimates remain unaffected. Unlike difference in differences studies, it is straightforward to implement this method even when multiple units are treated multiple times.

We lay out our narrative as follows. We start by introducing the vulnerability measure in Section 2.2 and our empirical approach in Section 2.3. Section 2.4 describes the data and institutional context and Section 2.5 shows the results. In Section 2.6 we show various robustness checks, compare our method to common models of the literature and present results for other sectors and teenagers. We perform several policy simulations in Section 2.7. Finally, we discuss the relevance of our new insights in Section 2.8, which is also the conclusion.

2.2 Vulnerability Measure

The key challenge in this paper is to identify how strongly each county would be affected by a minimum wage change. We cannot use the Current Population Survey (CPS) or American Community Survey (ACS) as they have only limited geographic information.[6,7] Instead, we start from county-quarter level earnings data from the QCEW[8] and augment it with lower granularity data from the CPS, CES[9] and OES[10]. This leads to an estimate of how many food services workers would require a pay jump to comply with a 10% minimum wage increase for each county and quarter.

We summarise the procedure in Figure 2.1. Minimum wages in the US are defined on the hourly level, so step 1 is to convert the quarterly earnings from the QCEW to an hourly level. We calculate average hours worked per quarter from both the CPS

[6]The CPS only provides info for 410 out of 3142 counties and the ACS only shows county information for the most populous counties and is thus likely to exclude the truly vulnerable areas. For an approach that does manage to use the ACS, we refer to Godoy and Reich (2019) who supplement the county information with public use microdata areas (see also Case and Deaton, 2017).

[7]We want to use counties as geographical unit as this allows us to use the rich and high quality information in the Quarterly Census of Employment and Wages and the Quarterly Workforce Indicator datasets.

[8]QCEW: Quarterly Census of Employment and Wages, see also Section 2.4.2.

[9]CES: Current Employment Statistics

[10]OES: Occupational Employment Statistics

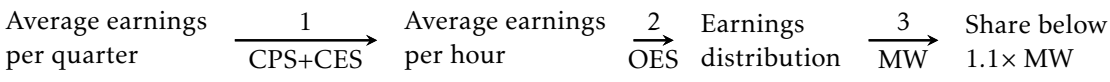


Figure 2.1: Construction of the vulnerability measure.

and the CES. The CPS provides hours worked per *week* for each state from 1990-2017 but has no reliable information on the number of weeks actually worked per quarter. The hours worked per quarter estimates from the CES do factor in variation in weeks worked per quarter, but are only available at the state-level from 2007 onward. Therefore, we regress the CES measure on the CPS measure using the 2007-2018 data, with state-specific coefficients. We then predict CES values for all states and years in our sample. On average the CPS measure overestimates hours worked per quarter by 20%. The combined measure accounts for variations between states and years, and factors in that food services workers rarely work all weeks of a quarter. We can then divide quarterly earnings by average hours worked per quarter to obtain average hourly earnings.

Step 2 is to convert these average hourly earnings into a wage distribution. The OES relates average hourly earnings to earnings at the $10/25/50/75/90^{th}$ quantile for each state. We apply this distribution to the county-quarter level average earnings and interpolate linearly between the quantile points. This provides a cumulative distribution function of hourly earnings for each county-quarter.

We illustrate the process so far for the Medina, Jack and Dallas counties, respectively the 33rd, 66th and 100th quantile of highest earning counties in Texas in the first quarter of 1994, with average quarterly earnings of \$1703, \$1937 and \$2730. Based on CPS data, the average food services worker in Texas worked 36.13 hours per week in 1994. However, the CES data tell us that in Texas we need to subtract 20% to extrapolate that to quarterly hours, giving us 375 hours per quarter rather than 470. Dividing quarterly earnings by quarterly hours, we get average wages of \$4.54, \$5.16 and \$7.28 per hour. Next, the OES tells us that the bottom tenth percentile of workers in the leisure and hospitality sector in Texas earn 77% of the mean wage, increasing to 82% for the 25th percentile, all the way to 139% at the 90th percentile.

Step 3 then is to calculate for each county-quarter which share of food services workers earns less than 1.1x the prevailing minimum wage. For the example counties, the minimum wage in 1994q1 was \$4.25, a 10% increase would bring this to \$4.67. Inverting the county-quarter level cdf then tells us the hypothetical share of affected workers in 1994q1 would be 76% in Medina County, 53% in Jack County and less than 10% in Dallas County.

Finally, we smooth the shares over two years and take the median over time to get a county-level measure of vulnerability.[11]

Figure 2.2 shows the distribution of affected shares. There is a clear spike at 10%,

[11] Results remain identical if we do not smooth, take the mean instead of the median or allow vulnerability to change over time, see Section 2.6.

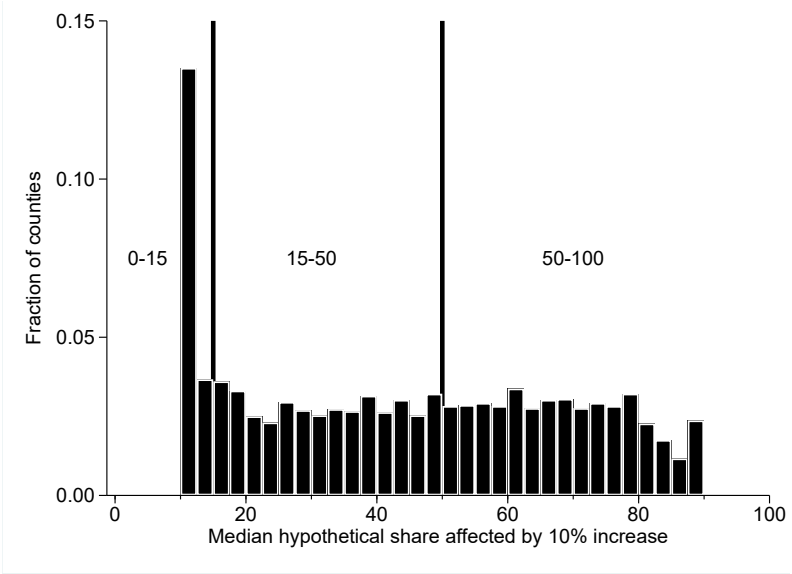


Figure 2.2: Distribution of hypothetical shares, with base thresholds.

the lowest value we can identify from the OES wage distribution. The distribution is then fairly uniform until the small jump at 90%, the highest value we can identify. These shares contain a non-negligible amount of measurement error (see Data Appendix 1 for a discussion). Therefore, in our main regressions we split counties into three broad groups: a low vulnerability group with median shares below 15%, a high vulnerability group where the share is above 50% and a middle 'grey zone' group in between.[12] We run regressions separately for the high and middle groups to ensure that the estimates are not driven by the choice of middle threshold. Moreover, we present results for a range of bottom thresholds and show that they change in the expected way. We validate our measure through the earnings effects, which are monotonically increasing in county vulnerability and significantly different between groups (see Section 2.5), suggesting our measure effectively reflects county vulnerability to minimum wage changes.

Figure 2.3 shows the vulnerability status of each county and a video showing group membership and minimum wage changes over time can be viewed on Youtube: `https://youtu.be/x-wWiSutVUc`.

[12] For the three example counties, the medians are 49, 32 and 10%. Medina County and Jack County would end up in the middle 'greyzone' group, whereas Dallas County is considered a low vulnerability county. An example of a highly vulnerable county would be Runnels County, with quarterly earnings in 1994q1 of just $1495 and a median affected share of 80%.

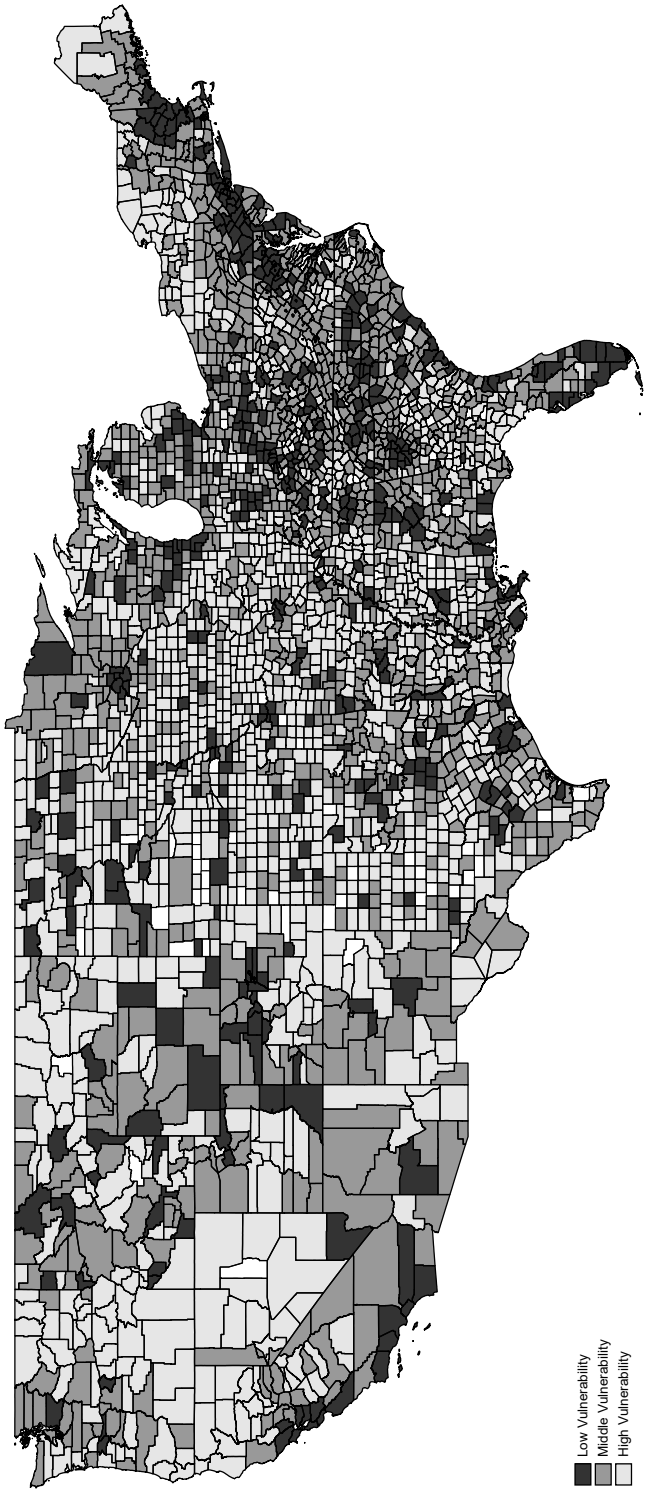


Figure 2.3: US Counties and their vulnerability status.

2.3 Empirical Strategy

The goal of this paper is to credibly estimate the causal effect of minimum wage policy on employment and earnings in low-wage sectors. In order to maximise external validity, our method incorporates all minimum wage changes between 1990-2018.[13] We take two steps to maximise internal validity. First, we compare counties within state borders to avoid state-level endogeneity. The presence of state-level confounders is well established, e.g. Wursten (2019) shows that there is a strong ideological slant to minimum wage policy which also affects restaurant employment through other channels. Allegretto et al. (2013) shows that there are fundamental differences between states that raise their minimum wages and those that track the federal rate. Cengiz et al. (2019) finds that employment changes in the upper tail of the wage distribution are correlated with the minimum wage, even though these employees earn too much to be affected by the wage floor, even indirectly. Hitherto, the solution has been to impose restrictions on the scope and form of these confounders, e.g. Wursten (2019) only filters out politics-based endogeneity, whereas the various statistical tweaks in the literature impose strong functional form restrictions (e.g. state-level trends that are linear (Allegretto, Dube and Reich, 2011) or region-time dummies at the census division level (Dube, Lester and Reich, 2010)).

Second, we move away from the levels specification most popular in the literature (with some notable exceptions)[14] and estimate everything in differences. The advantages and disadvantages of this approach are closely linked. Regressions in levels estimate long term correlations between variables, i.e. *are higher minimum wages associated with lower employment*? This is the most interesting question to answer, but it is also most vulnerable to omitted variable bias and thus less likely to be causal. In differences, the question changes subtly to, *how does employment change when the minimum wage changes*?

If both employment and the minimum wage are in first differences, the coefficient only picks up contemporaneous effects. However, the impact of minimum wages could very well be delayed, especially with regards to employment (Meer and West, 2016). We turn to the local projections approach to flexibly estimate the dynamic effects of the minimum wage (Jorda, 2005). The local projection method estimates a separate regression for each time horizon (three quarters ahead, two quarters ahead, ..., three quarters after). In each regression, the dependent variable and any relevant control variables are differenced over the horizon:

$$\begin{aligned}\Delta_h y_{it} = \beta_{h,j}\Delta mw_{st} \times I(g_i = j) + \delta_{1,h}\Delta_h y_{it}^{TOT} + \delta_{2,h}\Delta_h pop_{it} \\ + \theta_{iq} + \phi_{st} + \epsilon_{it}\end{aligned} \tag{2.1}$$

The dependent variable y_{it} is either the employment headcount or average weekly earnings in the food services sector in county i and quarter t. All lower case variables

[13] We do exclude counties affected by sub-state minimum wage policies from our sample.

[14] Meer and West (2016) for example use long differences, which we discuss in more detail in Appendix 2.

are in logs. We include the total private sector equivalent of the dependent variable y_{it}^{TOT} to control for the business cycle and population pop_{it} as it could affect both the demand for food services and the supply of food services workers.[15] θ_{iq} is an interaction of county dummies and quarter of year dummies which filter out season-specific county fixed effects. The state-time dummies ϕ_{st} remove all variation between states. As a result, the log minimum wage variable Δmw_{st} only enters the equation through its interaction with the vulnerability indicator $I(g_i = j)$, where j ∈ {high, middle, low}. In order to minimise the impact of our threshold selection, we always include just two out of three groups, e.g. in our main results we compare high and low vulnerability counties.

Finally, we take long differences Δ_h of the dependent variable and the controls. Instead of working in levels, we look at evolutions over horizons h, with separate regressions for each horizon.[16] For example, if $h = 3$, then $\Delta_h emp_{it} = emp_{i,t+3} - emp_{i,t-1}$ and we would thus estimate the impact of minimum wage changes on employment three quarters later. Conversely, if $h = -2$, then $\Delta_h emp_{it} = emp_{i,t-1} - emp_{i,t-2-1}$ and we would estimate whether employment in high vulnerability counties was already different before the minimum wage change.

As a result, we should interpret our coefficient of interest, $\beta_{h,j}$ as the cumulative effect of the minimum wage over horizon h in group j relative to the effect in the omitted group (e.g. high vs low vulnerability counties). As both dependent variable and minimum wages are in logs, this effect can be interpreted as a difference in elasticities, e.g. a 10% change in the minimum wage leads to a $0.1 * \beta_{h,j}$ percentage point larger change in employment in counties of group j than in those belonging to the omitted group.

This approach has three main limitations. First, we are limited to estimating relative effects, e.g. of high versus low vulnerability counties. While we believe this heterogeneity is informative by itself, we will also present arguments to suggest that the low vulnerability counties are indeed barely affected, suggesting the relative effects are close to the total ones. Second, as we estimate separate regressions for each horizon, we can only get to short term effects. If we make the horizon too long, the estimates become the sum of multiple minimum wage effects. In return however, we do not suffer from the overlapping treatment problem raising increasing attention in the literature (e.g. Goodman-Bacon, 2018). Any overlap would be visible as post-treatment effects showing up in the pre-treatment estimates, while the post-treatment effects remain unbiased (as long as there are no anticipation effects). Moreover, the definition of long term effects is not very clear for a policy that changes yearly in many states and is eroded by inflation (Sorkin, 2015). Finally, we are still susceptible to county level endogeneity. We can test for structural differences by observing pretreatment trends. But if states change multiple policies simultaneously with minimum wages and these affect counties heterogeneously, this will not show up in the pretreatment trends. That said, our methodology would at least filter out the average

[15] We filter out food services employment from the total private sector employment control, likewise for the earnings equivalent.

[16] We omitted the h-subscript in Equation 2.1 for θ_{iqh}, ϕ_{sth} and ϵ_{ith} to keep notation light.

effect of those other policies, which would be fully attributed to the minimum wage in cross-state studies.

2.4 The Data

2.4.1 Minimum Wages

Structured, regression-ready data on state level minimum wages were obtained from the personal website of David Neumark (Neumark, Salas and Wascher, 2014*b*).[17] We observe 460 minimum wage changes (counted at the state level), of which 115 are due to federal minimum wage changes, 345 are state minimum wage changes and 134 of those are the result of indexation at the state level. Figure 2.4 shows these state level minimum wages over time. The lowest line is the federal minimum wage, which increased seven times during our timeframe. The other lines represent the states which went beyond the federally mandated minimum. We exclude counties affected by local minimum wage ordinances (e.g. city or county laws) based on the UC Berkeley Labor Center Inventory of US City and County Minimum Wage Ordinances.[18]

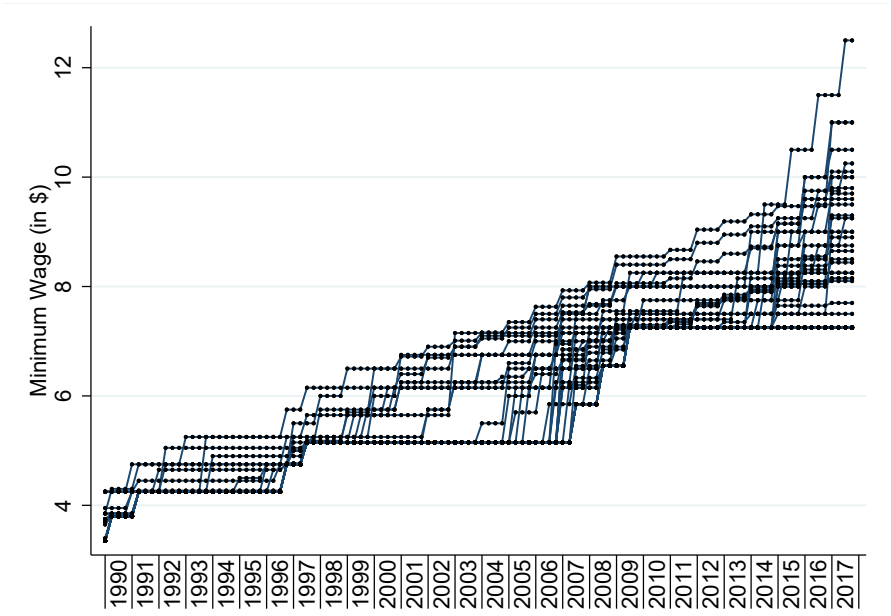


Figure 2.4: Evolution of state minimum wages between 1990 and 2017.

[17] Wayback Machine Mirror: https://web.archive.org/web/20180604085025/http://www.economics.uci.edu/~dneumark/datasets.html

[18] Wayback Machine Mirror: https://web.archive.org/web/20190126014543/http://laborcenter.berkeley.edu/minimum-wage-living-wage-resources/inventory-of-us-city-and-county-minimum-wage-ordinances/

The average increase in the minimum wage was 8.7%, but there has been considerable heterogeneity. A quarter were smaller than 4.1% (mostly cost of living adjustments) and there were a few rather large ones, most noticeably the 35% increase in 2007 in Michigan, from $5.15 to $6.95. To date, Colorado is still the only state to have ever lowered the minimum wage (-0.4% in 2010, due to a decline in the consumer price index the year before). Figure 2.5 provides a full histogram (one percent bins).

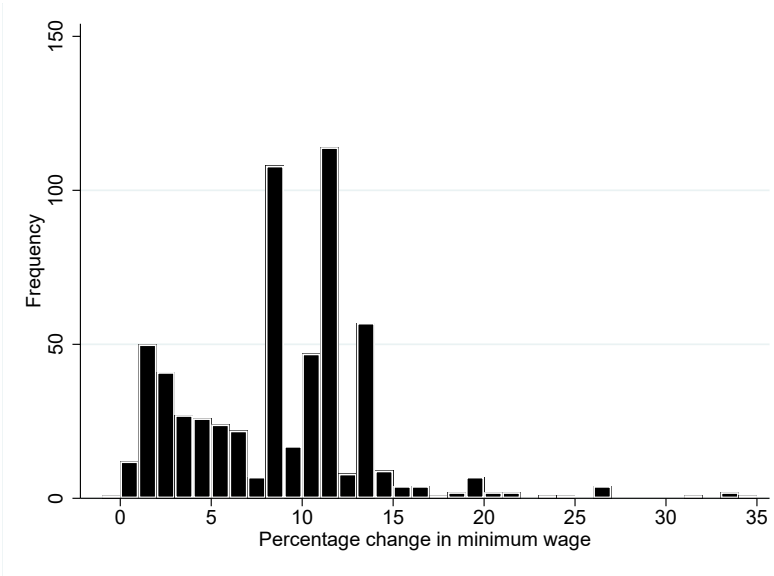


Figure 2.5: Size of state minimum wage changes between 1990 and 2017.

2.4.2 Employment and Wages

County-quarter level earnings and employment come from the Quarterly Census of Employment and Wages (QCEW). The QCEW is compiled from unemployment insurance records and covers 97% of salaried workers in the US. We use data from 1990-2018 on the food services and drinking places sector.[19,20] This sector includes full service restaurants, limited service restaurants (e.g. fastfood joints), caterers, mobile food places (e.g. food trucks), cafetarias and bars. The bulk of employment is in the first two restaurant subsectors (see Table 2.1). We focus first on the food services sector because the minimum wage is considerably more binding there than in the overall economy (see Figure 2.6).[21] The share of employees earning up to 1.1 times the minimum wage in the overall economy is relatively minor in our time

[19] NAICS 722

[20] In a strict sense, the regression sample runs from 1991q1-2017q1 because our local projection method looks three quarters in the future and past.

[21] Based on CPS-MORG data (Current Population Survey, Merged Outgoing Rotation Group).

Table 2.1: Industry composition of NAICS 722

Industry Code	Industry Name	Establish-ments	Weekly Earnings	Employment	Emp share in 722
722	Food Services	619 454	355	11 288 888	1
722511	Full Service Restaurants	240 564	395	5 261 639	0.47
722513	Limited Service Restaurants	235 267	294	4 252 800	0.38
722515	Snack Bars (ex. alcohol)	56 938	318	631 825	0.06
722310	Food Service Contractors	21 330	498	484 856	0.04
722410	Drinking places (in. alcohol)	42 390	340	375 448	0.03
722320	Caterers	12 023	431	157 011	0.01
722514	Cafetarias and Buffets	6 615	339	111 089	0.01
722330	Mobile Food Services	4 327	362	14 220	< 0.01

Situation in the first quarter of 2017. Employment is calculated as the average over the three months of the quarter (rounded to the nearest integer). Nationwide.

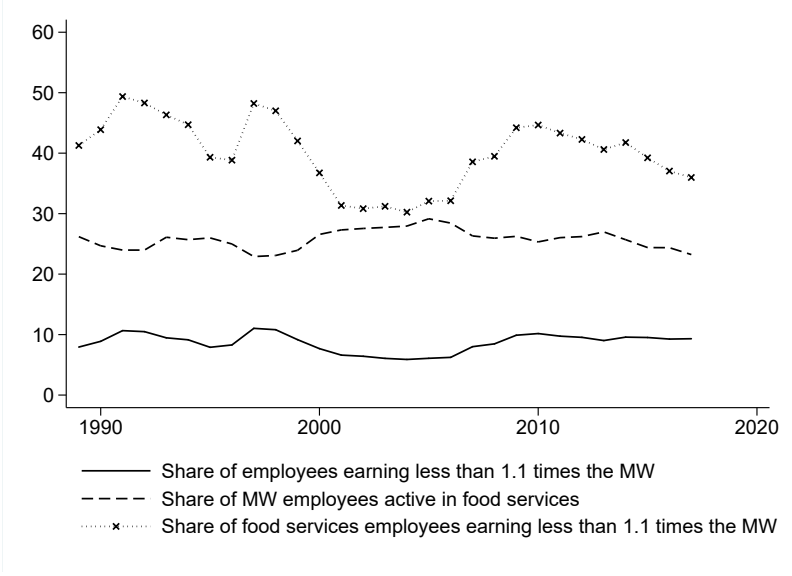


Figure 2.6: Minimum wage earner shares

period, barely exceeding 10% of workers, going up to 30-50% in the food services sector. The latter sector represents around 25% of all minimum wage workers and is thus a good starting point for any minimum wage study.[22] In Section 2.6 we show that our results also hold when we consider all plausibly affected sectors.

Table 2.2: Restaurant Worker Characteristics

	Food Services	Working Population	Overall Population
Median Age	27	42	32
Median Hours Worked	32	40	
Median Family Income	48K	78K	74K
Median Poverty Level Ratio	223	396	351
... share below 1.38	30%	12%	17%

Based on the American Community Survey 2017 1% sample.

We highlight some key characteristics of food services workers in Table 2.2 using data from the American Community Survey 2017 1% sample.[23] The median food services worker is 15 years younger than his non-food services counterpart, works fewer hours per week (32 vs 40), is part of a poorer family (48K annual income vs 78K) and is at 2.23 times the federal poverty line. 30% of food services workers belong to a family earnings less than 1.38 times the federal poverty line, the effective eligibility limit of Medicaid at the time of writing.[24] The equivalent share is only 17% in the overall population, going down to 12% amongst the working population, suggesting the food services sector is disproportionately relevant for any policy that aims to reduce poverty or inequality.

One limitation of the QCEW is that it does not provide any information on hours worked, a dimension receiving increasing attention from research based on other datasets (e.g. Jardim et al., 2017).[25] This means that our employment estimates are restricted to the extensive margin (headcounts). Nevertheless, we also calculate weekly earnings elasticities which combine wage and hours worked effects and are consistently positive, suggesting that any reductions in hours worked are more than compensated through higher hourly wages.

[22] The graph looks similar when one uses 1.25 times the MW as a threshold.

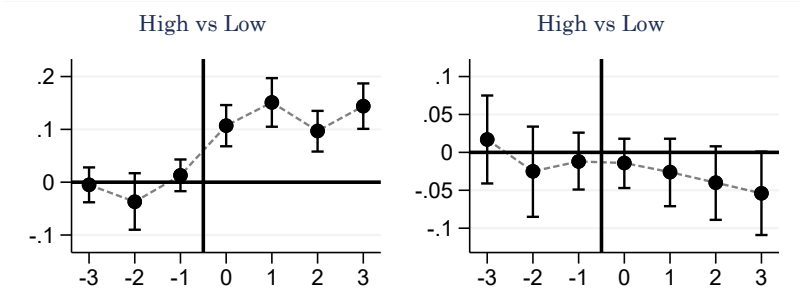
[23] Respondents were assigned to food services if their occ2010 code was between 4000-4150 which IPUMS classifies as *Food Preparation and Serving*, more information can be found at `https://web.archive.org/web/20190306143321/https://usa.ipums.org/usa-action/variables/OCC2010#description_section`.

[24] Medicaid is the social healthcare program in the US.

[25] Unfortunately, these datasets tend to be both limited in geographical scope and are not publicly available.

2.5 Results

2.5.1 High vs Low



(a) Earnings **(b)** Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. The x-axis shows time (quarters) relative to the minimum wage change. Exact numbers are available in Table B8 in the Appendix.

Figure 2.7: Elasticity with respect to the minimum wage, high vs low vulnerability.

We start by comparing high vulnerability counties (affected share > 50) to low vulnerability counties (share < 15) in Figure 2.7.[26] Panel A shows the relative elasticity of average weekly earnings to the minimum wage. In the pretreatment period (-3 to -1 on the x axis) we see no significant differences between high and low counties, suggesting that earnings dynamics in these counties were not structurally different. In the posttreatment period, counties of the high vulnerability group experience consistently larger earnings elasticities, with differences reaching +0.14 (se 0.02) three quarters after the minimum wage hike. This would imply that after a 10% minimum wage increase, weekly earnings in highly vulnerable counties increase 1.4 p.p. more than in low vulnerability counties. The most credible average earnings elasticities estimated in e.g. Wursten (2019) ranged between 0.14-0.22, indicating that these differences in elasticities represent at the very least a large portion of the total effect. Note that these positive *weekly* earnings elasticities combine the effect on hourly wages and hours worked and thus preclude large reductions in hours.[27]

The effect on employment is considerably smaller. The post period differences are small and statistically insignificant. The point estimate after three quarters is -0.05 (95% CI: [-0.11,0.00]), implying an own wage employment elasticity of around

[26] Exact numbers for all graphs can be found in Appendix 5.

[27] In fact, the weekly earnings in the QCEW are defined as quarterly earnings divided by 13. As a result, we can also exclude any major changes to the number of weeks worked per quarter.

-0.36.[28] Even the lower bound of the 95% confidence interval is small compared to the negative effects found in the literature.[29] In the next section we argue that these relative effects are likely to be close to the total effects, especially for employment.

2.5.2 Relative or total effects?

We omit the state-time dummies from the regression equation to be able to estimate earnings and employment elasticities for both groups. The price we pay is that now we are vulnerable to state-level endogeneity. We judge the credibility of this sparse model by evaluating the pretreatment trends (common in the literature) and comparing the implicit between-group differences with those we estimated before (new).

Panel A of Figure 2.8 shows the stark difference between high and low vulnerability counties. After three quarters, the earnings elasticity in the high group is more than three times larger than in the low group, at respectively +0.20 (se 0.03) and +0.06 (se 0.02). We find only very small pretreatment effects and the implicit difference between the two groups is indistinguishable from the one estimated in the saturated model (0.14 vs 0.14).

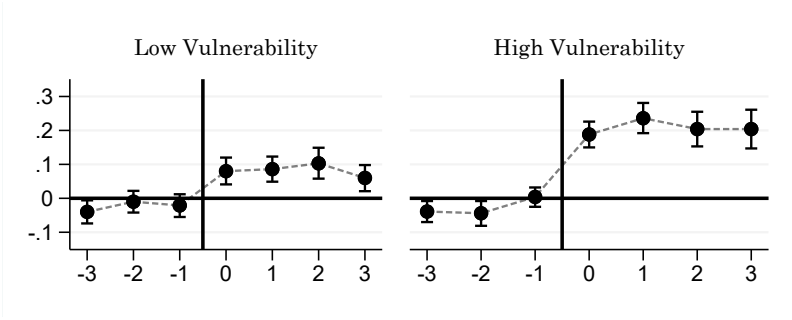
We find no significant disemployment effects in Panel B for the low vulnerability counties with a point estimate of -0.02 (CI: [-0.07, 0.02]). The discrepancy between the implicit and estimated difference is small in absolute terms, at -.03 versus -.05. The confidence interval for the high vulnerability group in this sparse model is almost identical to the CI of the relative model, at respectively [-0.10, 0.00] and [-0.11, 0.00], further suggesting that the effects estimated in the relative model are indeed total effects, especially for employment.

This would be in line with expectations based on the values of the share variable. The low vulnerability group comprises counties where the median share of affected employees was below 15%. Within that group, over half the counties have a median close to 10, the lowest value we can identify (Figure 2.9). As a result, it is not surprising that these counties do not respond strongly to minimum wage changes.[30] The own wage employment elasticities remain stable, at -0.33 and -0.25 for respectively the low and high vulnerability counties.

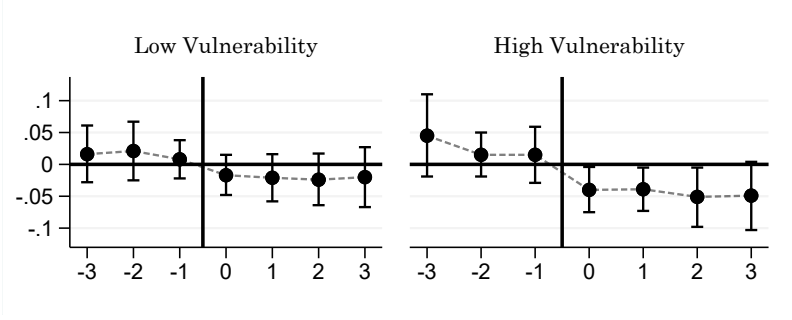
[28] Own wage employment elasticities (OWE) relate how employment responds to an increase in the average wage. They are more comparable between studies than the individual elasticities as they factor in how strongly minimum wages affect the studied group. Dube (2019) characterises OWEs less negative than -0.4 as small and any more negative than -0.8 as large. Values more negative than -1 would imply that employment losses outweigh earnings gains. When calculating the OWEs we assume that earnings and employment elasticities in the omitted counties are some fixed multiple of the relative elasticities.

[29] E.g. Brown, Gilroy and Kohen (1982) established a consensus band between -0.10 and -0.30, the review of Neumark and Wascher (2006) finds many studies predicting even more negative employment elasticities.

[30] As a caveat to this argument, do note that the shares are subject to measurement error, see Appendix 1.



(a) Earnings



(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. Time dummies not interacted by state. The coefficients show total elasticities (not relative to some other group). Exact numbers are available in Table B9 in the Appendix.

Figure 2.8: Elasticity with respect to the minimum wage, sparse model.

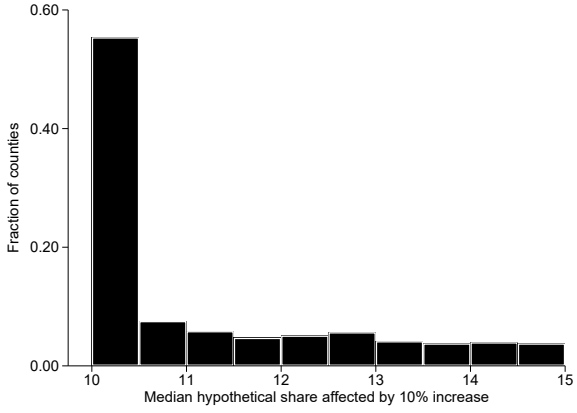
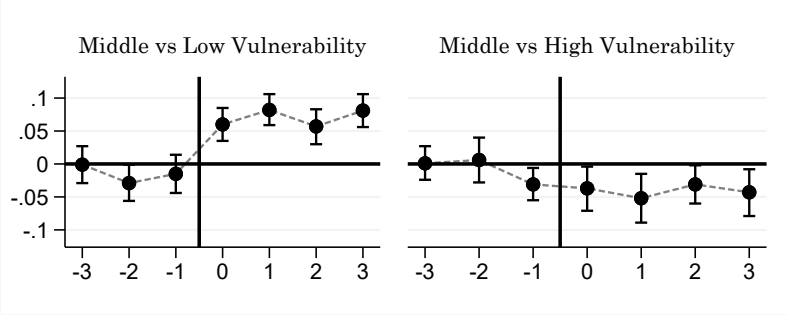


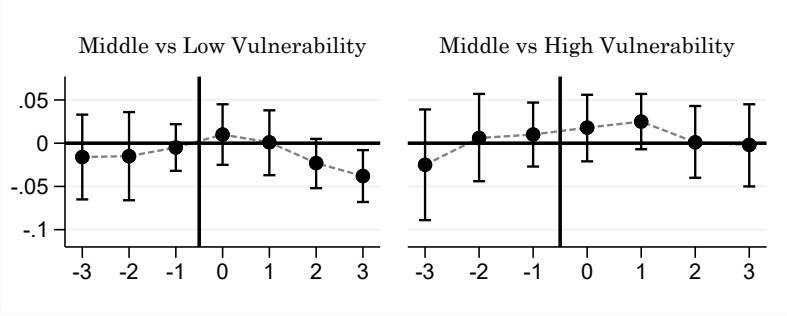
Figure 2.9: Distribution of hypothetical shares within the low vulnerability group.

2.5.3 Middle 'greyzone' group

We further validate our vulnerability measure by examing the middle 'greyzone' group (Figure 2.10). As expected, we find that earnings effects are stronger in the middle group than in the low vulnerability group (+0.08, se 0.01) but weaker than in the high vulnerability group (-0.04, se 0.02). Employment effects are larger than in the low vulnerability group (-0.04, se 0.015) but not significantly different from those in the high group (-0.00, se 0.02), again suggesting that any disemployment effects are either small or non-existent.



(a) Earnings



(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies. The coefficients show how the elasticities differ relative to the comparison group in the graph title. Exact numbers are available in Table B10 in the Appendix.

Figure 2.10: Elasticity with respect to the minimum wage, saturated model, greyzone vs the other groups.

2.6 Robustness Checks

We perform three sets of robustness checks. First, we show that our results are robust to various specification tweaks. Second, we compare the local projection method to

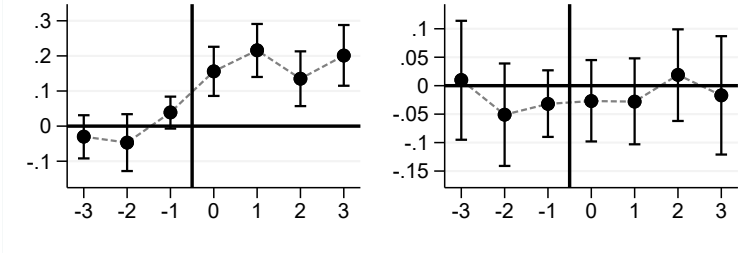
the standard models employed in the literature. Third, we illustrate that the effect is not unique to the food services sector, but also applies to other low wage sectors and teenagers.

2.6.1 Specification Tweaks

Continuous interaction. First, we switch to a continuous approach, where we interact $Share_i$, the median share affected in county i with the change in the minimum wage:

$$\Delta_h emp_{it} = \beta_{h,j}^{INT} \Delta mw_{st} \times Share_i + \delta_{1,h} \Delta_h emp_{it}^{TOT} + \delta_{2,h} \Delta_h pop_{it} + \theta_{iq} + \phi_{st} + \epsilon_{it} \tag{2.2}$$

In Figure 2.11 we show the coefficients on the interaction term $\beta_{h,j}^{INT}$ for each horizon. In line with previous results, we find an earnings elasticity that increases with our vulnerability measure. We no longer find any significant evidence for heterogeneous employment effects. In Appendix Figure B11 we plot the marginal employment effects after three quarters to get an idea of the size of the confidence interval. We find that at the mean of the high vulnerability group (70% affected), we cannot exclude employment elasticities between [-0.08, 0.06]. Note that even the lower bound is still close to zero.[31]



(a) Earnings **(b)** Employment

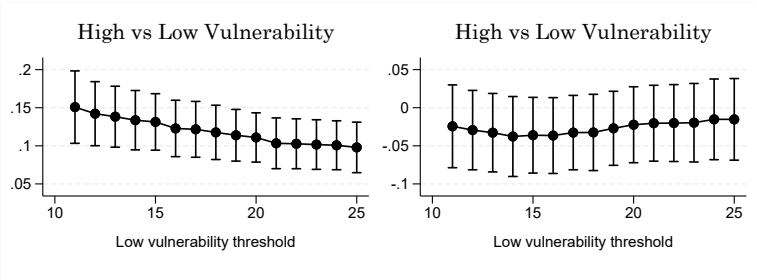
The markers show the coefficient of the interaction effect, which indicates how the minimum wage effect changes with the vulnerability measure. The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. Exact numbers are available in Table B11 in the Appendix.

Figure 2.11: Elasticity with respect to the minimum wage, saturated model, continuous interaction.

Threshold Values. In Figure 2.12 we show the difference in elasticity between counties in the high and low vulnerability groups (after three quarters) for different

[31] Implicit in this statement is that there are no base disemployment effects, cfr. Section 2.5.2.

bottom threshold values. Higher threshold values imply that increasingly affected counties enter the low vulnerability group. Reassuringly, we find that this reduces the differences between high and low vulnerability counties in terms of earnings, going from +0.15 (se 0.02) at 11% to +0.10 (se 0.02) at a 25% threshold. We do not see a similar evolution for employment, where the differences remain close to zero and insignificant at all thresholds. These results would be in line with an earnings effect that increases with vulnerability and the absence of a strong disemployment effect.



(a) Earnings **(b)** Employment

The markers show how the elasticities in high vulnerability counties differ relative to low vulnerability counties. The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies.

Figure 2.12: Elasticity with respect to the minimum wage, saturated model, bottom threshold variation, high vs low vulnerability.

In Appendix Figure B12 we show elasticities for the low vulnerability group as estimated in a model without state-time dummies, cfr. Section 2.5.2. Earnings elasticities increase as we raise the threshold, from an insignificant +0.03 (se 0.03) at 11% to +0.09 (se 0.02) at 25%. The employment elasticities on the contrary remain stable and close to zero, despite a tripling in the estimated earnings effects.

Vulnerability measure. Our results are robust to taking the mean vulnerability over time rather than the median (Appendix Figure B13). We find the expected pattern where earnings react more strongly in the greyzone than in low vulnerability counties and the difference is even larger in the highly vulnerable group. The differences in employment effects are insignificant and in the same ballpark, with CIs of [-0.05,0.01] and [-0.08,0.03] for the greyzone and high group respectively. We find similar results when we use raw rather than smoothed shares (Appendix Figure B15), exclude minimum wage change episodes when calculating the median (Appendix Figure B16) and allow group membership to fluctuate over time (Appendix Figure B14). Confidence intervals for the employment elasticities of the high vulnerability groups never include -0.1.

Functional form. In the baseline results we imposed a common relation between

food services employment and the control variables (total private sector employment and population size). This might not hold if e.g. tourism is an important source of product demand in some counties but not in others.[32] Appendix Figure B17 shows that the results remain unchanged if we allow this relation to vary freely by county. Earnings increase more in more vulnerable groups, whereas differences in employment remain either small or insignificant. In Figure B18 in the Appendix we push the data even further by interacting the controls by county *and* by season given that seasonal patterns in the food services business might be very different in the ski resorts of Eagle County, CO than in the metropolis of New York or the beaches of Miami. Such differences are not captured by national time fixed effects and do not necessarily wash out given the temporal concentration of minimum wage changes.[33] We find no changes in the results.

Next, we pool the low, middle and highly vulnerable counties into a joint regression (Equation 2.3). These regressions directly use the variation between all three groups to identify the coefficients and can be interpreted as a difference in differences analysis with different treatment levels.[34]

$$\begin{aligned}\Delta_h emp_{it} = {} & \beta_{h,2}\Delta mw_{st} \times I(g_i = 2) + \beta_{h,3}\Delta mw_{st} \times I(g_i = 3) \\ & + \delta_{1,h}\Delta_h emp_{it}^{TOT} + \delta_{2,h}\Delta_h pop_{it} \\ & + \theta_{iq} + \phi_{st} + \epsilon_{it}\end{aligned} \tag{2.3}$$

Figure 2.13 is essentially indistinguishable from the main results. Earnings effects become slightly smaller, at +0.08 (se 0.01) and +0.13 (se 0.02) which is also reflected in the employment effect CIs, respectively [-0.07,-0.01] and [-0.09,0.01].

Sample restrictions. Results are also robust to excluding the 10% least densely populated counties (Appendix Figure B19) and running separate regressions for limited and full service restaurants (NAICS 722513, Figure B20 and NAICS 722511, Figure B21). We find the expected earnings pattern in both subsectors. The small disemployment effect in the middle 'greyzone' group is slightly larger in the limited service restaurants sample (-0.06, se 0.02). In contrast, the employment elasticity in the high vulnerability counties is just -0.00 (se 0.04), likewise for full service restaurants in both vulnerability groups.

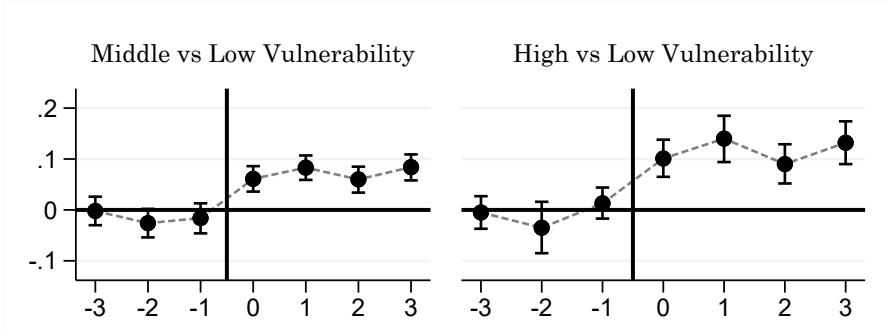
2.6.2 Alternative regression models

Differenced. In the next robustness check, we move closer the the literature by estimating a distributed lag model where we include three lags and leads of the mini-

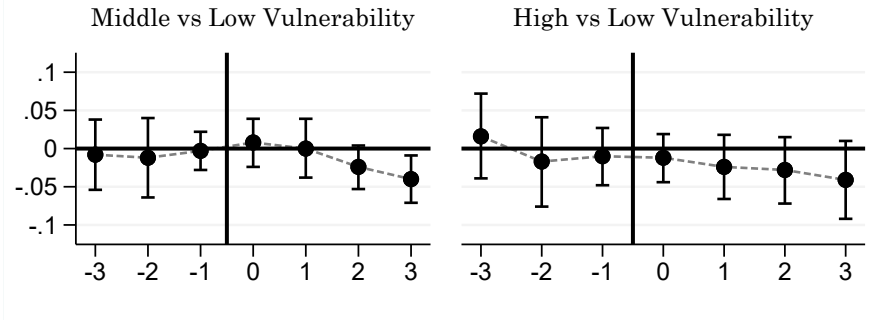
[32] For example, Las Vegas hosts twenty times more visitors than inhabitants, whereas in Los Angeles the ratio is two and in low tourism areas the ratio might approach zero (Las Vegas Convention and Visitors Authority, 2010).

[33] For example, 36% of minimum wage changes occurred in the 3rd quarter of the year (summer), whereas the last quarter only saw 7% of increases.

[34] Note that they are not ordered, i.e. we never impose that effects should be more severe in the highly vulnerable group than in the greyzone.



(a) Earnings



(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent, changes in population and group membership dummies. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies. The markers show how the elasticities differ relative to the low vulnerability group. Middle and High Vulnerability coefficients are estimated jointly. Exact numbers are available in Table B15 in the Appendix.

Figure 2.13: Elasticities with respect to the minimum wage, joint regressions.

mum wage change:

$$\Delta emp_{it} = \sum_{h=-3}^{3} \left[\beta_{h,2}\Delta_h mw_{st} \times I(g_i = 2) + \beta_{h,3}\Delta_h mw_{st} \times I(g_i = 3)\right] + \delta_1 \Delta emp_{it}^{TOT} + \delta_2 \Delta pop_{it} + controls_{it} \quad (2.4)$$

Table 2.3 shows the sums of coefficients for the pre and post period, the full regression table is available on request. Columns (3) and (6) show results for our main model (i.e. with state-time dummies) which are highly similar to our local projection estimates. Earnings increase by +0.09 (se 0.02) in the greyzone and by +0.15 (se 0.02) in the high vulnerability group. Employment decreases by -0.04 (se 0.02, CI: [-0.07, 0.01]) and -0.05 (se 0.03, CI: [-0.10, 0.01]) respectively, in line with the baseline local projection based estimates (all relative to the low vulnerability counties).

In columns (1) and (4) we show results for the differenced version of the twoway fixed effects model (Neumark and Wascher, 1992). The state fixed effects are differenced out, but we retain the time fixed effects. We find a relatively large earnings effect for the low vulnerability counties (+0.12, se 0.02) which also shifts up the greyzone and high group estimates (to +0.21, se 0.02 and +0.26, se 0.03). For employment we find significant pretrends, especially for the highly vulnerable group (+0.09, se 0.03), limiting the use of these results. In columns (2) and (5) we emulate the trends model (Allegretto, Dube and Reich, 2011; Dube, Lester and Reich, 2010) by adding state fixed effects, the differenced equivalent of state time trends. The results are essentially identical to the 2FE results, with strong earnings effects for all groups and significant pretreatment trends for employment, contrasting to the robustness of our state-time dummies model.

Levels. We also show results in levels as described by Equation 2.5 where β represents the main minimum wage effect and the γ_j coefficients measure pretreatment effects in half year intervals. The indicator function ensures that $\Delta mw_{i,t+2j}$ is nonzero only when it refers to the upcoming minimum wage. That is, if by $t + 2j$ the minimum wage has changed more than once, the difference term is set to zero.

$$emp_{it} = \beta * mw_{it} + \sum_{j=1}^{4} \gamma_j * \Delta mw_{i,t+2j} * \mathrm{I}(mw_{i,t+2j} \text{ is next MW}) + \delta_1 * e_{it}^{TOT} + \delta_2 * pop_{it} + \theta_{iq} + \mathrm{controls}_{it} + \epsilon_{it} \quad (2.5)$$

(1) 2FE $\quad \mathrm{controls}_{it} = \tau_t$

(2) Trends $\quad \mathrm{controls}_{it} = \tau_t + \psi_s * t$

(3) STD $\quad \mathrm{controls}_{it} = \phi_{st}$

In Table 2.4 we show results for the 2FE model, which includes time fixed effects as well as county by quarter of year dummies; the Trends model, which adds state time trends and finally the STD model which includes state-time dummies and only identifies relative effects. The main effect is in the *levels* rows, the other rows test for

Table 2.3: Elasticity with respect to the minimum wage - Differences

	Earnings			Employment		
	2FE	Trends	STD	2FE	Trends	STD
	(1)	(2)	(3)	(4)	(5)	(6)
Low vulnerability, pre	0.02	0.02		0.09	0.09	
	(0.03)	(0.03)		(0.05)	(0.05)	
Low vulnerability, post	0.12	0.12		-0.02	-0.02	
	(0.02)	(0.02)		(0.02)	(0.03)	
Middle vulnerability, pre	0.01	0.01	-0.01	0.07	0.07	-0.02
	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)
Middle vulnerability, post	0.21	0.20	0.09	-0.06	-0.06	-0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
High vulnerability, pre	0.04	0.04	-0.00	0.09	0.10	0.03
	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)	(0.03)
High vulnerability, post	0.26	0.26	0.15	-0.09	-0.07	-0.05
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
N	252688	252688	252461	252688	252688	252461
County by QoY Dummies	×	×	✓	×	×	✓
Time Dummies	✓	✓	×	✓	✓	×
State Dummies	✓	✓	×	✓	✓	×
State-Time Dummies	×	×	✓	×	×	✓

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for their total private sector equivalent and population levels. Included dummies are listed at the bottom of the table. QoY refers to Quarter of Year. Coefficients in Columns 1, 2, 4 and 5 show total elasticities (not relative to some other group). Columns 3 and 6 are relative to the low vulnerability group.

pretreatment discrepancies.[35] Some elements stand out. First, differences in earnings elasticities between the three groups are small in all models. The estimated differences in column (3) are just +0.04 (se 0.01) and +0.05 (se 0.01) for the greyzone and high vulnerability group respectively. Related, the 2FE and Trends models (columns 1 and 2) both estimate large earnings elasticities in the low vulnerability group, at +0.21 (se 0.03) and +0.19 (se 0.03) respectively. A potential explanation concerns the pretreatment trends, which are especially pronounced in the low vulnerability group for the 2FE and Trends models, but are also present in the other groups at varying significance levels.

The employment results in columns (4)-(6) face similar problems. The 2FE model estimates a large, albeit insignificant disemployment effect in the low vulnerability group of -0.18 (se 0.12) which increases to -0.34 (se 0.09) in the highly vulnerable group. Yet, the causality of this estimate is challenged by the strong presence of pretreatment trends, e.g. -0.26 (se 0.09) one semester ahead. The Trends model faces fewer pretreatment differences and finds a significant disemployment effect in the high group (-0.13, se 0.03). The STD model finds the same pattern, but also detects strong pretreatment differences for the highly vulnerable counties, even exceeding the main effect (-0.143, se 0.04 versus -0.138, se 0.03).

Overall, the excessive elasticity of earnings in the low vulnerability group and the abundance of pretreatment differences suggest estimations in levels are plagued by significant endogeneity concerns, in contrast to the local projection and differenced regressions of Figure 2.7 and Table 2.3.

2.6.3 Different samples

In this section, we consider minimum wage effects in different samples: a) demographic subgroups within the food services sector, b) teenagers in the entire economy and c) all workers in plausibly affected sectors.

Food services subgroups. The Quarterly Workforce Indicator dataset (QWI) allows us to estimate minimum wage effects by gender, race, age and education. We start by discussing earnings and employment effects for each subgroup within the food services sector (NAICS 722). The QWI panel is shorter than the QCEW as not all states joined the QWI program from the start.[36]

At 13 subgroups × 5 dependent variables (earnings and employment plus hiring, separation and turnover rates), the QWI produces too many results to discuss individually. Instead, we summarise the main take-aways here and refer to Appendix 3 for summary statistics, background information and tables containing all results at horizons -3, 0 and +3 [Tables (B4)-(B6)].

We find very similar results when we consider all workers (in food services). Relative earnings elasticities after three quarters are respectively +0.08 (se 0.02) and

[35] Due to the indicator function in Equation 2.5, we can interpret these coefficients as the systematic differences in earnings and employment X semesters before a minimum wage change, where X ∈ {1,2,3}.

[36] In 1990, only three states participated in the QWI program, steadily rising to 18 in 1995, 42 by 2000 and 50 from 2005 onward.

Table 2.4: Elasticity with respect to the minimum wage - Levels

	Earnings			Employment		
	2FE	Trends	STD	2FE	Trends	STD
	(1)	(2)	(3)	(4)	(5)	(6)
Low vulnerability, 3 semesters ahead	0.064	0.051		-0.057	0.065	
	(0.021)	(0.018)		(0.070)	(0.032)	
Low vulnerability, 2 semesters ahead	0.100	0.087		-0.074	0.058	
	(0.024)	(0.022)		(0.069)	(0.033)	
Low vulnerability, 1 semesters ahead	0.090	0.073		-0.116	0.066	
	(0.026)	(0.026)		(0.086)	(0.046)	
Low vulnerability, levels	0.206	0.191		-0.182	0.012	
	(0.031)	(0.026)		(0.115)	(0.037)	
Middle vulnerability, 3 semesters ahead	0.037	0.027	-0.028	-0.067	0.064	-0.000
	(0.019)	(0.018)	(0.013)	(0.061)	(0.031)	(0.033)
Middle vulnerability, 2 semesters ahead	0.058	0.048	-0.036	-0.081	0.060	0.005
	(0.023)	(0.021)	(0.018)	(0.063)	(0.028)	(0.023)
Middle vulnerability, 1 semesters ahead	0.048	0.035	-0.036	-0.145	0.051	-0.020
	(0.023)	(0.024)	(0.018)	(0.079)	(0.034)	(0.024)
Middle vulnerability, levels	0.243	0.232	0.037	-0.212	-0.019	-0.030
	(0.029)	(0.025)	(0.010)	(0.103)	(0.032)	(0.023)
High vulnerability, 3 semesters ahead	0.015	0.002	-0.047	-0.149	-0.010	-0.082
	(0.023)	(0.020)	(0.022)	(0.073)	(0.028)	(0.033)
High vulnerability, 2 semesters ahead	-0.002	-0.015	-0.092	-0.165	-0.015	-0.070
	(0.024)	(0.023)	(0.021)	(0.074)	(0.026)	(0.035)
High vulnerability, 1 semesters ahead	0.046	0.027	-0.038	-0.259	-0.050	-0.143
	(0.020)	(0.023)	(0.021)	(0.091)	(0.037)	(0.038)
High vulnerability, levels	0.272	0.248	0.048	-0.345	-0.126	-0.138
	(0.027)	(0.023)	(0.011)	(0.092)	(0.030)	(0.033)
N	268813	268813	268784	268813	268813	268784
County by QoY Dummies	✓	✓	✓	✓	✓	✓
Time Dummies	✓	✓	×	✓	✓	×
State-Time Dummies	×	×	✓	×	×	✓
State Trends	×	✓	×	×	✓	×

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for their total private sector equivalent and population levels. Included dummies are listed at the bottom of the table. QoY refers to Quarter of Year. Coefficients in Columns 1, 2, 4 and 5 show total elasticities (not relative to some other group). Columns 3 and 6 are relative to the low vulnerability group.

+0.12 (se 0.03) for middle and highly vulnerable counties, the respective relative employment elasticities are -0.01 (se 0.02, CI: [-0.05, 0.05]) and -0.01 (se 0.03, CI: [-0.07, 0.05]) (Figure B3 in the Appendix). Note that the 95% confidence intervals exclude large (< −0.1) disemployment effects and that the own wage employment elasticities are very small at respectively -0.13 and -0.08. We find that separation rates decline contemporaneously in the highly vulnerable group (elasticity -0.15, se 0.04), whereas hiring rates remain stable (-0.02, se 0.06, Figure B4).[37]

Unlike Dube, Lester and Reich (2016), we do find some results that are consistent with labor-labor substitution (Neumark and Wascher, 2007) in Figure B5. The employment elasticity for 14-18 year old workers is significant in some quarters, e.g. -0.10 (se 0.05) after one quarter[38] and the confidence intervals cannot rule out meaningful negative effects (e.g. [-0.14, 0.07] after three quarters). 19-21 year olds on the other hand see a significantly positive employment response of a similar magnitude (+0.12, se 0.05 after three quarters), which would be consistent with firms upgrading their workforce from cheap teenagers to young adults that might turn equally dear after the minimum wage change. This is potentially problematic for two reasons. First, this might bias studies that compare workers in the same area with different initial earnings (Clemens and Wither, 2019; Dustmann et al., 2019) as it implies minimum wages might have opposite employment effects on workers above and below the wage threshold. Second, it implies that the small to zero disemployment effects found in studies based on industry data could mask strong disemployment effects in subgroups within those industries. Nevertheless, when we calculate own wage employment elasticities, we find that these remain close to zero, even for 14-18 year olds in the food services sector (e.g. -0.18 after three quarters), suggesting that even this potentially most vulnerable subgroup still largely benefits from higher minimum wages.[39]

We find a very tight null effect after three quarters on employment for adults aged 22-64 working in the food services sector (middle CI: [-0.03, 0.04], high CI: [-0.06, 0.06]). Likewise for food services workers with a college degree (Figure B7, middle CI: [-0.05, 0.04], high CI: [-0.07, 0.08]). We do find positive earnings effects for those workers, but the 95% confidence interval approaches zero in the high vulnerability counties and includes zero in some quarters for the middle group (Figure B6). Workers with less than a high school degree and those with only a high school degree both see robust increases in earnings (resp +0.08, se 0.03 and +0.09, se 0.03 in the high vulnerability counties after three quarters) without significant decreases in employment, although the confidence intervals do approach -0.1 in the highly vulnerable counties ([-0.08, 0.07] and [-0.10, 0.04] respectively).[40]

[37] These results are in line with the average results in Dube, Lester and Reich (2016).

[38] This does not hold when we exclude heavily distorted observations, but the CIs remain wide enough that we cannot exclude economically meaningful negative effects. The distortion is the result of the QWI confidentiality requirements.

[39] An own wage employment elasticity of 0 would mean employment of this group does not react to the (minimum wage induced) wage increase. If the elasticity exceeds -1 (negatively), any wage gains are fully lost through reduced employment.

[40] Note that the estimates by split do not need to average to the overall mean effect, as the split regressions

We do not detect large differences by race (Figure B8-B9). Earnings effects in the high group after three quarters are larger for those who identify as black, but the difference with whites is not significant (0.16 vs 0.10, p-value of difference: 0.28). The point estimates for the relative employment elasticities are small even in the highly vulnerable counties (white: 0.01, se 0.03; black: 0.01, se 0.08). The CI is tight for whites [-0.06, 0.08], but wide for blacks, where it doesn't exclude -0.1 [-0.16, 0.17].[41]

Overall, the QWI robustness checks indicate that our vulnerability measure picks up meaningful differences between counties (earnings elasticities are positive and larger for the high vulnerability group), that overall employment effects in the food services sector are small to zero (tight confidence intervals), but that there might be some substitution from teenagers (14-18) to presumably more productive young adults (19-21).

Teenagers. The QWI also allows us to look at teenagers across all sectors, as per Equation 2.6.[42] The dependent variable is then employment (or earnings) for all teenagers (14-18 years old) and we control for business cycle fluctuations through employment of all adults (ages 22-64).

$$\Delta_h emp_{it}^{TOT,14-18} = \beta_{h,j}\Delta mw_{st} \times I(g_i = j) + \delta_{1,h}\Delta_h emp_{it}^{TOT,22-64} + \delta_{2,h}\Delta_h pop_{it} + \theta_{iq} + \phi_{st} + \epsilon_{it} \quad (2.6)$$

Results are qualitatively identical to those from the food services sector, although with larger standard errors (Figure 2.14). We find positive earnings elasticities that are higher for the highly vulnerable counties after three quarters [middle: +0.08 (se 0.04), high: +0.13 (se 0.03)]. The employment elasticities are close to zero and insignificant, with relatively tight CIs (middle: [-.06, .05], high: [-.05, .09]). This corresponds to relative own wage employment elasticities of respectively -0.00 and +0.15. In Appendix Figure B10 we show that earnings barely respond for young adults aged 19-21 and that employment effects are positive but insignificant.

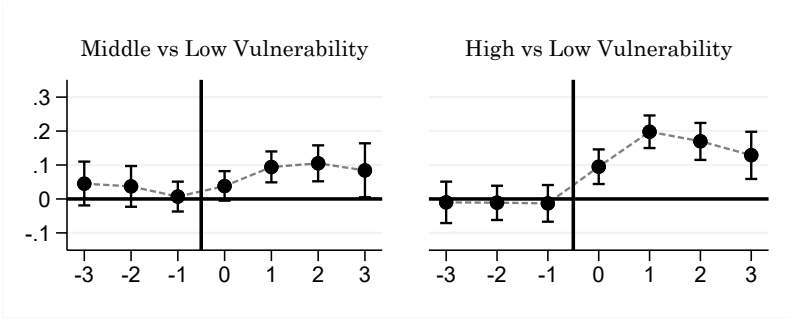
Low wage sectors. Finally, we return to the QCEW but apply our methodology to different sectors. We identify the 50 sectors (3 or 4 digit NAICS) with the lowest average weekly pay in 1990, 2000, 2010 and 2017 and retain those that appear in all cross-sections. After removing duplicates and sectors with fewer than 10 000 observations per regression, we are left with 18 potentially affected industries.[43] Summary statistics on these industries can be found in Appendix Table B16, they include day care services, various stores and personal services. We retained the county vulnerability classification based on food services, thus assuming that in terms of wage levels

directly compare teens to teens, young adults to young adults etc. The overall regressions do not factor in that the shares of these population groups might differ between counties.

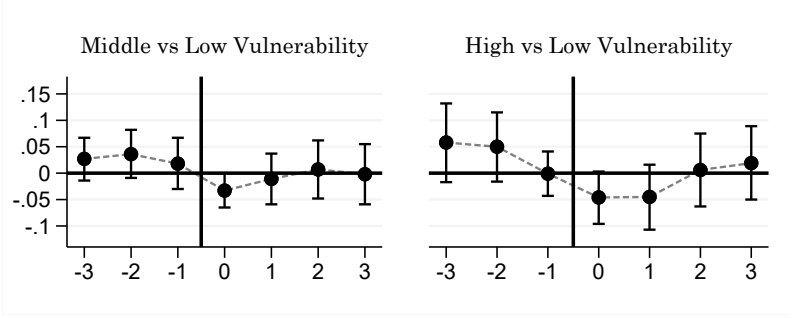
[41] The CI for blacks is tighter when you exclude the distorted and pre-2000 observations ([-0.1, 0.18]).

[42] We cannot apply our method to the more commonly used Current Population Survey data, as it has only very limited county information. See Godoy and Reich (2019) for a related exercise using a mix between counties and public use microdata areas.

[43] The duplicates arise because we allow both 3 and 4 digit NAICS industries. There is considerable size variation between industries of the same aggregation level. We retained the 4-digit sector if it was sufficiently large or covered the entire 3-digit sector. We switch to the 3-digit sector if the 4-digit sector is too small and earnings levels in 1990 were within 5 dollars difference.



(a) Earnings

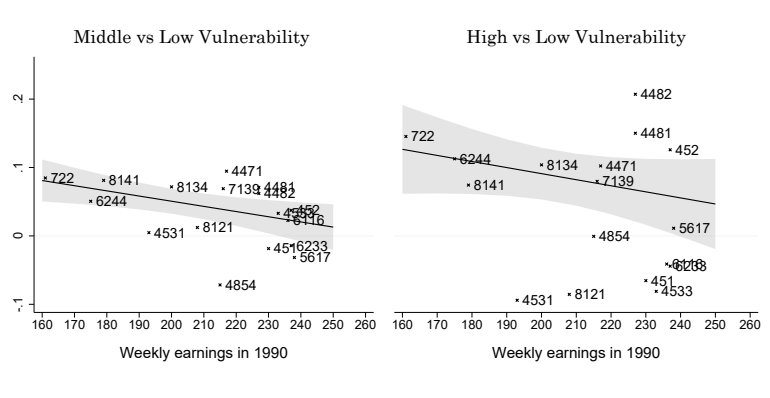


(b) Employment

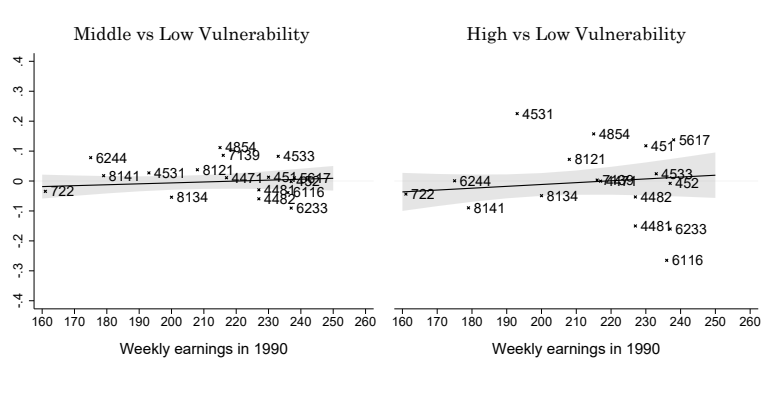
The handles show 95% confidence intervals, errors are clustered at the state level. These regressions include controls for changes in adult employment and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

Figure 2.14: Earnings and Employment elasticities with respect to the minimum wage, QWI dataset, teenagers (14-18) from all industries.

it is representative for the other low-wage sectors.[44] We then estimated our local projection model for each sector, separately for the greyzone and high vulnerability counties.



(a) Earnings



(b) Employment

Breakdown of the coefficients underlying Figure 2.16. Each cross represents a sector-level estimate, the black line the WLS prediction of minimum wage elasticities controlling for weekly earnings in 1990. Exact numbers are available on request.

Figure 2.15: Elasticities with respect to the minimum wage, multiple sectors, industry-specific estimates after three quarters.

Figure 2.15 shows the results after three quarters. Each cross represents a sector estimate, the line represents the prediction from a weighted least square (WLS) regression where we include average weekly earnings in 1990 as extra coefficient and use the inverse variance of each estimate as weight. All else equal, we would expect minimum wage effects to be smaller in sector with higher average earnings.

[44] It is not trivial to re-estimate the vulnerability per sector, as some of the intermediate data (e.g. hours worked per quarter) are only available for large sectors.

The shaded area represents the 95% confidence interval. The left-most cross shows results for the food services sector (722). We see a clear downward trend in the response of earnings to the minimum wage as we move to sectors with higher initial earnings. The elasticities become insignificant for higher earning sectors, suggesting we included the most relevant sectors in our analysis. On average, the earnings estimates for the highly vulnerable counties are significantly higher than for the middle 'greyzone' group and this also holds for most sectors individually. This indicates the food services based vulnerability measure is also appropriate in the other low wage sectors. The employment effects in panel B are insignificant at each point in the initial earnings distribution and the food services sector estimates do not appear to be outliers.

In Figure 2.16 we plot the constant from the weighted least square regression for each horizon.[45] We see the expected pattern for the relative earnings elasticities (middle: +0.05, se 0.01; high: +0.09, se 0.02). The relative employment elasticities are even smaller than when we only considered food services and the corresponding confidence intervals are tighter (middle: [-0.03, 0.02], high: [-0.05, 0.03]), further bolstering the point that disemployment effects are either absent or small.[46]

2.7 Policy Simulations

In this section, we simulate four policy scenarios: the national effects of a 10% increase in all minimum wages on food services employment, the effect of a $15 minimum wage, the impact of a $15 minimum wage where we adjust county vulnerability and finally a fourth scenario where we repeat the previous simulation for all low-wage sectors. The first two concretize our point estimates, the last two illustrate how strongly the effects of a large minimum wage shock are underestimated if vulnerability is kept fixed.

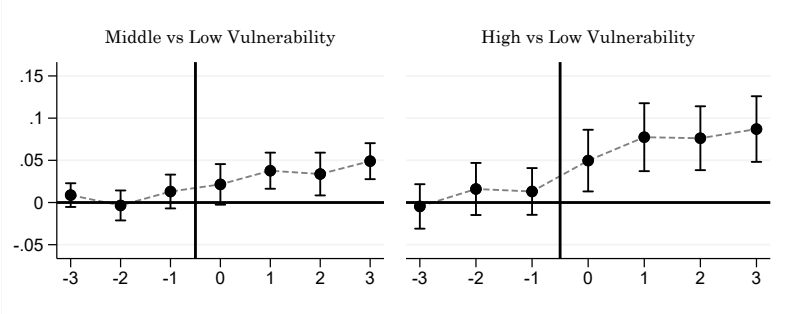
In Table 2.5 we show results under the assumption that the low vulnerability counties are not affected. We also show results based on the model without state-time dummies in Appendix Table B7. As these increase earnings more than they decrease employment, we consider the table presented below as a conservative prediction of minimum wage effects.

In Scenario A, we simulate the effect of a 10% minimum wage increase on county-level average employment and earnings in 2017 and aggregate to the national level.[47] We find a reduction in food services employment of 14621 jobs, equivalent to 0.1% of baseline employment. The numbers in brackets show calculations based on the lower and upper limits of the 95% CIs of the original regressions and illustrate that the uncertainty around this estimate is fairly narrow. Average weekly earnings would increase by $0.9, a 0.2% increase. These effects are small because the low vulnerability counties represent 68% of all employment and were assigned zero effect.

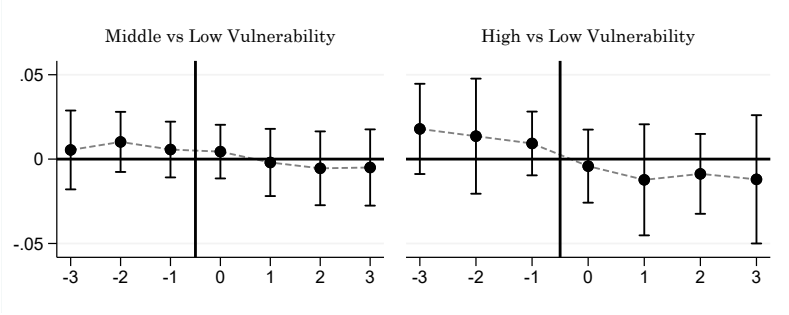
[45] We exclude initial earnings as we are interested in the raw elasticities.

[46] Own wage employment elasticities for both groups are -0.1.

[47] The mean for earnings is weighted by employment in 2017.



(a) Earnings



(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent, changes in population and group membership dummies. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Mean and confidence intervals over sectors were calculated using a weighted least squares estimation using the inverse variance as weight. Exact numbers are available in Table B17 in the Appendix.

Figure 2.16: Elasticities with respect to the minimum wage, multiple sectors.

Table 2.5: Policy Simulations

	Δ Employment (#)	Δ Employment (%)	Δ Earnings ($)	Δ Earnings (%)
Food Services				
Scenario A	-14 621	-0.1	0.9	0.2
10%, fixed	[-26 800, -2 396]	[-0.2, 0.0]	[0.6, 1.1]	[0.2, 0.3]
Scenario B	-83 799	-0.7	5.1	1.4
15$, fixed	[-152 128, -13 941]	[-1.3, -0.1]	[3.5, 6.7]	[1.0, 1.8]
Scenario C	-335 291	-2.9	28.6	7.9
15$, adjusted	[-664 520, 5 318]	[-5.8, 0.0]	[19.8, 37.7]	[5.4, 10.4]
All Low-Wage Sectors				
Scenario D	-354 945	-1.7	22.1	5.5
15$, adjusted	[-1 603 740, 1 023 286]	[-7.5, 4.8]	[4.2, 41.3]	[1.0, 10.2]
Food Services Employment 2017: 11.4M		Low Wage Sector Employment 2017: 21.3M		
Food Services Weekly Earnings 2017: $364		Low Wage Sector Weekly Earnings 2017: $404		

Main results based on the coefficient estimates of the baseline model (Figure 2.7 and 2.10). Results between brackets are recalculations based on the lower and upper limits of those coefficients' 95% confidence interval. Starting values are averages over 2017. Sample contains 97.5% of all employment in the food services sector. Earnings results are weighted by employment in 2017. Scenario A: 10% increase in the minimum wage, keeping county vulnerability fixed. Scenario B: All minimum wages are raised to $15, county vulnerability remains fixed. Scenario C: All minimum wages are raised to $15, but now county vulnerability is recalculated based on an increase to $15. Scenario D: Same as C, but for all low wage sectors considered in Section 2.6.3.

Scenario B simulates the effect of a $15 federal minimum wage (56% increase on average).[48] The impact on employment and earnings is also about six times larger, but still fairly limited at -0.7% for employment and +1.4% for earnings. This changes considerably in Scenario C, where we adjust county vulnerability by calculating the share that would be affected by a $15 minimum wage in 2017. The effects are over four times larger, with employment losses of -2.9% and earnings increasing 7.9%, corresponding to over 335 thousand lost jobs, but also almost $29 of extra earnings per week per worker. Under worst case assumptions, employment losses and earnings gains are roughly in balance, with lower bounds of -5.8% and +5.4%. Under best case assumptions, employment remains unaffected (upper bound of 0.0) and earnings surge by 10.4%.

The mean predictions for all low wage sectors in Scenario D are smaller, at -1.7% and +5.5% for employment and earnings respectively (see Appendix Table B16 for a list of sectors). This does hide large uncertainty around the coefficient estimates as reflected in the confidence intervals, at [-7.5,4.8] and [1.0,10.2].

These simulations highlight the importance of non-linearities in minimum wage impacts. Accounting for adjustments in vulnerability status makes up 80% of the total effect, which would be missed entirely if one extrapolated traditional estimates. Nevertheless, estimating other sources of non-linearity remains an important avenue for future research. Substantially higher minimum wages may affect different population groups, more tradeable sectors (e.g. Harasztosi and Lindner, 2019) and have more complicated general equilibrium effects.

2.8 Conclusion

In this paper we exploit variation in wage distributions across counties to identify the earnings and employment effects of the minimum wage. We find that minimum wages increase earnings more in counties where they are expected to affect more employees' wages directly. Employment effects are small and not consistently significant. We find earnings elasticities are 0.14 larger in highly vulnerable counties with expected affected shares above 50% compared to counties where that share is below 15%. Employment elasticities in these vulnerable counties are more negative by -0.05. The 95% confidence intervals only rarely include elasticity differences below -0.10 and almost always include zero (i.e. the effect is not significant at 5%). We show that it is plausible that the low vulnerability counties are not or barely affected by minimum wage changes. When we consider multiple low wage sectors or teenagers, we still find robust evidence for increasing earnings effects whereas the employment effects are insignificant, with tight confidence intervals.

We validate our vulnerability measure by showing that employment and earnings effects in the greyzone (between 15-50% affected) are in between those of the high and low vulnerability groups. Moreover, the difference between high and low counties becomes smaller as we include increasingly heavily affected counties into

[48] For San Francisco, CA and King County, WA, the increase is smaller than in the 10% scenario.

the low vulnerability group. The results are robust to alternative ways of defining vulnerability and a host of specification tweaks.

We conclude by simulating the effect of a 10% increase in the minimum wage. We find that in the food services sector this increases average earnings by 0.2%, compared to an employment loss of -0.1%. This is a conservative estimate as these simulations assume that the low vulnerability counties do not respond at all to the policy shock. They become more favourable to the minimum wage if we drop that assumption. The impact of a $15 minimum wage depends heavily on whether you adjust the vulnerability measure to this large shock, with earnings gains of 1.4% if you do not and 7.9% if you do, illustrating the importance of non-linearities in minimum wage effects, one aspect of which we capture in this paper.[49]

Overall, the estimations in this paper suggest that the positive earnings effects of minimum wage policy exceed any estimated negative impacts on employment, which are small and generally not statistically significant.

[49] The employment loss predictions are -0.7 and -2.9 respectively.

Chapter 3

Limits to the external validity of minimum wage research

The internal validity of minimum wage research is hotly debated. Its external validity has received considerably less attention. We provide a general framework to think about external validity and identify the implicit assumptions required to extrapolate historical minimum wage variation to the effects of a large minimum wage change. We find decent historic coverage in terms of industry, occupation and income quartiles. Coverage is limited in particular for interactions with expected pay raises and job tenure.

3.1 Introduction

The effects of minimum wage policy have been extensively studied, resulting in over 800 scholarly articles (Wolfson and Belman, 2019). The internal validity of those studies has been the source of debate for several decades, but less attention has been devoted to the gap between historical minimum wage variation and the type of policies favoured by its proponents. On average minimum wages increased by 8.7% in the past three decades (Wursten, 2020), whereas the 'Raise The Wage Act' (HR 582) proposed by the 116^{th} Congress would double the federal minimum wage from \$7.25 to \$15. In this paper, we study the external validity of minimum wage research by analysing whether past research is sufficiently broad to predict the effect of such large increases.

We start by gathering facts about those affected by past policies using the Current Population Survey 1990-2017 (CPS) to gain insights into the demographics of this sample, including their age, education level, role within their household and job tenure. We show how these characteristics varied over time, over study types (e.g. teenager based versus overall low wage worker studies) and how feasible it is to estimate minimum wage effects by subgroups.

Then we calculate how those characteristics would change when we impose increasingly high minimum wages at the end of the sample period (up to $15). We find that this hypothetical group of affected workers differs in terms of age, tenure, household role, education level, geographic location and the size of the required pay increase. It is remarkably similar in terms of industry, occupation and position within the income distribution. Moreover, we show that some subgroups affected by high minimum wages have never truly been affected historically, implying that at least some extrapolation is required to predict the impact of a large minimum wage change. These subgroups represent 50% of the population affected by a $15 minimum wage if we define them in terms of occupation-education-required raise cells. If the minimum wage only reaches $15 in 2023 (2030) then this share does drop to 30% (8%).

We conclude by sketching a roadmap for future minimum wage research that could reduce the uncertainty around the effects of a large minimum wage change. This combines empirical and theoretical work. First, there is scope to estimate minimum wage effects for finer subgroups. This is crucial because average effects for broad groups (e.g. teenagers, restaurant workers or all low wage workers) are only partially representative for the population affected by a large increase and could mask strong negative and positive effects within the studied group.[1]

From the theoretical side, we propose a reversed modelling approach, that starts from the wealth of empirical results and imposes structure around it to generalise those findings to large nationwide minimum wage changes. It could start from 'known' effects for particular subgroups (e.g. in terms of age and occupation) and then model how those change when the required pay increase reaches unprecedented heights (see Heckman and Vytlacil, 2005 for an extensive discussion of this type of model). This contrasts to the standard approach, that builds a structural model first and then calibrates its primitives to the data, often bypassing the entire reduced form literature on earnings and employment effects. Somewhat poetically, reducing the gap between the two literatures might also shorten the leap of faith required when changing minimum wage policy.

This paper complements several strands of literature. First, we provide context to existing minimum wage studies (e.g. Cengiz et al., 2019; Meer and West, 2016; Dube, Lester and Reich, 2010; Wursten, 2019; Clemens and Wither, 2019; Allegretto, Dube and Reich, 2011; Neumark, Salas and Wascher, 2014*b*) by documenting in detail who has been affected by past minimum wage increases and how that translates into future policy changes.

Second, our work complements earlier efforts to identify target populations. E.g. Cooper (2017) describes characteristics of the $15 population, but does not relate those to historic variation and Dube (2019) performs a similar exercise for a higher minimum wage in the UK. Closest to our paper are Reich (2019) and Reich, Allegretto and Montialoux (2019), which try to predict the effects of specific policy proposals in

[1] One example that is currently studied is the possibility of labor-labor substitution, where higher wages push out low skilled workers in favour of higher skilled alternatives. See e.g. Neumark and Wascher (2007); Dube, Lester and Reich (2016); Wursten (2020).

the US. The first focuses more on the empirical side and the second on the theoretical side, whereas the goal of this paper is to locate the edge between the two.

Finally, this paper relates to the literature discussing external validity in economics (Heckman and Vytlacil, 2005; Gechter, 2015; Andrews and Oster, 2017) and randomised control trials (Stuart et al., 2011; Glasgow et al., 2006; Bell and Stuart, 2016). We put forward several complementary approaches to investigate the differences in observable characteristics between the studied sample and the target population.

The rest of this paper is structured as follows. We start by restricting our concept of external validity in Section 3.2. Section 3.3 describes the data and general methodology. Next, we show graphically where the past and future overlap in Sections 3.4-3.7. We discuss what this implies for external validity and sketch a way forward in Section 3.8. Section 3.9 concludes.

3.2 External validity

Imagine a policymaker who wants to know whether he should implement a particular policy and tries to predict its impact through the existing body of literature. In the spirit of Imai, King and Stuart (2008) and Stuart et al. (2011) we could decompose their prediction error as

$$\Delta^e = \left(\mathbf{\Delta_{S_X}} + \Delta_{S_U}\right) + \left(\Delta_{T_X} + \Delta_{T_U}\right) \tag{3.1}$$

where S refers to bias due to differences between the historic sample and the population targeted by the proposal. T refers to study-specific bias. Their subscripts refer to the portions due to observable (X) and unobservable (U) components. In the context of economics, the four terms can be interpreted as the bias arising from a) observable differences between sample and target population [Δ_{S_X}], b) unobservable differences, which could include changing general equilibrium effects, [Δ_{S_U}], c) imperfect study design [Δ_{T_X}] and d) endogeneity bias [Δ_{T_U}].

We largely abstract from the last three terms and examine whether the bias originating from observable differences between sample and population can be brought to zero. We distinguish between Δ^e, the estimation error for one particular estimate e, and

$$\Delta^{AGG} = f(\Delta^1, \Delta^2, ..., \Delta^n), \tag{3.2}$$

the estimation error left after aggregating the results from several estimates. In this paper, we evaluate several aggregation strategies in the context of introducing a \$10-\$15 minimum wage, summarised in Table 3.1.

As a first strategy, the policymaker could average over all empirical studies she considers relevant and apply that to the entire population (cfr. meta-analysis). We show in Sections 3.4 and 3.5 that there are considerable differences in terms of observable characteristics between the historical sample and the target population, suggesting the bias arising from the Δ_{S_X} term could be considerable. E.g. the average treated worker in the past was younger, had lower tenure and was less educated

Strategy	Evaluation
Averaging over empirical studies	Historically treated sample has different characteristics than target population
Analysis by subgroup	For certain subgroups it is not feasible to predict their effects because they have not been affected in the past
Theory only	Theoretical models have ambiguous predictions
Theory-empirics hybrid	Feasible, but certain construction blocks are missing (both empirically and theoretically)

Table 3.1: Aggregation strategies considered in this paper.

than the target population which could alter how those workers and their employers respond to minimum wage changers.

Second, the policymaker could split the working population into subgroups, obtain estimates per subgroup and aggregate up using the weight of each subgroup in the target population. An example is the Congressional Budget Office (CBO) report on implementing a $15 minimum wage by 2025 (CBO, 2019). They split the population in teenagers and adults, derive a median effect for each group and then aggregate up using their share in the target population.[2] Whether the external validity bias term Δ_{S_X} is smaller in this setup depends on the granularity of the subgroups. E.g. the crude teenager/adult split in the CBO report does not resolve the differences in age, tenure and education we raised above. However, creating finer splits is not necessarily a solution as it might not be feasible to obtain estimates for each cell. We illustrate this in Section 3.6 by counting the number of observations in the CPS. We find that even if we require just 100 treated observations between 1990-2017, we only cover 23% of the $15 population if we split by occupation, tenure and required raise size.[3] Note that the policymaker might also care about the impact on these specific subgroups (e.g. lowly educated young adults).

Third, she could choose to disregard all empirical evidence and focus entirely on theoretical labour market models. In Section 3.8 we briefly illustrate that there is considerable disagreement between and even within models, suggesting this is not a fruitful approach.

The fourth option we consider is a hybrid between theory and empirics, which starts from the subgroups for which we can get estimates and then extrapolates those to approximate the impact on the subgroups for which we do not. We show in Section 3.6 that the main bottlenecks are job tenure and the required pay increase. This extrapolation could be ad-hoc, or inspired by economic theory. For example, one

[2] Somewhat surprisingly, the studies they use to generate the adult-specific effect are at least partially identified through the effects on teenagers.

[3] The subgroups are weighted by their share in the target population.

would expect that any employment losses are lower for workers with more tenure, but increase as the required raise becomes larger.

3.3 Data, methodology and context

Our main data source is the 1990-2017 Current Population Survey (CPS), accessed through IPUMS (Flood et al., 2020). We combine data from the basic monthly files with the Job Tenure, Food Security and Annual Social and Economic supplements through the IDs generated by IPUMS. This gives us demographic information on each worker, as well as their earnings when they are in the outgoing rotation group (months 4 and 16). We exclude imputed data as identified by the IPUMS data quality flags and calculate hourly earnings as weekly earnings divided by the usual hours worked per week for those who do not report hourly wages. We exclude observations where hourly wages are less than 75% of the prevailing minimum wage.

We observe 460 minimum wage changes (counted at the state level), of which 115 are due to federal minimum wage changes, 345 are state minimum wage changes and 134 of those are the result of indexation at the state level.[4] Figure 3.1 shows these state level minimum wages over time. The lowest line is the federal minimum wage, which increased seven times during our timeframe. The other lines represent the states which went beyond the federally mandated minimum.

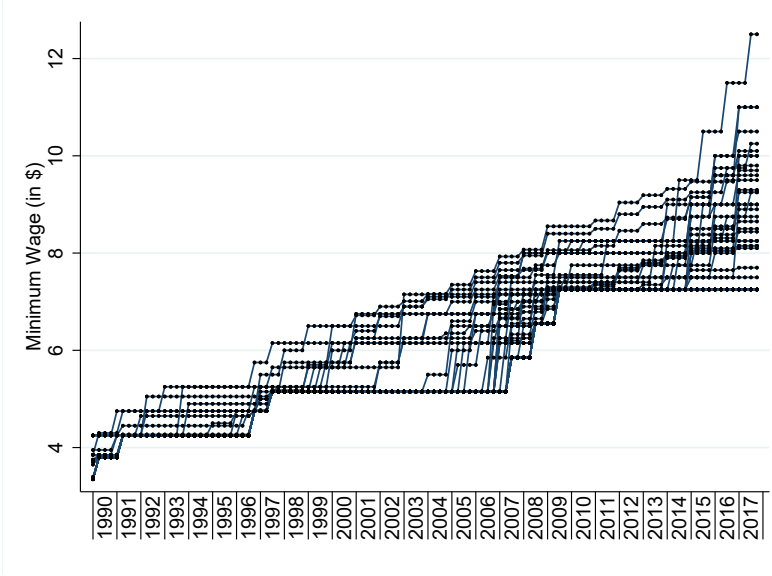


Figure 3.1: Evolution of state minimum wages between 1990 and 2017. Copied from Wursten (2020).

[4]Structured, regression-ready data on state level minimum wages were obtained from the personal website of David Neumark (Neumark, Salas and Wascher, 2014*b*).[5]

We define workers as affected if their wage is below the minimum wage six months in the future. In 2017, the last year of our data, we additionally calculate who earns less than $10-15 per hour. We define those workers as affected by a large minimum wage change and show separate results for the hypothetical $10, $11, $12, $13, $14 or $15 minimum wage. Which value is more relevant depends on the policy being evaluated. The 'Raise The Wage Act' (HR 582) which aims to raise the minimum wage to $15 by 2024 corresponds roughly to the $13 scenario if we deflate by 2.3% yearly.[6] The lower values might be more relevant if the COVID-19 pandemic crowds out all regular policy proposals. Alternatively, the 'Fight for $15' movement started in 2012. $15 in 2012 correspond to $17 in 2017 dollars, suggesting higher values will continue to attract lobbying attention.[7]

3.4 Shares over time

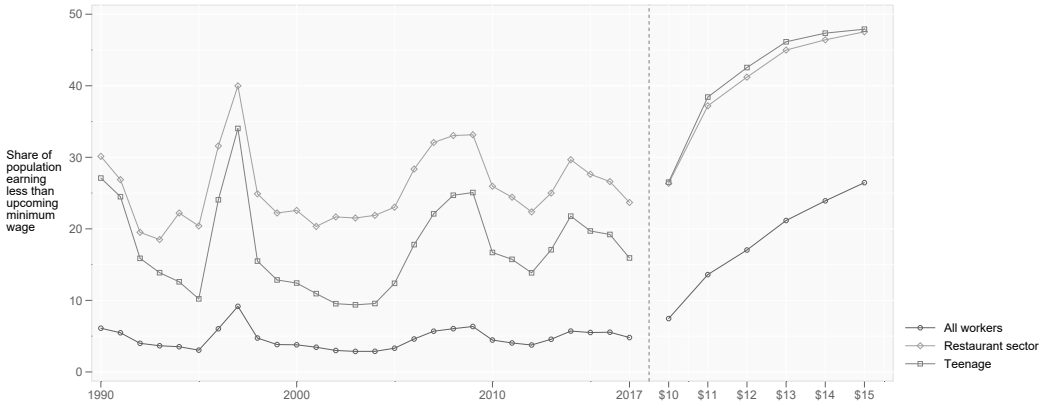


Figure 3.2: Affected shares for the restaurant sector, teenagers and the overall working population. Historical and projected.

Figure 3.2 immediately highlights the tension between existing research and potential large minimum wage changes. On the left, we show historical shares of the population affected by minimum wage policy. For example, the spike around 1996 reflects the federal minimum wage hikes of that period. The right side illustrates how the sample of treated workers changes at the introduction of increasingly high minimum wages (up to $15). Historically, the share of affected workers hovered near 5%, with a spike of 9% in 1997. This still covers the anticipated share of a $10 an

[6] This is the average CPI inflation predicted by the Congressional Budget Office's January 2019 ten-year forecast. Whether this is an accurate representation depends on three factors: a) the accuracy of the inflation forecast, b) whether wage growth in the lower part of the wage distribution follows price inflation and c) that wage growth is relatively uniform across the other dimensions considered (tenure, occupation and education).

[7] Source: `https://web.archive.org/web/20200512190614/https://fightfor15.org/about-us/`.

hour minimum wage (8%), but already at $11 the share increases to 14%. At $15 an hour, 27% of the working population would be affected, five times more than the historical average.

We also show shares for restaurant workers and employed teenagers separately. A vast share of minimum wage research has focused on these groups (e.g. Dube, Lester and Reich, 2010; Neumark, Salas and Wascher, 2014*b*) as they have historically been most strongly affected by minimum wage policy. Throughout the sample, restaurant workers were six times more likely to be affected than the average worker (for teens the ratio is four). Even in these targeted groups, proposed minimum wage policies would hit harder than before as a $15 minimum wage would affect almost every second worker in both subgroups (vs. at most 35-40%), potentially giving rise to different general equilibrium effect.

In the figures that follow, we try to identify the dimensions in which past minimum wage policy was similar and where proposed hikes are in entirely new territory. For ease of interpretation, we flip the analysis around relative to Figure 3.2: rather than looking at affected shares within a group, we show the shares of each group in all affected.

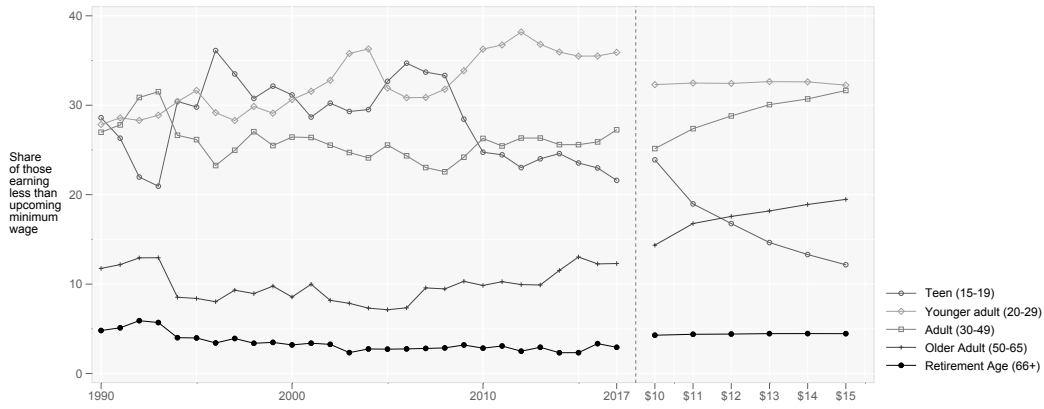


Figure 3.3: Share of affected workers by age group. Historical and projected.

We start by digging deeper into the age distribution of affected workers in Figure 3.3. Teenagers represented between 20-40% of all affected workers with peaks around the two large federal increases of 1996 and 2007. However, the right pane shows that studies based on this subgroup are only of limited use to predict the impact of large minimum wage changes as only 12% of workers affected by a $15 minimum wage would be teenagers (a 60% drop relative to the historical mean).

On the other side of the spectrum, the share of adults aged 50-65 doubles from an average of 10% to 19% at $15. These workers are at a different stage of their career, have different skills, work different positions and hold different contracts.

A similar story holds for retail and food services workers, the second group often studied (Figure 3.4). In the last two decades, they made up between 60 and 70 percent

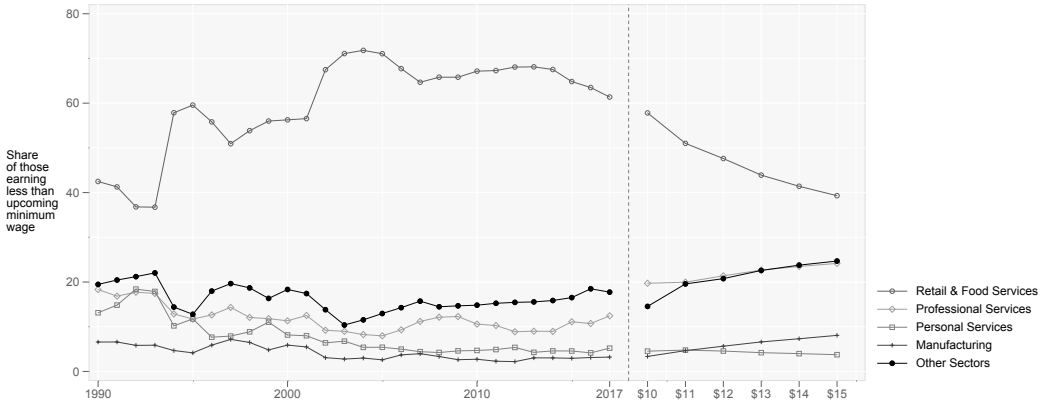


Figure 3.4: Share of affected workers by (modified) industry group. Historical and projected.

of all affected workers. They would remain the largest group for large minimum wage increases but their share almost halves to 39% for a $15 floor. The slack is taken up by professional services (24%), manufacturing (8%) and the restgroup of other sectors (25%). These industries differ in their skills and educational requirement, the tradeability of their output, labour contract types and both product and labour market competition intensity.

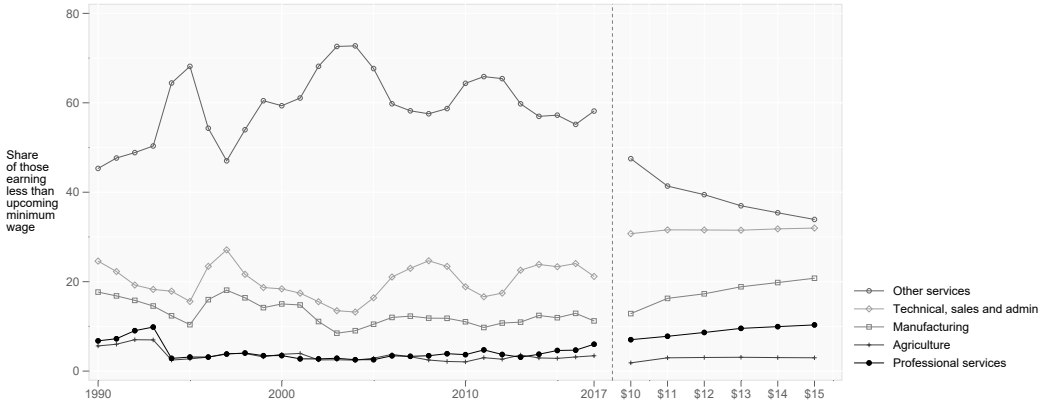


Figure 3.5: Share of affected workers by (modified) occupation group. Historical and projected.

We find similar evolutions when we turn to occupations in Figure 3.5. Other services, which includes most of the food and drink serving occupations historically comprise 59% of all affected workers, with spikes up to 73%. This share drops down to 34% for a hypothetical $15 floor. All other groups become more important relative

to recent decades, with the biggest increases for technical, sales and admin occupations (32%); manufacturing (21%) and professional services (10%).

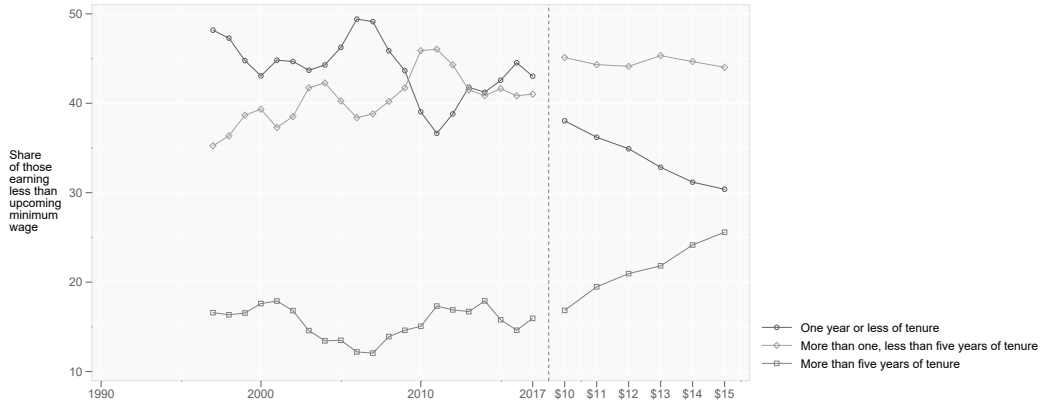


Figure 3.6: Share of affected workers by job tenure. Historical and projected, two year moving averages.

In Figure 3.6 we divide affected workers by their job tenure. Historically, these had either spent less than a year at their current job (44%) or anything between one and five years (41%, especially towards the end of the sample). Only 16% of affected workers had more than five years of tenure. For a $15 minimum wage, the share of beginning workers (less than one year of tenure) drops to 30%, with the shortfall made up entirely by the experienced variety (more than five years of tenure, 25%).

Similar concerns regarding the external validity of minimum wage research arise as for the changing age composition: workers with longer tenure may have stronger contracts, be less flexible and have more job specific skills than their starting counterparts. As a result, they might be harder to fire, more difficult to replace and follow a different trajectory when placed back on the job market.

In Figure 3.7 we split minimum wage workers by their education level. These shares have been surprisingly volatile over the sample. The federal minimum wage changes of 1996 and 2007 appear to have particularly affected workers without a high school diploma (44% and 38%), whereas this group's share never exceeded 28% after 2010. The share of workers with a high school diploma has remained fairly constant around 30%. On the contrary, the share of workers with at least some college education fluctuated heavily, from a low of 27% in 1997 to a high of almost 50% in 2012. The same trend is visible in the projected shares for a $15 minimum wage: less than high school workers represent just 20% of affected workers, whereas 45% has attended at least some college.

We split the college group into workers with and without degree in Figure 3.8. The evolution has been very similar, with actual college graduates representing about a third of all affected workers that attended some college. This share increases for a $15 minimum wage where they would make up about half the college group (20% of

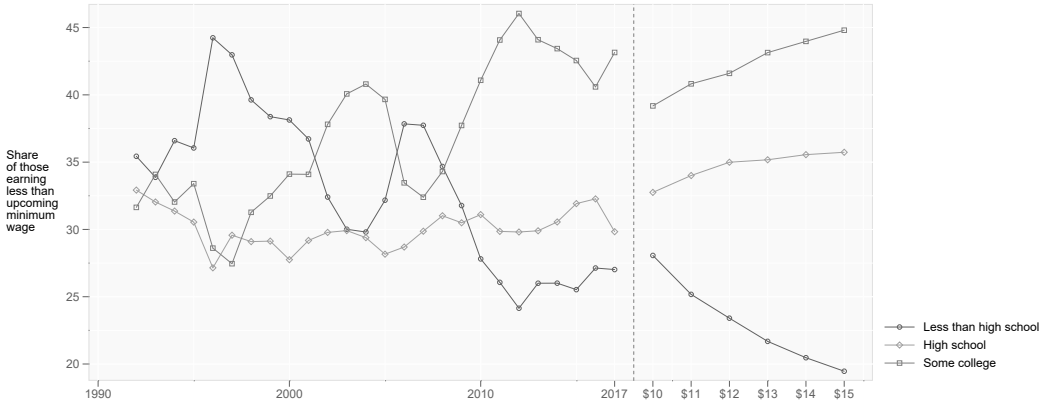


Figure 3.7: Share of affected workers by education level. Historical and projected.

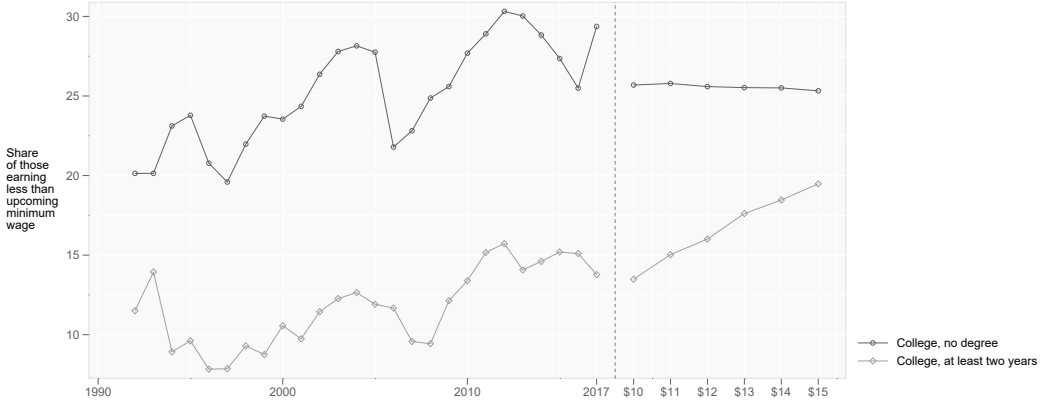


Figure 3.8: Share of affected workers by education level, college only. Historical and projected.

all affected workers). To the best of our knowledge, no one has studied the effect of minimum wage policy on college graduates. Indeed, they are often used as placebo group to validate studies on workers with less schooling (e.g. Godoy and Reich, 2019).

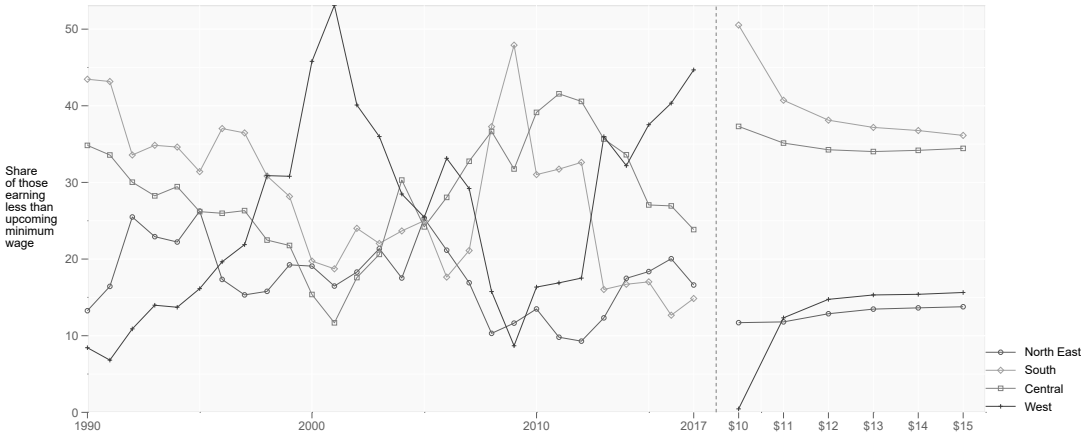


Figure 3.9: Share of affected workers by aggregated census division. Historical and projected.

There is also considerable geographical variation. In Figure 3.9, we see the difference between state and federal minimum wage changes. Where the West of the US tops the charts during regular years, this shifts to the South and Central states during federal minimum wage years.[8] We see the same pattern for the projected shares, with Western states unaffected at $10 and South and Central states accounting for ± 35% each. This highlights both the non-random nature of state minimum wages (cfr Allegretto, Dube and Reich, 2011 and Wursten, 2019) and the danger of extrapolating state-based studies to large federal minimum wage changes which bite in opposite states.

Figure 3.10 shows that the vast majority of historically affected workers (83%) required a pay increase of less than 15%. This changes drastically for the projected minimum wages. At $15 per hour, only 14% remains in that bin. The largest group (42%) would require a pay increase exceeding 50%, followed by a change between 15 and 50% (43% of affected workers). This matters in particular because the adaptation mechanisms preferred by the firm might depend on the size of the minimum wage change which wold give rise to nonlinear effects of the minimum wage. E.g. it could be that small wage increases are absorbed through decreases in markups, moderate ones by changes in prices, whereas large increases lead to personnel changes.

In Appendix 1 we show that higher minimum wages would increasingly affect families where the minimum wage worker is the sole breadwinner (57% vs 36% historically), that minimum wage policy mainly affects the poorest two quartiles of

[8] We use the following division. West: AK, CA, HI, OR, WA. South: TX, OK, AR, LA, MS, AL, TN, KY, WV, VA, MD, DE, DC, NC, SC, GA, FL. Central: MT, ID, NV, WY, UT, AZ, CO, NM, ND, SD, NE, KS, MN, IA, MO, WI, IL, MI, IN, OH. North East: PA, NJ, NY, CT, RI, MA, NH, VT, ME.

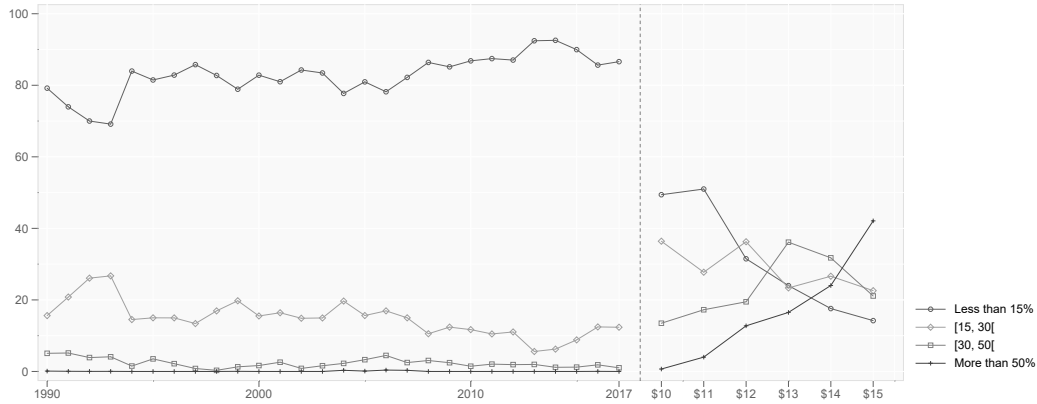
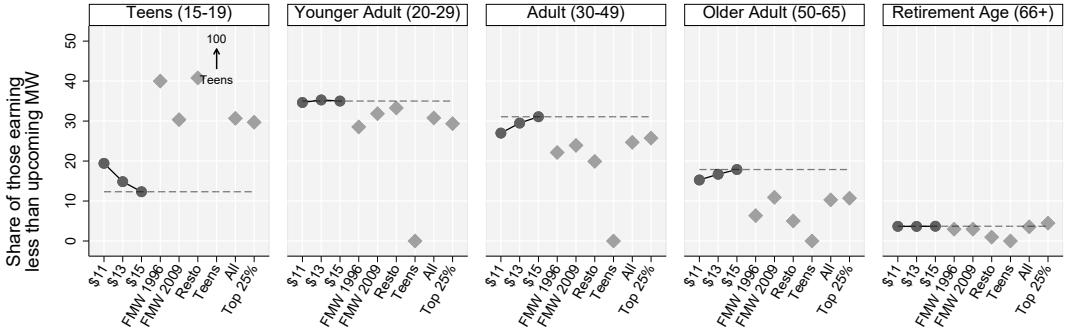


Figure 3.10: Share of affected workers by distance to new minimum wage. Historical and projected.

worker households (which remains true for a $15 floor) and that minimum wage workers are disproportionately likely to have insufficient funds to purchase food (35% versus 23% in the general working population, remains stable for the simulated large minimum wages).

3.5 Similarity by study type

Existing studies use some subset of the variation illustrated above. In this section, we show the characteristics of the treated group in various analysis archetypes and compare those to the predicted $15 treated group.



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	$15
Similarity	0.82	0.92	0.81	0.24	0.91	0.92	1

Figure 3.11: Similarity By Age Group

We start with the age distribution in Figure 3.11. The first panel shows the share of teenagers within the affected population. Within that panel, the three connected dots show this fraction for respectively a \$11, \$13 and \$15 minimum wage (cfr Figure 3.3). The first diamond estimates the same fraction for a hypothetical study of the 1996 federal minimum wage change (FMW 1996). That is, we restrict the sample to the six months before October 1996 and calculate which share of the affected workers in that time period were teenagers. We do the same for the 2009 federal minimum wage change (FMW 2009), but now restrict the sample to the states identified as bound by Clemens and Wither (2019) to represent more accurately what type of estimation is actually feasible.

Next, we restrict the sample to those employed in the restaurant sector[9] (Resto) and to teenagers (Teens), representing analyses in e.g. Dube, Lester and Reich (2010); Neumark, Salas and Wascher (2014*b*); Wursten (2019). The fifth type (All) does not impose any restriction at all and thus considers all observations where a worker earns less than the upcoming minimum wage (cfr Cengiz et al., 2019; Meer and West, 2016).[10] Finally, in the last type (Top 25%) we restrict the sample to the 25% of observations with the largest percentage gap between their current wage and the upcoming minimum. In practice, this includes all workers with an expected wage increase of 12.6%. This group represents efforts by e.g. Godoy and Reich (2019) and Wursten (2020) to identify minimum wage effects of the most vulnerable groups.

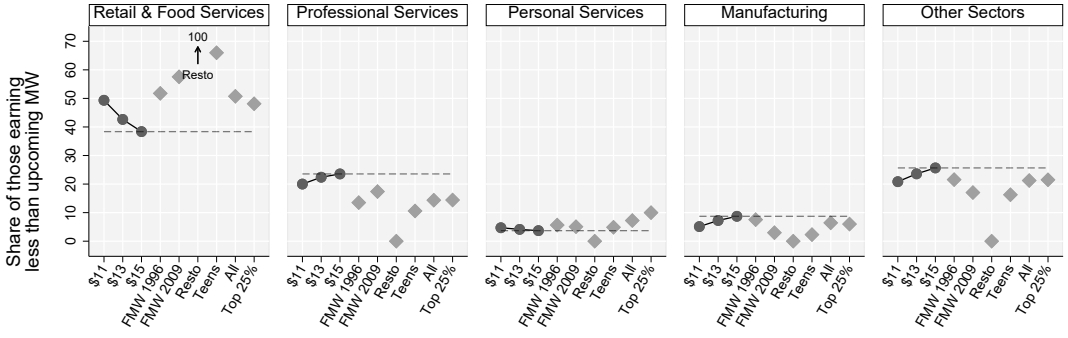
We find that all studies have a disproportionate share of teenagers. By design, this share is 100% in the teenager based studies. The prediction for the \$15 minimum wage is only 12%, much lower than any of the listed studies, which hover between 30 and 40%. Shares of young adults are much closer, all hovering near 35%. The gaps widen to about ten percentage points for adults and older adults. The share of 65-plussers is negligible in all cases. Below the graphs, we also show cosine similarity scores, which aggregate differences across these five age groups to one similarity index (range 0-1, higher is more similar, always defined relative to the \$15 scenario). As expected, the teenager based studies perform worst in this dimension (Score: 0.24). The clear winners are the FMW of 2009 and the unrestricted studies (All, Top 25%), however the difference with the FMW of 1996 and the restaurant based studies is not that large (0.82/0.81).

Figure 3.12 shows the industry distribution. We find that the retail and food services sector is overrepresented in all studies, particularly those based on restaurants (100%), teenagers (65%) and the FMW of 2009 (58%). Professional services and to a lesser extent the restgroup of other sectors are underrepresented. Perhaps surprisingly, the share of the manufacturing sector in workers in the \$15 population remains low and thus similar to the respective shares of the FMW of 1996 and the unrestricted studies (9% vs 6-8%).

We find the same pattern for occupations in Figure 3.13. The restgroup of other

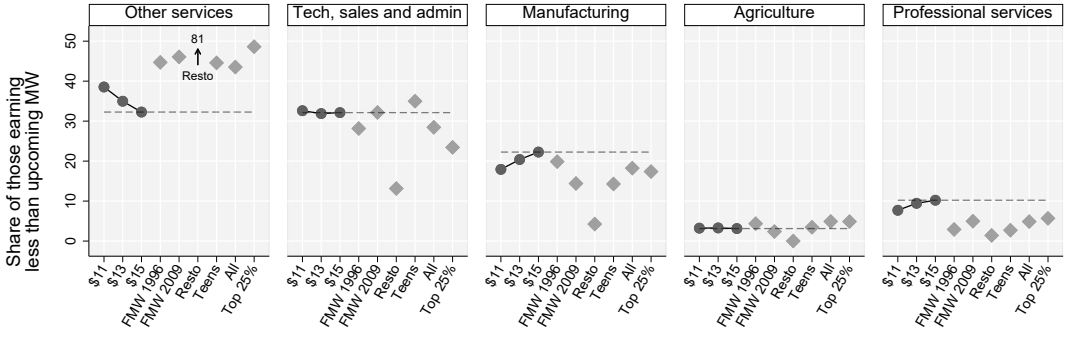
[9] The CPS does not provide NAICS codes, so we use code 641 of the ind1990 variable in the CPS files provided by IPUMS. Its description is 'Eating and drinking places'.

[10] We refer to Cengiz (2019) for a discussion on how well these studies manage to identify the treated observations.



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	$15
Similarity	0.96	0.94	0.73	0.89	0.96	0.96	1

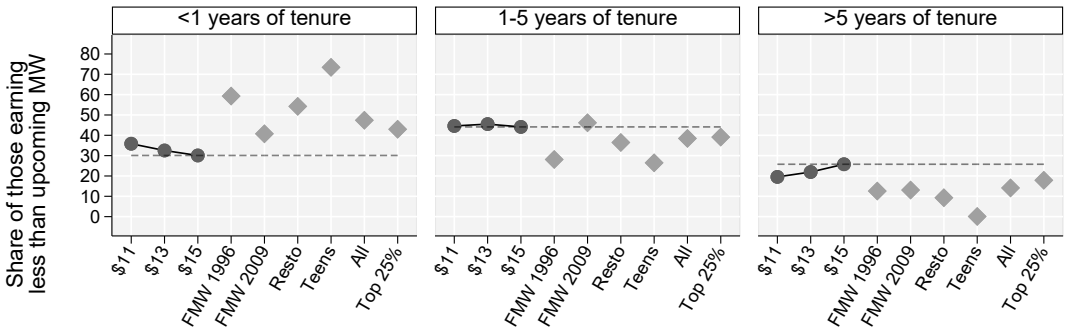
Figure 3.12: Similarity By Industry



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	$15
Similarity	0.96	0.96	0.74	0.96	0.97	0.94	1

Figure 3.13: Similarity By Occupation

services is overrepresented as it contains the main retail and food sector occupations. The shares of the FMW of 1996 and the unrestricted studies come quite close to the \$15 numbers for manufacturing, agriculture and tech, sales and admin. Professional services are underrepresented across the board. This group includes managers, engineers, scientists, doctors, nurses, teachers, social scientists, lawyers, writers and writers. It is difficult to use existing minimum wage variation to identify the impact on this group, as even in the unrestricted studies they represent only 5% of observations.[11]



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	\$15
Similarity	0.85	0.96	0.89	0.73	0.94	0.97	1

Figure 3.14: Similarity By Job Tenure

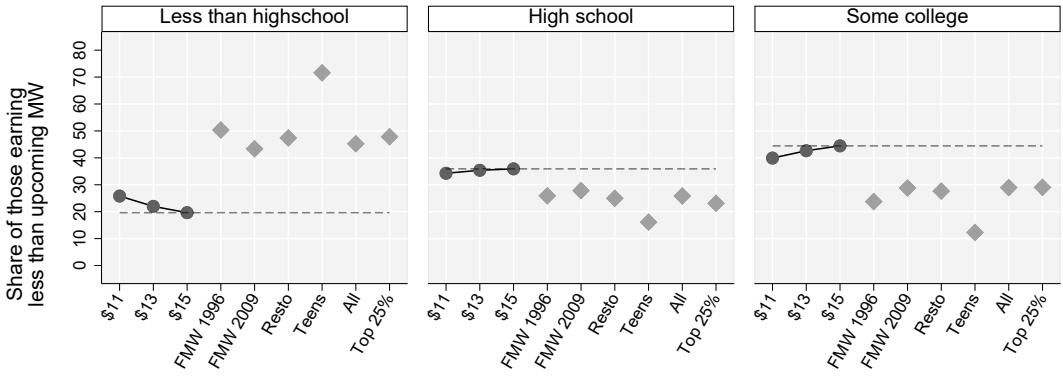
There are large differences in job tenure between approaches (Figure 3.14). At \$15, only 30% of affected workers would have less than one year of tenure. By contrast, this share is 70% for historically affected teens, 55% in the restaurant sector and 60% in the 1996 FMW episode. These differences are also reflected in the share with tenure between 1 and 5 years, and especially the group with more than 5 years. The FMW change of 2009 and the unrestricted studies (All, Top 25%) approach the \$15 distribution quite closely, with FMW 2009 doing better for 1-5 years and the Top 25% sample for 5 or more years. This is also reflected in the similarity scores, which are particularly high for FMW 2009 (0.96) and Top 25% (0.97) and low for teenagers (0.73).

The role of the minimum wage earner within the household also varies considerably (Appendix Figure C6). 90% of teenagers are not the sole earner in their household, versus 45% in the \$15 scenario. The discrepancy is smaller in the other approaches, with the FMW of 2009 as best of class. Household income distributions are almost identical across study types and close to the \$15 scenario (Appendix Figure C7).[12] Poor households are perhaps slightly underrepresented in studies of teenagers,

[11] E.g. Cengiz et al. (2019) estimate a 95% confidence interval for the own wage employment elasticity in the 'other' sector of [-1.4, 1.7] which means they cannot rule out a scenario where employment losses exceed earnings gains.

[12] As in Appendix Figure C2, we include only households with at least one working member and adjust

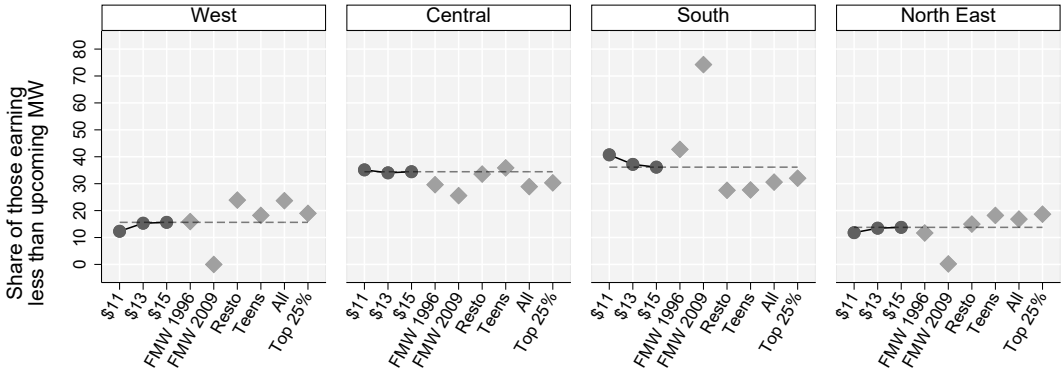
but the differences are small. Cosine similarities are 0.99 or larger.



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	$15
Similarity	0.80	0.88	0.84	0.56	0.86	0.84	1

Figure 3.15: Similarity By Education

This is not the case for education levels (Figure 3.15). The share of affected workers without a high school diploma is 20-30 percentage points lower in the $15 scenario (and up to 50 p.p. for the teenager sample). Workers with at least some college degree make up most of the difference (over 10 p.p.). Unsurprisingly, teenager studies are wholly uninformative about the minimum wage effects of this group.



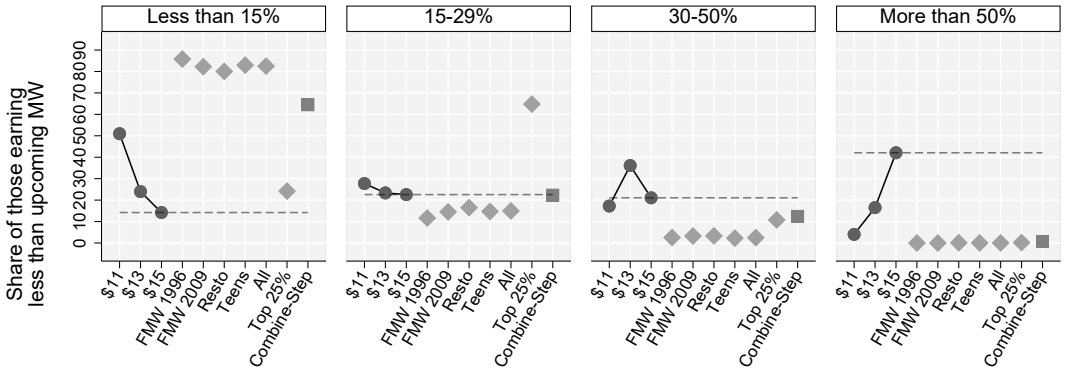
Type	FMW96	FMW09	Resto	Teens	All	Top 25%	$15
Similarity	0.99	0.84	0.98	0.98	0.98	0.99	1

Figure 3.16: Similarity By geography

Counter-intuitively, Figure 3.16 shows that the FMW of 2009 is a bad example

income by household size.

for a federal minimum wage of $15 in terms of geography as it overrepresents the Southern states and underrepresents the Western and North Eastern states, whereas the other setups match the projected shares very well (similarity scores above 0.98). The main cause is that we restricted the FMW 2009 sample to the treated states identified by Clemens and Wither (2019). This illustrates that even if the affected population in a particular type of study is representative, this does not hold for all studies applied to that context (usually due to a tradeoff between external and internal validity).



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	Combine-Step	$15
Similarity	0.33	0.35	0.36	0.34	0.34	0.54	0.46	1

Figure 3.17: Similarity By Expected Pay Increase

The historic-projected gap is largest when we look at percentage pay increases in Figure 3.17. Almost 9 in 10 historically affected workers required a raise of 15% or less to reach the new minimum wage, compared to just above 1 in 10 in the $15 scenario. On the other side of the spectrum, the $15 scenario would require a raise over 50% for 4 in 10 workers which applied to less than 1% in the historic samples, even if we restrict attention to the Top 25% affected. The latter do have some mass in the two mid-groups, 15-29% and 30-50%. Combining the three steps of the 2007-09 federal minimum wage change does not solve the issue (Combine-Step, squares) as that sample still lacks mass in the 50% plus group.[13]

3.6 Evidence by sub group

Overall, we find that none of the studies are representative on all dimensions. In this section we show that it is also not possible to obtain effect sizes per subgroup and

[13] In Combine-Step, we take all workers in 2007m1-m6 and compare their current hourly wage to the final 2009-step ($7.25, both deflated by CPI-U). Note that this assumes wage growth for these individuals tracks overall inflation. Given that we expect this group to have considerably higher wage growth (e.g. Dustmann et al., 2019), we can treat this as an upper bound of the share receiving a large pay increase.

aggregate those up to a weighted average by showing how much data we have for each subgroup. This proxies for the feasibility of analysing each subgroup separately. In Figure 3.18 we show for each main dimension (education, occupation, age, pay raise and tenure) which share of the $15 population is covered if you require at least X observations in the CPS. For example, the CPS has over 10 000 treated observations in each education group, thus covering 100% of the $15 population over the entire presented range of required observations. On the contrary, coverage in terms of expected pay raise drops to 60% at 50 observations because the CPS only contains 50 observations with an expected pay increase over 50% and this group represent 40% of the $15 population.[14]

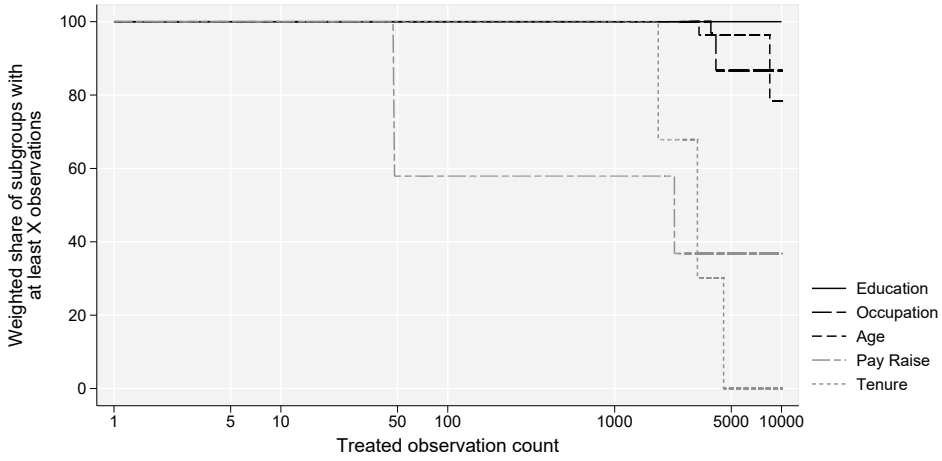


Figure 3.18: Share of represented sub-groups, one dimension.

If we set our threshold at 100 observations, we find 100% coverage for all dimensions except for the expected pay increase.[15] Table 3.2 shows that the same pattern holds when we consider two dimensions simultaneously.[16] The interactions that include the expected pay raise are at 58% coverage, whereas those without remain near 100%.[17] The interaction between expected raise and tenure is lowest of all as the tenure information is only available through the January/February Job Tenure Supplement of the CPS, not in the monthly questionnaire.

Coverage drops further if we interact three dimensions, as we illustrate for occupation-tenure-raise (23%) and occupation-education-raise cells (50%) in Figure 3.19. The main culprit remains the expected pay raise, as coverage increases to respectively

[14] One way to think of these lines is as survivor curves in the number of CPS observations (I am grateful to David Card for this suggestion).

[15] The results do not change qualitatively if we set a different threshold.

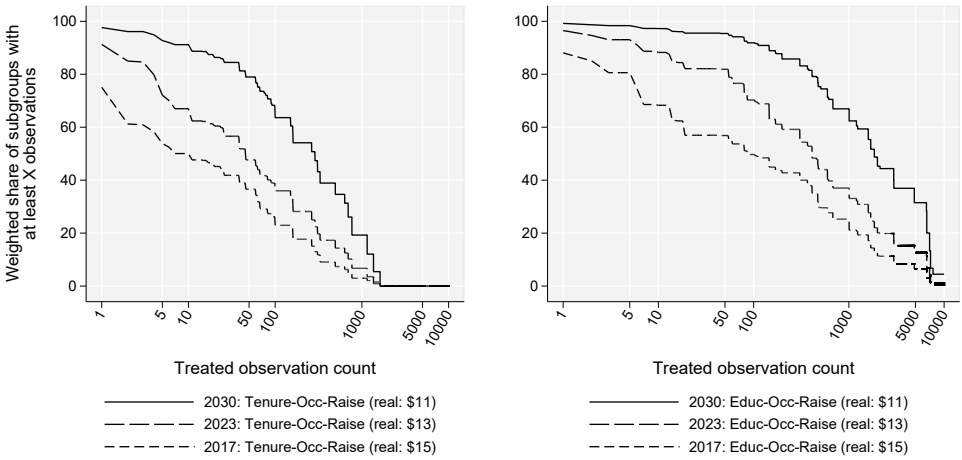
[16] We only show a selection of interactions to keep the table clear.

[17] Note that by definition coverage of the interactions group cannot exceed coverage of any of its constituents.

One Dimension	%	Two Dimensions	%	Three Dimensions	%
Age	100	Occ-Edu	100	Occ-Tenure-Raise ($15)	23
Occupation	100	Occ-Tenure	98	Occ-Tenure-Raise ($13)	36
Education	100	Edu-Raise	58	Occ-Tenure-Raise ($11)	64
Tenure	100	Occ-Raise	58	Occ-Edu-Raise ($15)	50
Pay Raise	58	Age-Raise	57	Occ-Edu-Raise ($13)	70
		Tenure-Raise	41	Occ-Edu-Raise ($11)	92

Table 3.2: Coverage at 100 observations per subgroup.

36% and 70% for a $13 minimum wage ($15 dollar in 2023) and 64% and 92% at $11 ($15 in 2030).



(a) By Job Tenure, Occupation and Expected Raise

(b) By Education, Occupation and Expected Raise

Figure 3.19: Coverage of represented sub-groups, three dimensions.

We illustrate this further in Table 3.3, which shows the share of each education-occupation-raise cell in the $15 population. Values in black have over 100 observations in the historic sample, those in grey do not. We collapsed the occupation dimension to three levels to keep the table informative. If one believes that minimum wage effects are relatively homogeneous within these cells, then this table suggests it is feasible to estimate cell-specific effects for all groups up to a 30% pay increase. For increases between 30 and 49%, this ceases to be the case for professional services and high school or more manufacturing workers, however these cells represent just 4% of the $15 population. None of the education by occupation groups have sufficient observations for a pay increase above 50% (representing 42% of the $15 population). The majority of this weight is on the non-professional services group. A

credible extrapolation for that group to a 50%+ increase would reduce the uncovered population to 16%.

Education	Occupation	< 15%	15-29%	30-49%	> 50%
Less than high school	Non-professional services	1	1	2	8
	Professional services	0	0	0	0
	Manufacturing and agriculture	1	2	2	3
High school	Non-professional services	3	5	5	10
	Professional services	0	1	0	1
	Manufacturing and agriculture	2	3	2	3
Some college	Non-professional services	4	7	6	12
	Professional services	1	2	1	2
	Manufacturing and agriculture	2	2	1	3
	Total	14	23	21	42

Table 3.3: Share in $15 population by education, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample.

We find better coverage in Table 3.4, which repeats the previous exercise for the $13 population ($15 by 2023). In this context, the uncovered cells represent just 26% of the target population, reduced to 13% if one manages to extrapolate the non-professional services estimates. In Appendix Tables C2-C4 we show that the situation is similar for age-occupation-raise cells, but considerably worse when we turn to tenure-occupation-raise cells (for the same reasons listed before).

Education	Occupation	< 15%	15-29%	30-49%	> 50%
Less than high school	Non-professional services	2	3	6	5
	Professional services	0	0	0	0
	Manufacturing and agriculture	2	2	2	1
High school	Non-professional services	5	6	9	4
	Professional services	1	1	1	0
	Manufacturing and agriculture	3	3	3	1
Some college	Non-professional services	7	7	10	4
	Professional services	2	1	2	1
	Manufacturing and agriculture	2	2	3	1
	Total	24	23	36	17

Table 3.4: Share in $13 population by education, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample.

3.7 Summary of empiric coverage

We briefly summarise the results before we turn to the discussion.

Shares over time. A striking first result is the sheer difference in the number of affected workers. Where existing minimum wage changes never involved more than 10% of the workforce, this increases to over 25% for a $15 minimum wage. Even within highly affected sub groups, the treated share reaches unseen levels, with almost 50% of restaurant workers and teenagers earning less than $15 per hour nationwide. The composition of affected workers also changes. Adults aged 30-65 make up almost half the target population, whereas the teenager share drops to almost 10%. Related, the share of workers with less than one year of tenure drops to about 30% (from a high of 50%), whereas 25% of affected workers would have over five years of tenure. These workers increasingly attended at least some college (and often attained a college degree), whereas the share of those without a high school degree drops to 20%. The increase in pay required to comply with minimum wage legislation would also be unprecedented - historically, almost no one received a raise above 30%, whereas this applies to almost 60% of workers for a $15 minimum wage. As the minimum wage bites more widely, we also see an increase in households where all workers are affected by the minimum wage (57%). The majority of this group also has dependants to take care of.

However, in other aspects existing minimum wage variation is surprisingly representative. Both in terms of industry and occupation, the projected shares are within the range of previous episodes. That is, although the share of manufacturing and technical jobs would be above the historical average, it is similar to the share during the 1996 federal minimum wage change. The distribution by income quartile also remains stable, affecting mainly the poorest quartile (45% of affected population) and passing by the richest (less than 10%).

... by study type. The discrepancies described above are present in all study types considered. Analyses that focus on a particular subgroup are least representative, not just in their niche, but also in the other dimensions (e.g. studies based on teenagers cannot capture the age distribution by definition, but are also less representative in terms of education levels, tenure and income distributions). The samples based on the federal minimum wage change of 2009 or the top 25% of treated workers are equally and sometimes more representative than the overall population.

... by subgroup. It is not possible to combine existing studies to fully cover the population affected by a $15 minimum wage. If we consider 100 observations sufficient to analyse an education-occupation-raise cell, we only cover half the $15 population. This drops to 23% if we divide cells by *tenure*, occupation and required raise size. The bottleneck is the size of the required raise. If we postpone the minimum wage change to 2030, we cover 92% of education-occupation-raise cells, but still just 64% of tenure-occupation-raise cells. If we delay to 2023 instead, coverage drops to respectively 59% (education-...) and 28% (tenure-...).

3.8 Edge between empirical and theoretical territory

In this section, we discuss the options available to the policymaker. We use the facts gathered in this paper to show that neither a purely theoretical nor a purely empirical approach can fully predict the effects of a large minimum wage change. Instead, we suggest a hybrid approach that combines empirical data points with theoretical modelling assumptions to produce a transparent framework for estimating minimum wage effects. Throughout this discussion we assume the policymaker is primarily interested in the effect of minimum wages on earnings and employment and assume that these drive e.g. the health and wellness effects attracting increasing attention (Dow et al., 2019).

Theory based. There is no consensus between theoretical models on the effects of minimum wage policy. In the competitive labour market model forcing wages above the market-clearing rate leads to unemployment. But this no longer holds when we assume firms have some bargaining power, which could derive from lack of competition in the labour market (classic monopsony models) or search frictions and non-pecuniary benefits. Then, minimum wages can increase employment by lowering the marginal cost of labour, even if the average cost increases. However, at sufficiently high minimum wage levels, the relation changes signs and minimum wages again reduce employment (Dickens, Machin and Manning, 1999). In a model of efficiency wages with observed effort, the sign of the employment effect depends on tradeoffs in the labour supply decision of the low wage workers (Deltas, 2007). Prospective workers can take up both more and less schooling depending on the level of the minimum wage (Flinn, Gemici and Laufer, 2017). Pure theory models also cannot provide the inputs required to estimate a welfare function that balances the gains and losses of employers, consumers, employees and those who might lose their job.

Empirics based. Those inputs need to come from empirical studies. Here too, there is considerable disagreement between studies, although the gap appears to be narrowing in recent years (see e.g. Wolfson and Belman, 2019). However, even if there were no disagreement, we have shown in this study that the affected population in existing studies is not representative for those affected by a large minimum wage shock. Moreover, representativeness cannot be achieved by reweighting (e.g. Stuart et al., 2011), as we simply do not have enough information for certain cells (e.g. we cover only 50% of occupation-education-raise cells for a $15 minimum wage). As a result, anyone extrapolating current studies to a large minimum wage shock is implicitly making several assumptions on how those studies generalise to workers with different characteristics, particularly in terms of the size of the required raise.

Hybrid. Instead, we recommend making those assumptions implicit. That is, we suggest using analyses such as our own to identify the edge between what can be evaluated empirically and where you need structural assumptions to cover the full effect. An example is the simulation approach taken by Reich, Allegretto and Montialoux (2019), who model how firms, consumers and workers react to a large minimum wage shock. They calibrate these structural assumptions through generic estimates

from the literature on e.g. price passthrough, employee turnover responses and corresponding cost savings and price elasticities. In their calibration, employment remains stable as the positive forces (reduced employee turnover, higher prices, higher productivity) balance out the negative forces (higher payroll costs, reduced product demand).

The Reich, Allegretto and Montialoux (2019) study is relatively structural as it never incorporates empirical evidence on the earnings and employment effects of the minimum wage. A more reduced form version could start from these estimates and then build a structural model around it to fill in the gaps. This suggests three areas of further research. First, some of the subgroups identified in Section 3.4 have not been investigated empirically. For others, the confidence intervals are very wide.[18] Finding ways to tighten and complete this set of estimates would reduce the uncertainty of any model built around them. Second, there is almost no information on heterogeneity of minimum wage effects by firm characteristics. I.e., while there are estimates on how minimum wage effects differ by worker age (e.g. Liu, Hyclak and Regmi, 2016; Wursten, 2020), we do not know whether firm age and size matter.

Third, there is scope for more structural modelling around the existing empirical literature, given that even the perfect empirical study cannot be fully representative for large minimum wage changes. The results in this paper suggest a structure that starts from empirical estimates and then makes assumptions on how they generalise to other groups, in particular in terms of job tenure and required raise size. These assumptions would be untestable, but can be motivated through structural models in the spirit of e.g. Reich, Allegretto and Montialoux (2019); Aaronson, French and MacDonald (2008); Sorkin (2015); Flinn, Gemici and Laufer (2017) or from international evidence (e.g. Harasztosi and Lindner, 2019). Ideally, the model would allow the end user to modify the assumptions and observe how that affects the bounds of the implied minimum wage effects (e.g. how do they change if we make the employment effect more responsive to the size of the required pay increase).

3.9 Conclusion

In this paper, we compared the population of historically affected minimum wage workers to those that would see their wages changed by a large minimum wage increase (e.g. to $15). We find that such a large increase would affect considerably more workers in general and that these workers are older and have been at their job longer. They are more likely to be the sole earner in their household, to hold a college degree and to live in the Southern and Central regions of the US. They would also require a much larger pay increase to comply with the new minimum wage floor than those affected by recent minimum wages. We do not find large shifts in industry shares, occupations, income quartiles or food security.

[18] E.g. the 95% confidence interval for the own wage employment elasticities in the manufacturing sector estimated in Cengiz et al. (2019) is [-7.2, 4.4].

These imbalances are also present in most minimum wage studies and are sometimes even more extreme. By definition, studies based on teenagers do not capture the rest of the age distribution, but this also heavily distorts their education, tenure and family type distributions. A similar, albeit less extreme scenario plays out for restaurant workers. Studies based on the overall low wage population perform better, but remain biased towards younger workers without a high school degree who are not the sole earner in their family. None of the studies are able to match the expected pay increase implied by a large minimum wage increase. The difference becomes less pronounced as we delay the implementation of the change, e.g. studies looking at either the top 25% affected or combining the three steps of the 2007-09 federal minimum wage change are in the same ballpark as a $15 dollar minimum wage implemented in 2030 ($11 in 2017 dollars).

Moreover, we use the number of observations in the CPS to proxy for the information available per subgroup (e.g. college educated workers) and show that coverage is good for all characteristics except the required pay raise. As a result, coverage is limited for any multi-dimensional groups that includes the pay increase dimension (e.g. college educated workers requiring a pay increase above 50%). This implies that it is not possible to predict the effect of a large increase by reweighting results for each subgroup without making structural assumptions.

We combine these facts to evaluate various ways of predicting the impact of a large minimum wage change and put forward as optimal solution a model that starts from the extensive existing empirical literature, but then uses structural modelling to transparently generalise these results to the entire affected population.

Overall, the conclusion of this paper is cautiously optimistic. Although current empirical research cannot be extrapolated to a large minimum wage change as is, we do find that the gap is smaller than might be expected. For example, one might expect the minimum wage to become relevant in manufacturing, yet we show that most wages in that sector already exceed even the largest minimum wage hike considered in this study. Moreover, if we split the target population by broad education and occupation groups, then extrapolating minimum wage effects for non-professional services workers to higher required raise sizes already reduces the uncovered population to 13% (for a $15 minimum wage in 2023).

Bibliography

Aaronson, Daniel, Eric French, and James MacDonald. 2008. "The Minimum Wage, Restaurant Prices, and Labor Market Structure." *The Journal of Human Resources*, 43(3): 688–720.

Abowd, John M., Bryce E. Stephens, Lars Vilhuber, Fredrik Andersson, Kevin L. McKinney, Marc Roemer, and Simon Woodcock. 2009. "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators." In *Producer Dynamics: New Evidence from Micro Data*. *NBER Chapters*, 149–230. National Bureau of Economic Research, Inc.

Addison, John T., McKinley L. Blackburn, and Chad D. Cotti. 2012. "The Effect of Minimum Wages on Labour Market Outcomes: County-Level Estimates from the Restaurant-and-Bar Sector." *British Journal of Industrial Relations*, 50(3): 412–435.

Alesina, Alberto, and Paola Giuliano. 2015. "Culture and Institutions." *Journal of Economic Literature*, 53(4): 898–944.

Allegretto, Sylvia A., Arindrajit Dube, and Michael Reich. 2011. "Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data." *Industrial Relations: A Journal of Economy and Society*, 50(2): 205–240.

Allegretto, Sylvia, Arindrajit Dube, and Michael Reich. 2010. "Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data." Institute of Industrial Relations, UC Berkeley Institute for Research on Labor and Employment, Working Paper Series qt7jq2q3j8.

Allegretto, Sylvia, Arindrajit Dube, Michael Reich, and Ben Zipperer. 2013. "Credible Research Designs for Minimum Wage Studies." *IZA Discussion Paper Series*, , (7638): 1–79.

Alt, James E., and Robert C. Lowry. 1994. "Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States." *The American Political Science Review*, 88(4): 811–828.

Andrews, Isaiah, and Emily Oster. 2017. "Weighting for External Validity." National Bureau of Economic Research Working Paper 23826.

Autor, David H., Alan Manning, and Christopher L. Smith. 2016. "The Contribution of the Minimum Wage to US Wage Inequality over Three Decades: A Reassessment." *American Economic Journal: Applied Economics*, 8(1): 58–99.

Baskaya, Yusuf Soner, and Yona Rubinstein. 2015. "Using Federal Minimum Wages to Identify the Impact of Minimum Wages on Employment and Earnings across U.S. States." unpublished.

Beland, Louis-Philippe, and Sara Oloomi. 2017. "Party Affiliation and Public Spending: Evidence from U.S. Governors." *Economic Inquiry*, 55(2): 982–995.

Bell, Stephen H., and Elizabeth A. Stuart. 2016. "On the "Where" of Social Experiments: The Nature and Extent of the Generalizability Problem." *New Directions for Evaluation*, 2016(152): 47–59.

Belman, Dale, and Paul J. Wolfson. 2014. *What Does the Minimum Wage Do?* W.E. Upjohn Institute for Employment Research.

Besley, Timothy, and Anne Case. 2003. "Political Institutions and Policy Choices: Evidence from the United States." *Journal of Economic Literature*, 41(1): 7–73.

Bjørnskov, Christian, and Niklas Potrafke. 2013. "The size and scope of government in the US states: Does party ideology matter?" *International Tax and Public Finance*, 20(4): 687–714.

Bonica, Adam. 2015. "Database on Ideology, Money in Politics, and Elections (DIME)." Harvard Dataverse.

Brown, Charles, Curtis Gilroy, and Andrew Kohen. 1982. "The Effect of The Minimum Wage on Employment and Unemployment." *Journal of Economic Literature*, 20(2): 487–528.

Caliendo, Marco, Alexandra Fedorets, Malte Preuss, Carsten Schröder, and Linda Wittbrodt. 2018. "The short-run employment effects of the German minimum wage reform." *Labour Economics*, 53(C): 46–62.

Campbell, Andrea Louise, and Michael W. Sances. 2013. "State Fiscal Policy during the Great Recession: Budgetary Impacts and Policy Responses." *The ANNALS of the American Academy of Political and Social Science*, 650(1): 252–273.

Card, David. 1992. "Using Regional Variation in Wages to Measure the Effects of the Federal Minimum Wage." Princeton University, Department of Economics, Industrial Relations Section. Working Papers 680.

Card, David, and Alan B Krueger. 1994. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review*, 84(4): 772–93.

Card, David, and Alan B. Krueger. 2000. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Reply." *The American Economic Review*, 90(5): 1397–1420.

Case, Anne, and Angus Deaton. 2017. "Mortality and Morbidity in the 21st Century." Princeton University, Woodrow Wilson School of Public and International Affairs, Research Program in Development Studies. Working Papers 2017-spring.

CBO. 2019. "The Effects on Employment and Family Income of Increasing the Federal Minimum Wage." Congressional Budget Office Report.

Cengiz, Doruk. 2019. "Seeing Beyond the Trees: Using machine learning to estimate the impact of minimum wages on affected individuals." Job Market Paper.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The Effect of Minimum Wages on Low-Wage Jobs: Evidence from the United States Using a Bunching Estimator." National Bureau of Economic Research, Inc NBER Working Papers 25434.

Chang, Chun-Ping, Yoonbai Kim, and Yung hsiang Ying. 2009. "Economics and politics in the United States: a state-level investigation." *Journal of Economic Policy Reform*, 12(4): 343–354.

Clemens, Jeffrey, and Michael Wither. 2019. "The minimum wage and the Great Recession: Evidence of effects on the employment and income trajectories of low-skilled workers." *Journal of Public Economics*, 170: 53 – 67.

Cooper, David. 2017. "Raising the minimum wage to $15 by 2024 would lift wages for 41 million American workers." Economic Policy Institute Policy Report.

Deltas, George. 2007. "Can a minimum wage increase employment and reduce prices in a neoclassical perfect information economy?" *Journal of Mathematical Economics*, 43(6): 657–674.

Dickens, Richard, Stephen Machin, and Alan Manning. 1999. "The Effects of Minimum Wages on Employment: Theory and Evidence from Britain." *Journal of Labor Economics*, 17(1): 1–22.

DNC. 2016. "2016 Democratic Party Platform."

Dow, William H, Anna Godøy, Christopher A Lowenstein, and Michael Reich. 2019. "Can Economic Policies Reduce Deaths of Despair?" National Bureau of Economic Research Working Paper 25787.

Dube, Ardindrajit. 2019. "Impacts of minimum wages: review of the international evidence." HM Treasury Independent report PU2287.

Dube, Arindrajit, T. William Lester, and Michael Reich. 2010. "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties." *The Review of Economics and Statistics*, 92(4): 945–964.

Dube, Arindrajit, T. William Lester, and Michael Reich. 2016. "Minimum Wage Shocks, Employment Flows, and Labor Market Frictions." *Journal of Labor Economics*, 34(3): 663–704.

Dustmann, Christian, Attila Lindner, Uta Schoenberg, Matthias Umkehrer, and Philipp vom Berge. 2019. "Reallocation Effects of the Minimum Wage." Undefined Working Papers 0.

Even, William E., and David A. Macpherson. 2019. "Where Does the Minimum Wage Bite Hardest in California?" *Journal of Labor Research*, 40(1): 1–23.

Flinn, Christopher, Ahu Gemici, and Steven Laufer. 2017. "Search, Matching, and Training." *Review of Economic Dynamics*, 25: 260–297.

Flinn, Christopher J. 2006. "Minimum Wage Effects on Labor Market Outcomes under Search, Matching, and Endogenous Contact Rates." *Econometrica*, 74(4): 1013–1062.

Flood, Sarah, Miriam King, Renae Rodgers, Steven Ruggles, and J. Robert Warren. 2020. "Integrated Public Use Microdata Series, Current Population Survey: Version 7.0 [dataset]." `https://cps.ipums.org/cps/index.shtml`.

Frölich, Markus. 2006. "A Note on Parametric and Nonparametric Regression in the Presence of Endogenous Control Variables." Institute for the Study of Labor (IZA) IZA Discussion Papers 2126.

Gechter, Michael. 2015. "Generalizing the Results from Social Experiments: Theory and Evidence from Mexico and India." Working Paper.

Glasgow, Russell E., Candace C. Nelson, Lisa A. Strycker, and Diane K. King. 2006. "Using RE-AIM Metrics to Evaluate Diabetes Self-Management Support Interventions." *American Journal of Preventive Medicine*, 30(1): 67 – 73.

Godoy, Anna, and Michael Reich. 2019. "Minimum Wage Effects in Low-Wage Areas." IRLE Working Paper 106-19.

Goodman-Bacon, Andrew. 2018. "Difference-in-Differences with Variation in Treatment Timing." National Bureau of Economic Research Working Paper 25018.

Grossman, Jean Baldwin. 1983. "The Impact of the Minimum Wage on Other Wages." *Journal of Human Resources*, 18(3): 359–378.

Gu, Ji, Ryan A. Compton, Daniel C. Giedeman, and Gary A. Hoover. 2017. "A note on economic freedom and political ideology." *Applied Economics Letters*, 24(13): 928–931.

Harasztosi, Peter, and Attila Lindner. 2019. "Who Pays for the Minimum Wage?" *American Economic Review*, 109(8): 2693–2727.

Heckman, James J., and Edward Vytlacil. 2005. "Structural Equations, Treatment Effects, and Econometric Policy Evaluation." *Econometrica*, 73(3): 669–738.

Hill, Andrew J., and Daniel B. Jones. 2017. "Does partisan affiliation impact the distribution of spending? Evidence from state governments' expenditures on education." *Journal of Economic Behavior & Organization*, 143(C): 58–77.

Imai, Kosuke, Gary King, and Elizabeth Stuart. 2008. "Misunderstandings Among Experimentalists and Observationalists about Causal Inference." *Journal of the Royal Statistical Society, Series A*, 171, part 2: 481–502.

Jardim, Ekaterina, Mark Long, Robert Plotnick, Emma van Inwegen, Jacob Vigdor, and Hilary Wething. 2017. "Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence from Seattle." National Bureau of Economic Research, Inc NBER Working Papers 23532.

Jorda, Oscar. 2005. "Estimation and Inference of Impulse Responses by Local Projections." *American Economic Review*, 95(1): 161–182.

Kaitz, Hyman B. 1970. "Experience of the Past: The National Minimum." Bureau Of Labor Statistics Bulletin 1657.

Las Vegas Convention and Visitors Authority. 2010. "The Relative Dependence of Tourism of Major U.S. Economies." Las Vegas Convention and Visitors Authority.

Leigh, Andrew. 2008. "Estimating the impact of gubernatorial partisanship on policy settings and economic outcomes: A regression discontinuity approach." *European Journal of Political Economy*, 24(1): 256–268.

Liu, Shanshan, Thomas J. Hyclak, and Krishna Regmi. 2016. "Impact of the Minimum Wage on Youth Labor Markets." *LABOUR*, 30(1): 18–37.

Macrosty, Henry W. 1898. "The Recent History of the Living Wage Movement." *Political Science Quarterly*, 13(3): 413–441.

MaCurdy, Thomas. 2015. "How Effective Is the Minimum Wage at Supporting the Poor?" *Journal of Political Economy*, 123(2): 497 – 545.

Meer, Jonathan, and Jeremy West. 2016. "Effects of the Minimum Wage on Employment Dynamics." *Journal of Human Resources*, 51(2): 500–522.

Neumark, David. 2019. "The Econometrics and Economics of the Employment Effects of Minimum Wages: Getting from Known Unknowns to Known Knowns." *German Economic Review*, 20(3): 293–329.

Neumark, David, and William Wascher. 1992. "Employment Effects of Minimum and Subminimum Wages: Panel Data on State Minimum Wage Laws." *ILR Review*, 46(1): 55–81.

Neumark, David, and William Wascher. 2000. "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Comment." *American Economic Review*, 90(5): 1362–1396.

Neumark, David, and William Wascher. 2006. "Minimum Wages and Employment: A Review of Evidence from the New Minimum Wage Research." University of California-Irvine, Department of Economics Working Papers 060708.

Neumark, David, and William Wascher. 2007. "Minimum Wages, the Earned Income Tax Credit, and Employment: Evidence from the Post-Welfare Reform Era." Institute of Labor Economics (IZA) IZA Discussion Papers 2610.

Neumark, David, and William Wascher. 2017. "Reply to "Credible Research Designs for Minimum Wage Studies"." *ILR Review*, 70(3): 593–609.

Neumark, David, JM Ian Salas, and William Wascher. 2014*a*. "More on recent evidence on the effects of minimum wages in the United States." *IZA Journal of Labor Policy*, 3(1): 24.

Neumark, David, J. M. Ian Salas, and William Wascher. 2014*b*. "Revisiting the Minimum Wage—Employment Debate: Throwing Out the Baby with the Bathwater?" *ILR Review*, 67(3_suppl): 608–648.

Pickering, Andrew C., and James Rockey. 2013. "Ideology and the size of US state government." *Public Choice*, 156(3): 443–465.

Reed, W. 2006. "Democrats, republicans, and taxes: Evidence that political parties matter." *Journal of Public Economics*, 90(4-5): 725–750.

Reich, Michael. 2019. "Likely Effects of a $15 Federal Minimum Wage by 2024." IRLE Policy Report.

Reich, Michael, Sylvia Allegretto, and Claire Montialoux. 2019. "The Employment Effects of a $15 Minimum Wage in the US and in Mississippi: A Simulation Approach." IRLE Policy Report.

RNC. 2016. "Republican Platform 2016."

Rojas, Mariano. 2014. "Household Equivalence Scale." *Encyclopedia of Quality of Life and Well-Being Research*, , ed. Alex C. Michalos, 2947–2950. Dordrecht:Springer Netherlands.

Ropponen, Olli. 2011. "Reconciling the evidence of Card and Krueger (1994) and Neumark and Wascher (2000)." *Journal of Applied Econometrics*, 26(6): 1051–1057.

Sanderson, Eleanor, and Frank Windmeijer. 2016. "A weak instrument F-test in linear IV models with multiple endogenous variables." *Journal of Econometrics*, 190(2): 212–221.

Shor, Boris, and Nolan M. McCarty. 2011. "The Ideological Mapping of American Legislatures." *American Political Science Review*, 105(August): 530–551.

Sorkin, Isaac. 2015. "Are There Long-Run Effects of the Minimum Wage?" *Review of Economic Dynamics*, 18(2): 306–333.

Soss, Joe, Sanford F. Schram, Thomas P. Vartanian, and Erin O'Brien. 2001. "Setting the Terms of Relief: Explaining State Policy Choices in the Devolution Revolution." *American Journal of Political Science*, 45(2): 378–395.

Stuart, Elizabeth A., Stephen R. Cole, Catherine P. Bradshaw, and Philip J. Leaf. 2011. "The use of propensity scores to assess the generalizability of results from randomized trials." *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 174(2): 369–386.

Thompson, Jeffrey P. 2009. "Using Local Labor Market Data to Re-Examine the Employment Effects of the Minimum Wage." *ILR Review*, 62(3): 343–366.

Totty, Evan. 2015. "The Effect of Minimum Wages on Employment: A Factor Model Approach." Institute of Industrial Relations, UC Berkeley Institute for Research on Labor and Employment, Working Paper Series.

Warren, Patrick L. 2009. "State Parties and Taxes: A Comment on Reed in the Context of Close Legislatures." unpublished.

Wolfson, Paul, and Dale Belman. 2019. "15 Years of Research on US Employment and the Minimum Wage." *LABOUR*, 33(4): 488–506.

Wursten, Jesse. 2019. "Is Politics the Missing Piece of the Minimum Wage Puzzle?" KU Leuven, Faculty of Economics and Business, Department of Managerial Economics, Strategy and Innovation (MSI) Working Papers Department of Managerial Economics, Strategy and Innovation (MSI) 1905.

Wursten, Jesse. 2020. "Estimating the earnings and employment effects of the minimum wage through differences in exposure across US counties." KU Leuven, Faculty of Economics and Business, Department of Managerial Economics, Strategy and Innovation (MSI) Working Papers Department of Managerial Economics, Strategy and Innovation (MSI) 2006.

Zipperer, Ben. 2016. "Did the minimum wage or the Great Recession reduce low-wage employment? Comments on Clemens and Wither (2016)." Washington Center for Equitable Growth WCEG Working Papers n.a.

Appendix to Chapter 1

1 Endogenous control bias

In this section we provide an illustration of endogenous control bias using a simple Monte Carlo exercise. We start by drawing four independent, standard normal errors u_e, u_p, u_m, u_v and define our variables as described in Equations 3-6. In this setup, the instrument $values$ is fully exogenous and explains one fifth of the variation in political ideology pol.[19] The remainder is explained by the employment shock u_e (20%) and the political ideology shock u_p (60%).

$$values = u_v \tag{3}$$

$$pol = \frac{1}{5}values + \frac{1}{5}u_e + \frac{3}{5}u_p \tag{4}$$

$$mw = \frac{2}{5}u_p + \frac{3}{5}u_m \tag{5}$$

$$emp = 1 + pol + mw + u_e \tag{6}$$

The minimum wage is correlated with the political ideology variable as it depends both on the political ideology shock u_p (40%) and the minimum wage shock u_m (60%). Employment is then defined as the sum of a constant (1), pol, mw and the employment shock u_e, all entering with a coefficient of one. We apply three regressions to this data. First, in Equation 7 we omit the political ideology variable, simply regressing employment on a constant and the minimum wage.

$$emp = \alpha_1 + \beta_{1,M} * mw + \epsilon_1 \tag{7}$$

$$emp = \alpha_2 + \beta_{2,M} * mw + \beta_{2,P} * pol + \epsilon_2 \tag{8}$$

$$emp = \alpha_3 + \beta_{3,M} * mw + \beta_{3,P} * pol + \epsilon_{3,e} \tag{9}$$

$$pol = \gamma_3 + \delta_{3,M} * mw + \delta_{3,V} * values + \epsilon_{3,p} \tag{10}$$

In Equation 8 we add the political ideology variable as control. Finally, in Equations 9-10 we instrument the political ideology variable with the exogenous social values. We show Monte Carlo results in Table A1, setting the number of observations

[19] We omit the subscript i. There is no panel structure in this MC exercise.

to 1000 and the number of simulations to 5000. The mean estimate of the minimum wage effect in specification 7 (omitted variable scenario) is 1.46 times too large and even the bottom percentile of MC estimates is still 1.33 times too large. Turning to specification 8, we find that both the political ideology estimate (×1.61) and the minimum wage estimate (×0.72) are strongly biased, even though only the political ideology variable is correlated with the error term. When we instrument the political ideology variable (specification 9), the bias disappears for both estimates, with means of 1.00 (politics) and 0.99 (minimum wage).

Table A1: Monte Carlo results

	Mean	p1	p25	p50	p75	p99
Minimum wage estimates						
(7) No pol control	1.46	1.33	1.42	1.46	1.50	1.60
(8) With pol control	0.72	0.61	0.69	0.72	0.75	0.83
(9) With pol IV	1.00	0.81	0.94	1.00	1.06	1.22
Political ideology estimates						
(8) With pol control	1.61	1.49	1.57	1.61	1.64	1.73
(9) With pol IV	0.99	0.58	0.89	1.00	1.11	1.35

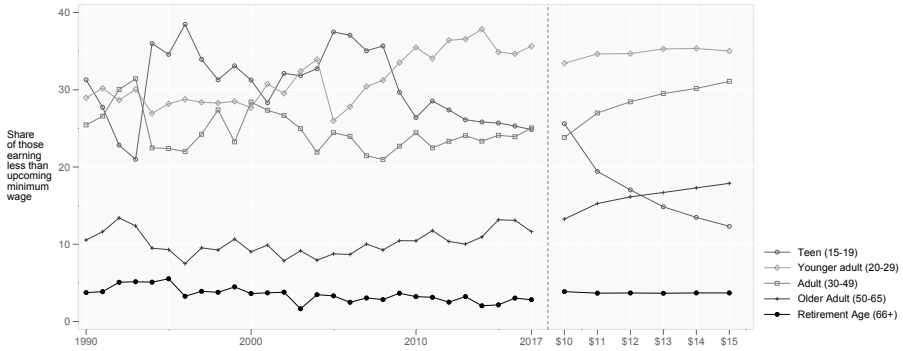
See Equations 7-9 for descriptions of the three estimated regression equations. Each regression used 1000 observations and was repeated 5000 times. The first column shows the mean of the coefficient estimates across simulations, the next columns show percentiles 1, 25, 50, 75 and 99. The true coefficients of the minimum wage and of politics are both one.

2 Tables and Figures

Table A2: Occupational distribution of the restaurant sector workforce in 2016

Occupation (abbreviated OCC titles)	# of employees in restaurant sector	% of restaurant employment	Average hourly wage (in $)
Fastfood and Counter Workers	3,031,790	29.47	9.57
Waiters and Waitresses	2,105,830	20.47	11.57
Cooks	1,659,270	16.13	11.28
Supervisors	742,610	7.22	16.66
Food Preparation Workers	424,590	4.13	10.57
Dishwashers	376,130	3.66	9.97
Hosts and Hostesses	360,060	3.50	10.15
Cashiers	313,850	3.05	9.79
Attendants and Helpers	273,830	2.66	10.44
Bartenders	269,980	2.62	12.88
Sales Workers and Drivers	217,080	2.11	10.39
Food Service Managers	143,040	1.39	25.58
Assorted Managers	70,460	0.68	36.35
Others (bakers, cleaners, ...)	286,600	3.59	13.62

Based on data from the Occupational Employment Statistics database.
The restaurant sector is defined as NAICS 7225 from the 2012 edition of the NAICS classification.



Source: CPS ORG. The left panel shows the share of workers that earned less than the upcoming minimum wage (6 months ahead) by age group. The right panel calculates those shares for hypothetical minimum wages up to $15 using 2017 wages.

Figure A1: Share of affected workers by age group. Historical and projected.

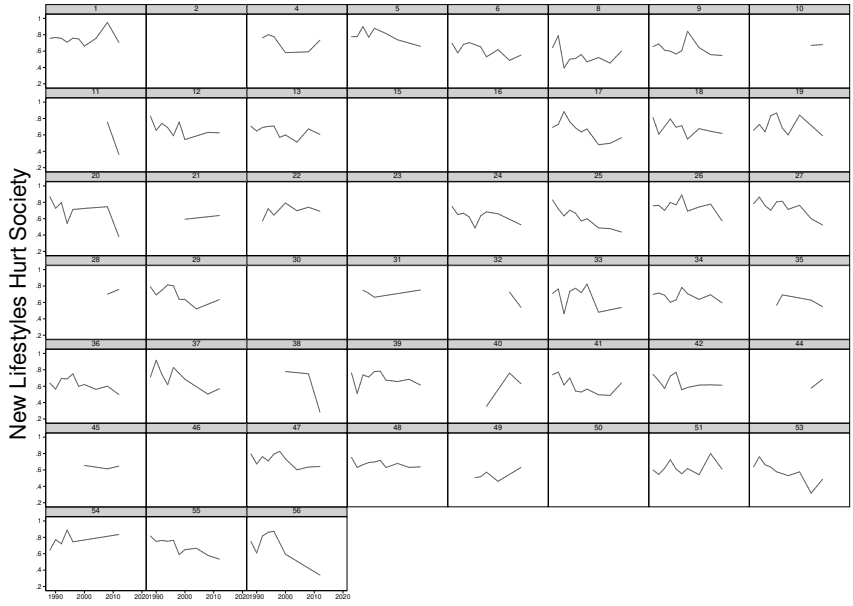


Figure A2: Example of one of the instruments.

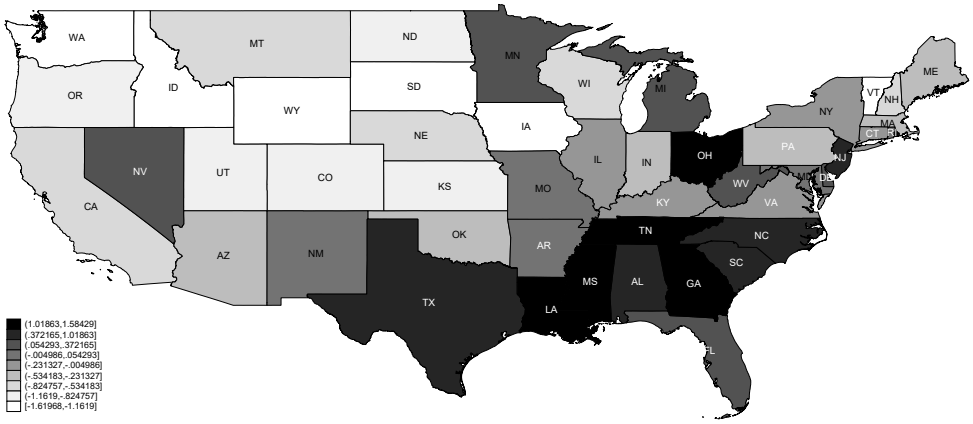


Figure A3: Loadings of the first factor (λ_1) averaged by state.

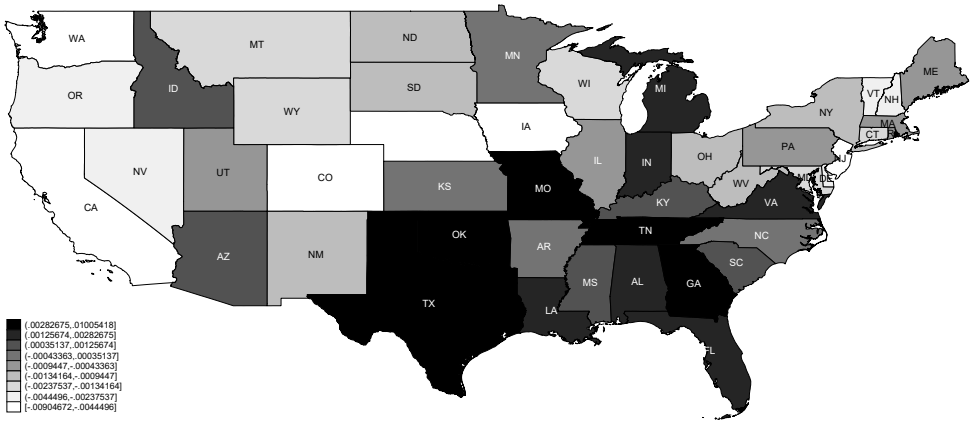


Figure A4: Slopes of the trend in political ideology over time by state.

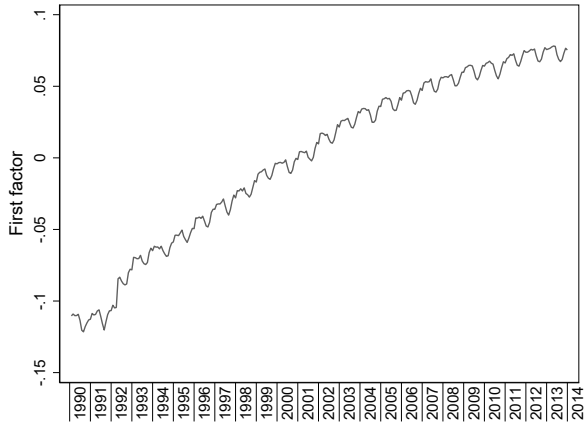


Figure A5: Evolution of the first factor (F_1) over time.

Table A3: Replication of Meer and West (2016)

Number of Years	1	2	3	4	5	6	7	8
				A: Original estimates				
Employment Elasticity	-0.020	-0.039*	-0.050**	-0.051**	-0.046*	-0.049**	-0.047**	-0.047**
	(0.017)	(0.021)	(0.023)	(0.024)	(0.024)	(0.021)	(0.020)	(0.022)
Observations	1734	1683	1632	1581	1530	1479	1428	1377
				B: Original estimate, political ideology sample				
Employment Elasticity	0.005	-0.003	-0.018	-0.035	-0.041*	-0.043*	-0.049**	-0.040*
	(0.014)	(0.017)	(0.017)	(0.022)	(0.024)	(0.022)	(0.019)	(0.022)
				C: Estimate with political ideology, political ideology sample				
Employment Elasticity	0.005	-0.005	-0.019	-0.035	-0.039	-0.041*	-0.046**	-0.037
	(0.014)	(0.017)	(0.017)	(0.022)	(0.025)	(0.023)	(0.020)	(0.023)
				D: Estimate with political ideology, political ideology sample, with state fixed effects				
Employment Elasticity	0.006	-0.000	-0.008	-0.016	-0.011	-0.001	-0.006	0.004
	(0.014)	(0.017)	(0.018)	(0.025)	(0.027)	(0.026)	(0.022)	(0.021)
Observations	699	652	618	570	521	472	423	374

Standard errors in parentheses, errors are clustered at the state level. The column numbers correspond to the number of periods over which the long difference is taken. All columns include year-by-region time fixed effects, and state-specific time-varying controls: log population, the share aged 15-59, and log real gross state product per capita. Note copied from Meer and West (2016). *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: First and Second Stage of Instrumented 2FE regressions

		Ideology	Ideology	Ideology
1^{st} Stage	Abortion always allowed	0.189	0.193	0.184
		(0.296)	(0.299)	(0.371)
	Atheist	0.837***	0.839***	0.816***
		(0.259)	(0.257)	(0.259)
	Increase federal crime spending	0.532	0.535	0.342
		(0.327)	(0.328)	(0.347)
	Dislike military†	0.734	0.731	0.962
		(0.650)	(0.651)	(0.662)
	Dislike gays†	-0.782*	-0.787**	-0.523
		(0.402)	(0.401)	(0.391)
	Equal Rights gone too far	0.144	0.144	0.383
		(0.436)	(0.434)	(0.434)
	Like Big Business†	0.210	0.207	0.209
		(0.267)	(0.269)	(0.266)
	Govt should offer little support	0.801***	0.804***	0.725***
		(0.264)	(0.262)	(0.242)
	Blacks have not been discriminated	-0.395	-0.391	-0.263
		(0.257)	(0.257)	(0.254)
	New Lifestyles hurt society	0.588	0.591	0.646**
		(0.402)	(0.402)	(0.324)
	Minimum Wage (log)	-1.274**	-1.287**	-1.259***
		(0.504)	(0.511)	(0.479)
	Population (log)	0.032*	0.032**	0.039*
		(0.019)	(0.015)	(0.023)
	Total Private Employment (log)	-0.006		-0.087
		(0.156)		(0.182)
	Total Private Earnings (weekly, log)		0.000	
			(0.249)	
		Emp Rest	Earnings Rest	Emp Finance
2^{nd} Stage	Median political ideology of house	0.100***	-0.038***	0.047**
		(0.031)	(0.012)	(0.018)
	Minimum Wage (log)	0.044	0.169***	-0.019
		(0.067)	(0.038)	(0.051)
	Population (log)	0.045***	0.007***	0.053***
		(0.006)	(0.002)	(0.004)
	Total Private Employment (log)	0.575***		0.387***
		(0.055)		(0.045)
	Total Private Earnings (weekly, log)		0.183***	
			(0.027)	
	N	187092	62364	357552
	SW F-test pvalue	0.001	0.001	0.000
	SW χ^2-test pvalue	0.000	0.000	0.000

See notes of Tables 1.4 and 1.5. †Instruments marked with a dagger are based on thermometer questions. Respondents were classified as liking a particular group if they gave it a score larger than 50 out of 100. *Stars*: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix to Chapter 2

1 Sources of measurement error

There are multiple sources of measurement error in our vulnerability indicator. First, while the QCEW is based on a near census of employment, the CPS, CES and OES are based on surveys with limited samples. Second, in using state-level data we assume that the average number of hours worked per quarter is fairly uniform across counties within states. If high wage counties also offer more hours we would underestimate the differences in hourly wages and thus in affected shares. Third, we assume that the relation between weekly hours and quarterly hours has remained fairly constant between 1990 and 2017. Fourth, the quarterly hours from the CES are based on the leisure and hospitality sector, where 27% of employees are not active in the food services sector. Nevertheless, we believe the adjustment through the CES is worthwhile because hourly wages without it are implausibly low. Fifth, the

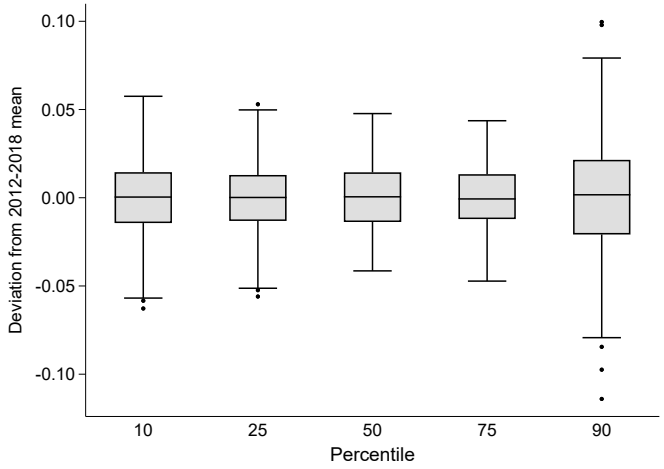


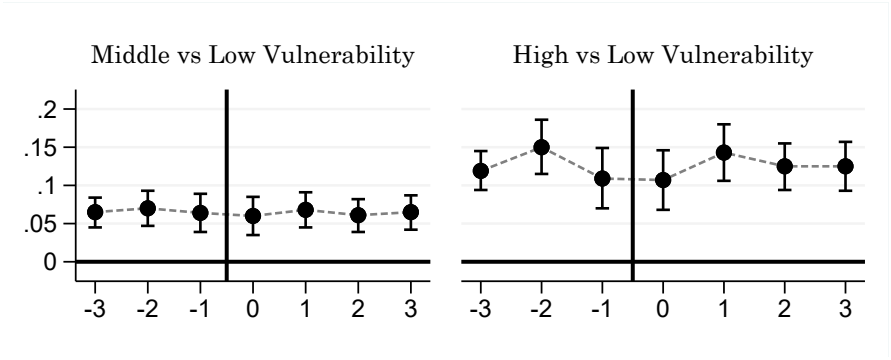
Figure B1: Deviations from mean percentage. Boxplot: box represents IQR, the whiskers the adjacent values, the dots the outside values.

wage distribution data is at the state level and is thus the sum of variation within counties and between counties. By applying it directly to the mean county-quarter level wage, we therefore overestimate the variability within counties. Moreover, we use the distribution of 2012 for all years as this is the earliest the data is available. We show in Figure B1 that at least post 2012 these shares do not move much, with 50% of observations lying within two percentage points of the mean. Nevertheless, combined with the need to linearly interpolate between quantile points, we do not feel confident interpreting the shares directly. I.e. we will not claim to estimate the impact of a 10% increase in affected share on the earnings and employment effect. Instead, our main conclusions are about differences between the three broad groups (low, medium and high vulnerability) and we use the earnings effects to validate our measure. Indeed, in Sections 2.5 and 2.6 we find that earnings effects are increasing in our measure, indicating that even if it would not capture absolute vulnerability, it at least provides a valid ordering of county vulnerability.

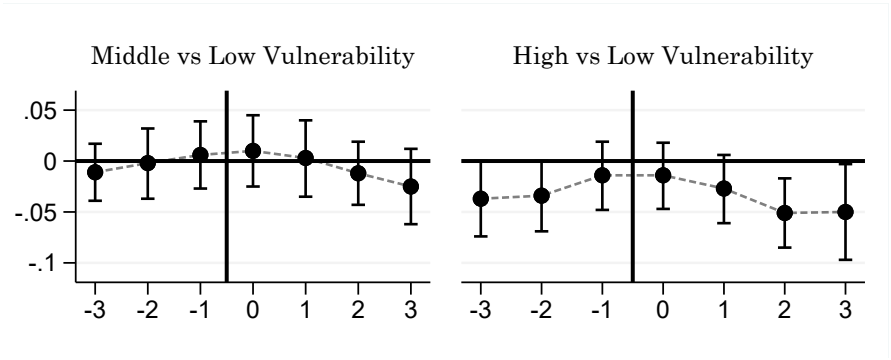
We turn to the 410 counties in the CPS for further validation. The CPS provides self-reported hourly wages which we can use to calculate the share of employees in the food services sector directly affected by a 10% minimum wage hike.[20] We calculate these shares per county-year and take the median value per county. We find averages of 33, 44 and 61% for respectively the low, medium and high vulnerability groups, suggesting that although our measure overestimates the variance between counties, it does capture the rank order.

[20]When hourly wages were not reported, we divided weekly wages by usual weekly hours. We exclude individuals reporting wages less than 75% of the current minimum wage.

2 Complete long differences



(a) Earnings



(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies. The markers show how the elasticities differ relative to the low vulnerability counties. Exact numbers are available in Table B18 in the Appendix.

Figure B2: Elasticity with respect to the minimum wage, complete long differences (CLD).

In this section we compare our local projection approach to the complete long differences method of Meer and West (2016), henceforth the CLD method. On first sight they appear very similar (Equation 11 illustrates the CLD method). Indeed, the only difference is that where they take long differences of all variables (Δ_h), we stick to *first* differences for the minimum wage variable (Δmw_{st}).[21] This small discrepancy fundamentally changes the interpretation of the minimum wage coefficient. In Figure B2 we show earnings and employment elasticities for the CLD approach. The earnings elasticities in particular are now essentially constant over time. For middle

[21] Note that we abstract from the rest of our methodology here, e.g. the split into groups and the state-time dummies.

and highly vulnerable groups they hover around respectively +0.07 (se 0.01) and +0.13 (0.02), both in the pre- and the posttreatment period.

$$\Delta_h emp_{i,t} = \sum_{j=1}^{3} \beta_{h,j} \Delta_h mw_{st} \times I(g_c = j) + \delta_{1,h} \Delta_h emp_{i,t}^{TOT} + \delta_{2,h} \Delta_h pop_{i,t} + \theta_{iq} + \phi_{st} + \epsilon_{iht} \tag{11}$$

The identical earnings elasticities in the pre- and posttreatment period would normally be cause for concern, but in this case they cannot be interpreted as such. For in the CLD method you cannot interpret the horizons as windows around minimum wage changes. Rather, larger horizons show how estimates change as you move from first differences to longer and longer differences (up to 8 year differences in Meer and West, 2016). In our setting, the distinction between pre and post effects of the minimum wage is a key part of our validation exercise. Thus, we need the horizons to reflect the time relative to the minimum wage change. On the other hand, Meer and West (2016) investigate differences between short-run and long-run estimates of minimum wage effects, which means in their context the rolling window interpretation of horizons in the CLD approach is more meaningful. Moreover, the local projections approach ignores multiple changes within the horizon, whereas the CLD method takes these into account. This is less relevant in our short window, with only 13% of minimum wage changes occurring within three quarters of the previous one, than in the eight year horizons of Meer and West (2016).

	Data		Δ_0	← Pre			Post →			
				Δ_{-3}	Δ_{-2}	Δ_{-1}	Δ_0	Δ_1	Δ_2	Δ_3
t	MW	W	MW	W	W	W	W	W	W	W
-4	0	10	.	.	.	.	.	.	.	.
-3	0	10	0	.	.	.	0	0	0	2
-2	0	10	0	.	.	0	0	0	2	2
-1	0	10	0	.	0	0	0	2	2	2
0	1	12	1	0	0	0	2	2	2	2
1	1	12	0	2	2	2	0	0	0	0
2	1	12	0	2	2	0	0	0	0	.
3	1	12	0	2	0	0	0	0	.	.
4	1	12	0	0	0	0	0	.	.	.

Table B1: Example of our data

In Tables B1 and B2 we illustrate in detail why the horizons in the complete long differences approach of Meer and West (2016) have a different interpretation than in our local projection exercises. The two tables show hypothetical data extracts. Table B1 shows the situation in our local projection framework. Each line represents the observation in time t for a particular county. In our example, the minimum wage MW increases from zero to one at time $t = 0$ and raises earnings W from ten to twelve

(immediately and permanently). The only non-zero value for the minimum wage change variable is at $t = 0$ (black font). This observation affects seven regressions. The numbers in the last columns of the table represent the value of the dependent variable, which is the long difference of earnings for each horizon. We see that it is zero in the pretreatment period ($\Delta_{-3}W$, $\Delta_{-2}W$, $\Delta_{-1}W$) and two in the post period ($\Delta_0 W$, $\Delta_1 W$, $\Delta_2 W$, $\Delta_3 W$), which is exactly what we would expect.

			⟵ Pre						Post ⟶							
	Data		Δ_{-3}		Δ_{-2}		Δ_{-1}		Δ_0		Δ_1		Δ_2		Δ_3	
t	MW	W	MW	W	MW	W	MW	W	MW	W	MW	W	MW	W	MW	W
-4	0	10	.	.	.	.	.	.	.	.	.	.	.	.	.	.
-3	0	10	.	.	.	.	.	.	0	0	0	0	0	0	1	2
-2	0	10	.	.	.	.	0	0	0	0	0	0	1	2	1	2
-1	0	10	.	.	0	0	0	0	0	0	1	2	1	2	1	2
0	1	12	0	0	0	0	0	0	1	2	1	2	1	2	1	2
1	1	12	1	2	1	2	1	2	0	0	0	0	0	0	0	0
2	1	12	1	2	1	2	0	0	0	0	0	0	0	0	.	.
3	1	12	1	2	0	0	0	0	0	0	0	0	.	.	.	.
4	1	12	0	0	0	0	0	0	0	0	.	.	.	.	.	.

Table B2: Example of data in the style of Meer and West (2016)

Table B2 depicts the same situation in the CLD framework. The first three data columns (t, MW, W) are exactly the same. The last columns now show long differences for both earnings W and the minimum wage MW. Observations with non-zero minimum wage changes are in black, others in grey. In this context, the minimum wage shock occurring at $t = 0$ shows up in all observations, albeit in different columns/horizons. The reason is that the horizons are not defined relative to a minimum wage shock. Instead, they are the lengths of a rolling window. Thus, one could think of moving across horizon lengths in the CLD framework as showing how results change as you go from first differences to longer and longer differences.

Take regressions of horizon $h = 2$ and $h = -3$. Both chop up the data in exactly the same blocks of four time periods and estimate how the cumulative change of the minimum wage over the entire block is related to the cumulative change in earnings.[22] As they work with the same blocks, they also end up with identical coefficient estimates, which explains the similarity of the supposedly pre- and posttreatment results.[23,24]

[22] The reason $h = 2$ is identical to $h = -3$ and not $h = -2$ is that we defined the contemporaneous effect as $h = 0$ rather than $h = 1$.

[23] Monte Carlo results demonstrating this are available on request.

[24] In a finite sample you still get different results because in the $h = -3$ regressions, everything is shifted forwards three time periods which affects the beginning and end of the sample.

3 QWI

The QWI is publicly available and aggregates information from Longitudinal Employer Household Dynamics (LEHD) data, a restricted matched employer-employee micro dataset. It gained prominence in the minimum wage debate when Dube, Lester and Reich (2016) found that even though minimum wages do not affect employment stocks, they do affect employment flows (hiring and separation).[25] We use employment and earnings as measured at the start of the quarter and retain distorted observations in our main results. We use all years available in the QWI. The results are robust to excluding distorted observations and starting the sample in 2000, when most states had joined the QWI program.[26]

We focus on the food services sector (NAICS 722) and obtain data through the sex-age, sex-education and race-ethnicity endpoints.[27] We define the hiring rate as the number of new hires divided by employment at the start of the quarter, same for the separation rate. The turnover rate is the average of the two (Dube, Lester and Reich, 2016).[28] All variables enter the regressions in log-form. Summary statistics are presented in Table B3, a summary of all results is presented in Tables B4-B6 and the main results are discussed in Section 2.6.3.

4 Policy simulations without unaffected group assumption

In the policy simulations in the main body we assumed that the low vulnerability group was entirely unaffected by changes in the minimum wage. We reverse this assumption in Table B7 where we assume there is no state-level endogeneity and turn to the models without state-time dummies as the source for our simulations. The scenarios are identical to those in the main table.

There are two major differences in terms of outcomes. First, the ratio of earnings gains to employment losses doubles, going from factor two to factor four. Second, the absolute size of the effects increases, especially for earnings. E.g. in Scenario B, the employment losses almost double to -1.3%, whereas the earnings gains almost quadruple to 6.2%. The cause of these differences is twofold, first that the earnings coefficients from the models without the state-time dummies are larger for all three groups while employment estimates remain largely stable. Second, where in the main table all low vulnerability counties contributed zero changes, they now bring small earnings gains and (smaller) employment losses.

[25] For more information on the QWI dataset, we refer to Dube, Lester and Reich (2016) and Abowd et al. (2009).

[26] There are minor differences for certain outcomes, those are highlighted in footnotes.

[27] We wrote a powershell script to facilitate downloading this data, please contact the author if interested.

[28] We are grateful to the authors for providing access to their code.

Table B3: QWI: Summary Statistics

	Sector 722		All except 722	
	Mean	SD	Mean	SD
Monthly Earnings ($)	859	290	2620	898
Employment	2657	7870	28635	90248
Hiring rate	0.41	0.27	0.22	0.11
Separation rate	0.39	0.15	0.21	0.09
Turnover rate	0.40	0.20	0.21	0.09
Fractions				
- Gender: men	0.46		0.53	
- Age: 14-18	0.14		0.02	
- Age: 19-21	0.15		0.05	
- Age: 22-64	0.68		0.89	
- Education: < HS	0.22		0.13	
- Education: = HS	0.34		0.29	
- Education: > HS	0.45		0.59	
- Race: white	0.84		0.86	
- Race: black	0.15		0.13	

Earnings are in nominal dollars. Hiring, separation and turnover rates are quarterly. Includes distorted and pre-2000 data. HS stands for high school. Sample runs from 1990-2017, though not all states enter at the same time.

Table B4: QWI result summary table: Impact after three quarters

After three quarters	W	E	H	S	T
All	0.08/0.12	-0.01/-0.01	-0.03/0.13	-0.00/0.02	-0.02/0.06
Sex: Men	0.09/0.14	-0.04/-0.04	-0.05/-0.02	-0.06/-0.09	-0.06/-0.07
Sex: Women	0.06/0.10	-0.01/0.01	-0.03/0.11	0.01/0.07	-0.01/0.08
Age: 14-18	0.11/0.17	-0.02/-0.03	-0.07/0.08	-0.02/0.02	-0.05/0.04
Age: 19-21	0.12/0.20	0.05/0.12	-0.12/-0.04	-0.12/-0.05	-0.12/-0.08
Age: 22-64	0.05/0.11	0.00/-0.00	-0.03/-0.04	0.01/-0.03	-0.01/-0.02
Age: 65+	0.07/0.08	-0.07/-0.10	0.06/-0.42	0.01/0.01	0.03/-0.17
Edu: < HS	0.06/0.08	-0.00/-0.00	-0.02/-0.01	-0.01/-0.04	-0.02/-0.00
Edu: = HS	0.07/0.09	-0.00/-0.03	-0.04/-0.00	-0.02/-0.03	-0.03/-0.04
Edu: > HS	0.04/0.07	-0.01/0.00	-0.05/-0.07	-0.01/-0.05	-0.03/-0.07
Race: Black	0.11/0.16	-0.07/0.01	0.11/0.05	0.02/0.07	0.06/0.02
Race: White	0.07/0.10	-0.00/0.01	-0.05/0.12	0.01/0.05	-0.02/0.07
Race: Other	-0.02/0.31	0.01/0.04	-0.08/-0.38	-0.06/-0.05	-0.09/-0.24

The numbers before/after the slash report respectively the relative elasticity of middle and high vulnerability counties (compared to low vulnerability counties). The columns refer to earnings (W), employment (E), hiring rate (H), separation rate (S) and turnover rate (T), all in logs. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Middle and high group estimates derive from separate regressions. Standard errors clustered at the state level. Results in grey are not significant at 5%.

Table B5: QWI result summary table: Contemporaneous impact

Contemporaneous	W	E	H	S	T
All	0.07/0.08	0.01/-0.02	-0.01/-0.02	-0.06/-0.15	-0.04/-0.09
Sex: Men	0.08/0.12	-0.02/-0.09	0.03/0.02	-0.06/-0.16	-0.02/-0.08
Sex: Women	0.05/0.06	0.02/0.01	-0.03/-0.09	-0.07/-0.15	-0.06/-0.15
Age: 14-18	0.07/0.09	-0.01/-0.09	0.02/0.00	-0.01/-0.06	-0.01/-0.04
Age: 19-21	0.03/0.06	0.01/0.05	0.01/-0.11	-0.10/-0.18	-0.06/-0.14
Age: 22-64	0.06/0.08	0.01/-0.01	0.03/-0.10	-0.06/-0.14	-0.02/-0.13
Age: 65+	0.00/0.08	-0.07/-0.07	0.08/-0.07	-0.17/-0.09	-0.01/-0.06
Edu: < HS	0.08/0.06	-0.01/-0.02	0.09/-0.05	-0.02/-0.17	0.02/-0.07
Edu: = HS	0.07/0.08	0.01/-0.02	0.07/-0.04	-0.04/-0.18	0.01/-0.12
Edu: > HS	0.06/0.09	0.01/-0.00	0.02/-0.14	-0.07/-0.12	-0.03/-0.13
Race: Black	0.07/0.10	-0.06/-0.02	0.11/-0.21	-0.01/-0.12	0.06/-0.18
Race: White	0.06/0.09	0.02/-0.02	-0.01/0.00	-0.06/-0.13	-0.04/-0.07
Race: Other	-0.05/0.08	-0.00/0.01	0.10/-0.35	-0.08/-0.35	0.01/-0.30

See Table B4.

Table B6: QWI result summary table: Impact three quarters before

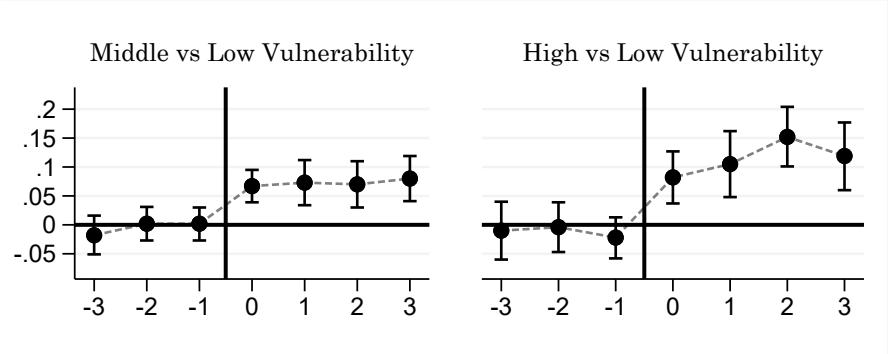
Three quarters before	W	E	H	S	T
All	-0.02/-0.01	0.02/0.02	-0.05/0.01	0.03/0.12	-0.01/0.07
Sex: Men	-0.01/-0.03	0.02/0.05	-0.07/-0.09	-0.01/0.11	-0.04/0.03
Sex: Women	-0.00/0.00	0.03/0.01	-0.07/0.01	0.06/0.12	-0.01/0.06
Age: 14-18	0.00/-0.02	0.06/0.10	-0.08/-0.03	-0.03/0.03	-0.04/-0.02
Age: 19-21	0.05/0.06	0.07/0.06	-0.06/0.04	0.02/0.10	-0.01/0.10
Age: 22-64	-0.00/0.01	0.01/0.03	-0.08/0.00	0.01/0.08	-0.03/0.04
Age: 65+	0.02/-0.01	0.00/0.06	-0.05/0.25	0.07/0.02	0.04/0.20
Edu: < HS	-0.02/0.01	0.01/0.01	-0.06/-0.04	0.03/0.12	-0.01/0.01
Edu: = HS	-0.02/-0.02	0.02/0.02	-0.06/-0.02	0.02/0.12	-0.02/0.06
Edu: > HS	-0.06/0.02	0.03/0.00	-0.07/0.09	0.00/0.07	-0.03/0.10
Race: Black	-0.04/0.01	0.02/-0.08	-0.10/0.23	0.02/0.17	-0.04/0.22
Race: White	-0.01/-0.02	0.03/0.04	-0.06/-0.03	0.02/0.09	-0.03/0.02
Race: Other	0.14/-0.06	0.05/0.05	-0.26/0.49	-0.04/0.14	-0.13/0.30

See Table B4.

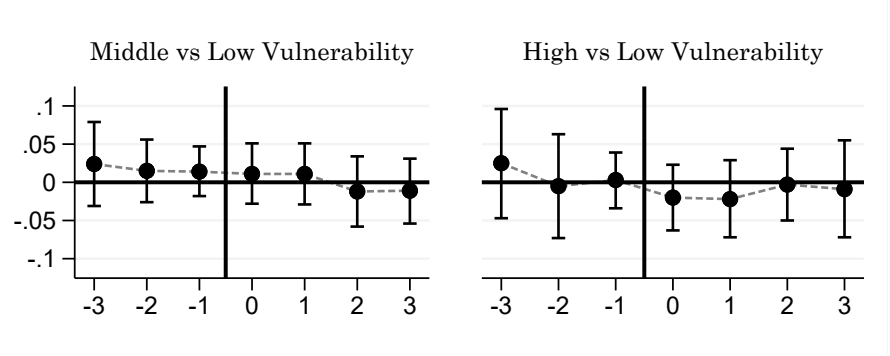
Table B7: Policy Simulations

	Δ Employment (#)	Δ Employment (%)	Δ Earnings ($)	Δ Earnings (%)
Food Services				
Scenario A	-27194	-0.2	3.3	0.9
10%, fixed	[-74950, 21024]	[-0.7, 0.2]	[1.9, 4.6]	[0.5, 1.3]
Scenario B	-153006	-1.3	18.6	5.1
15$, fixed	[-415307, 117919]	[-3.6, 1.0]	[10.9, 26.4]	[3.0, 7.2]
Scenario C	-306169	-2.7	41.9	11.5
15$, adjusted	[-635054, 27674]	[-5.6, 0.2]	[29.6, 54.6]	[8.1, 15.0]
Food Services Employment 2017: 11.4M Food Services Weekly Earnings 2017: $364				

See notes in Table 2.5.



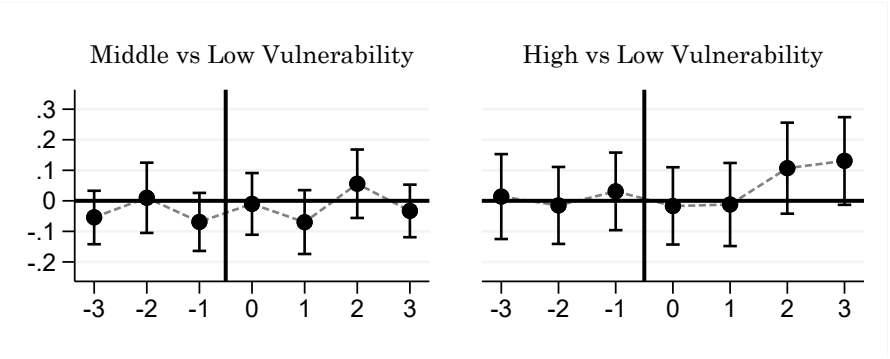
(a) Earnings



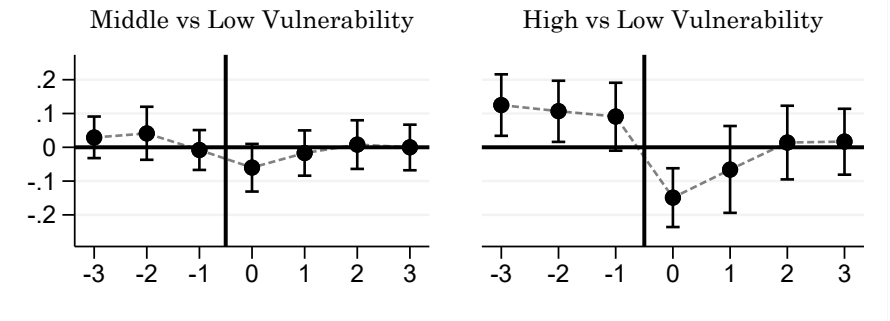
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

Figure B3: Earnings and Employment elasticities with respect to the minimum wage, QWI dataset.



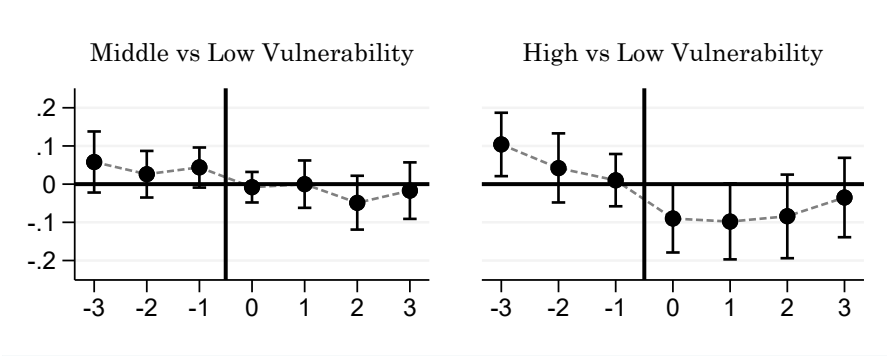
(a) Hiring rate



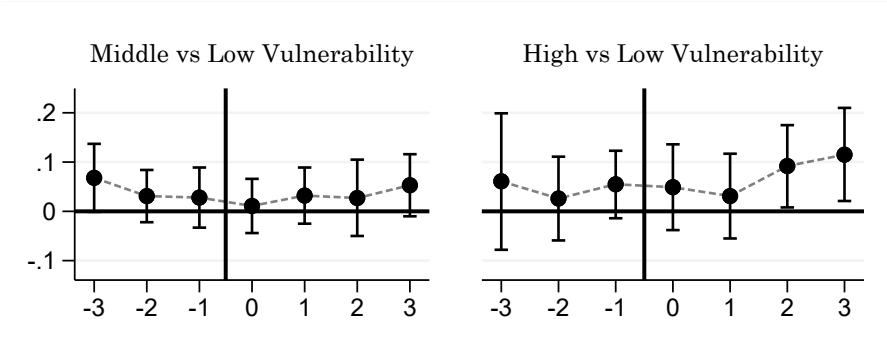
(b) Separation rate

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data. Hiring and separation rates are all new hires (separations) divided by employment at the start of the quarter.

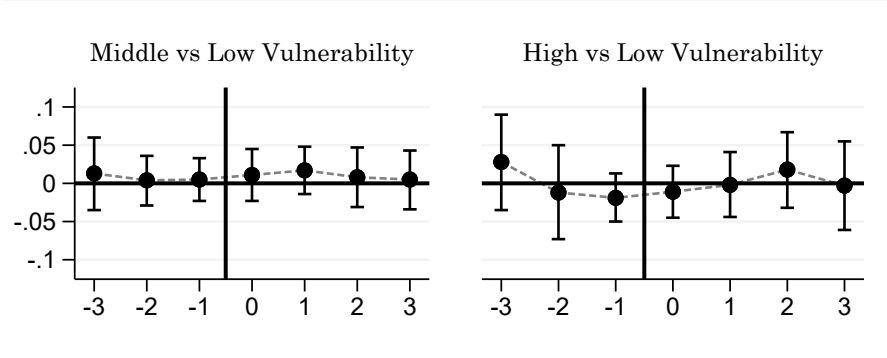
Figure B4: Hiring and separation elasticities with respect to the minimum wage, QWI dataset.



(a) Employment: age 14-18



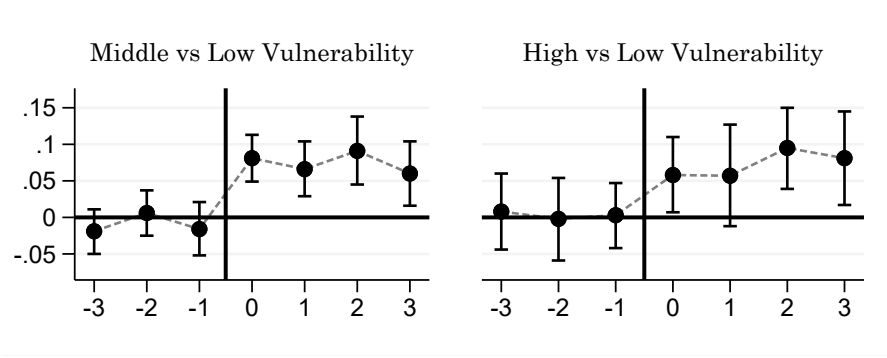
(b) Employment: age 19-21



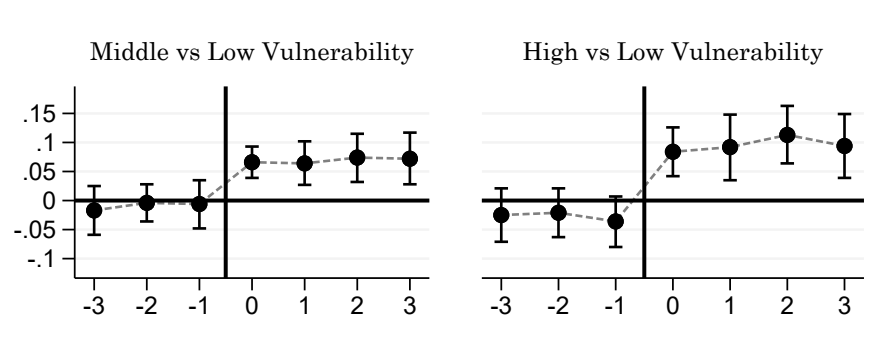
(c) Employment: age 22-64

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

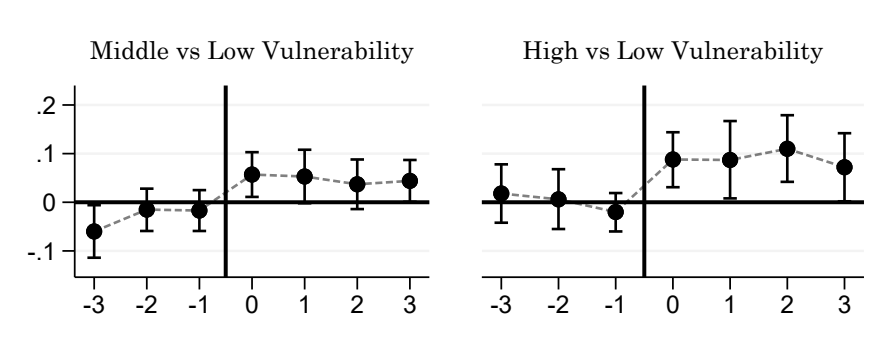
Figure B5: Employment elasticities with respect to the minimum wage, QWI dataset, by age.



(a) Earnings: less than highschool



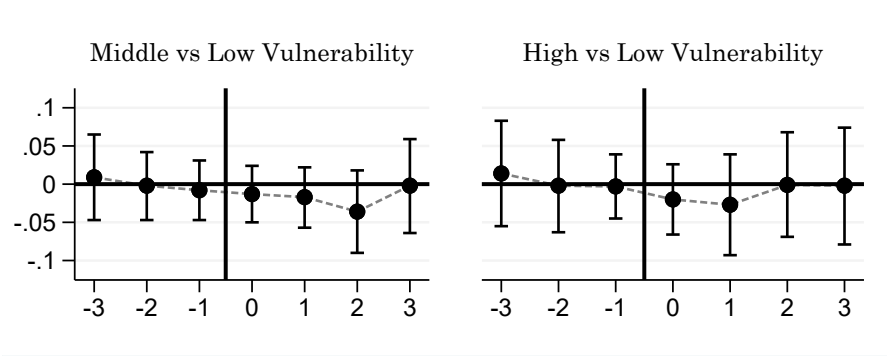
(b) Earnings: highschool



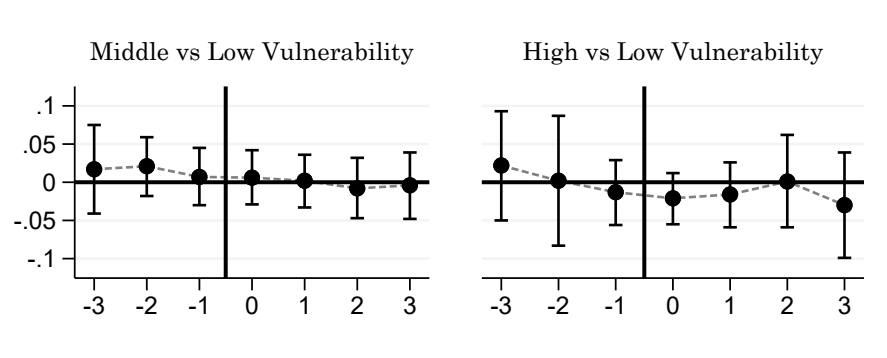
(c) Earnings: some college or more

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

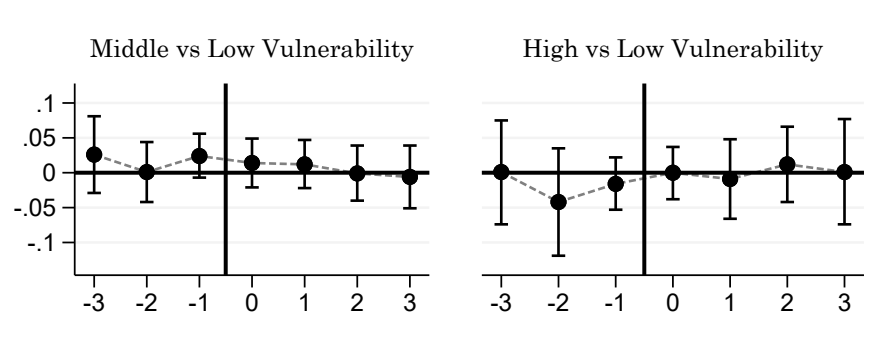
Figure B6: Earnings elasticities with respect to the minimum wage, QWI dataset, by education.



(a) Employment: less than highschool



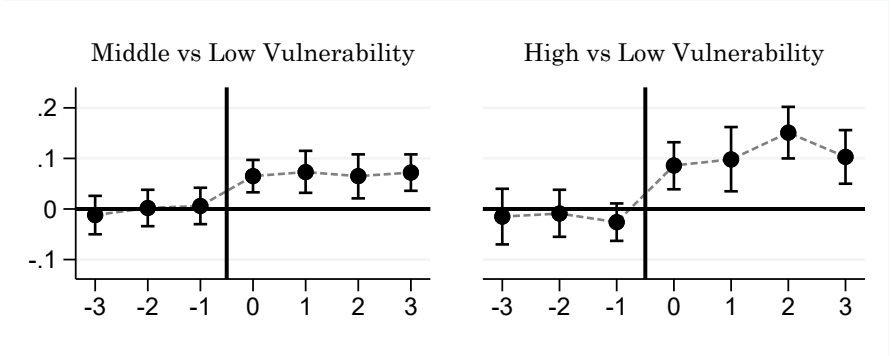
(b) Employment: highschool



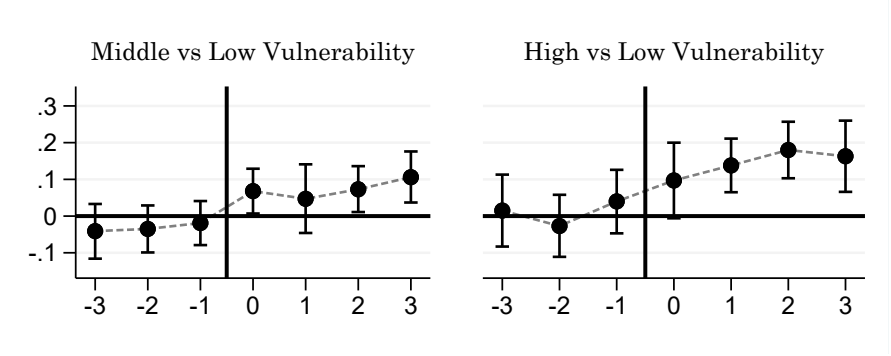
(c) Employment: some college or more

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

Figure B7: Employment elasticities with respect to the minimum wage, QWI dataset, by education.



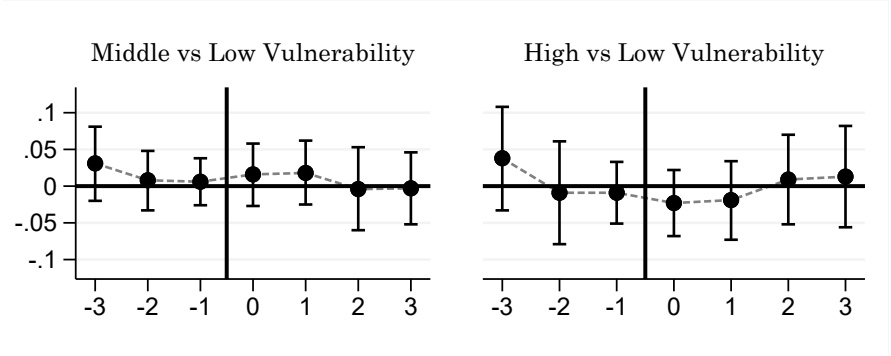
(a) Earnings: identify as white



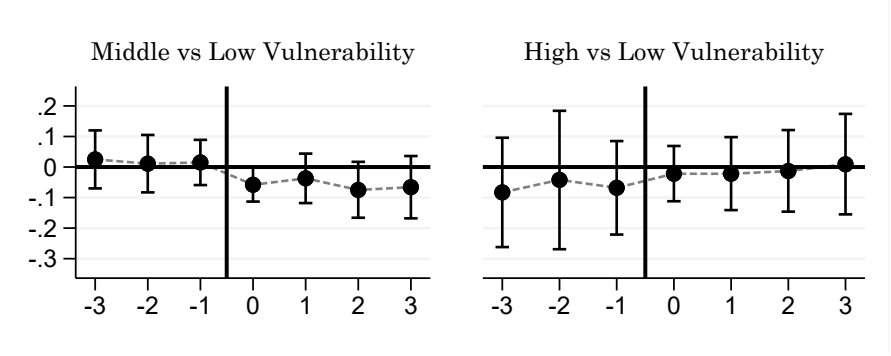
(b) Earnings: identify as black

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

Figure B8: Earnings elasticities with respect to the minimum wage, QWI dataset, by race.



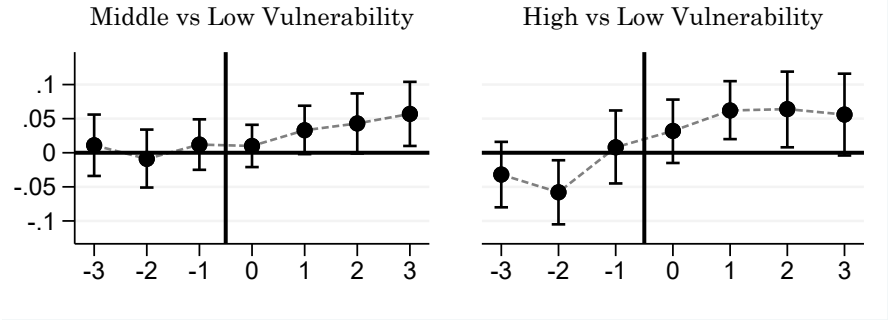
(a) Employment: identify as white



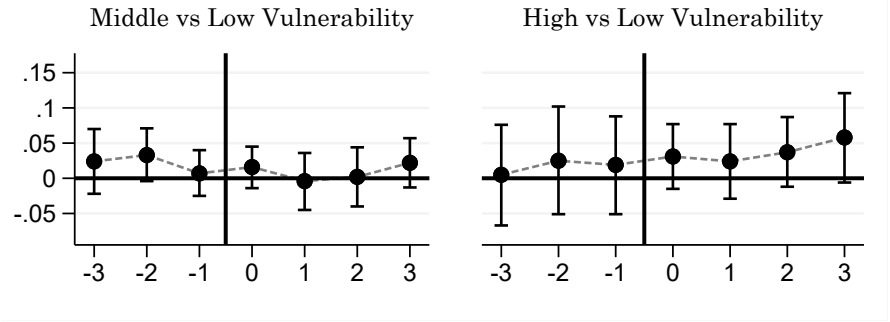
(b) Employment: identify as black

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

Figure B9: Employment elasticities with respect to the minimum wage, QWI dataset, by race.



(a) Earnings



(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. These regressions include controls for changes in adult employment and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request. Based on QWI data.

Figure B10: Earnings and Employment elasticities with respect to the minimum wage, QWI dataset, young adults (19-21) from all industries.

5 Tables

Table B8: Elasticity with respect to the minimum wage - High vs Low Vulnerability

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
High Vulnerability	-0.005	-0.037	0.013	0.107	0.151	0.097	0.144
	(0.016)	(0.027)	(0.015)	(0.019)	(0.023)	(0.019)	(0.021)
B: Employment	-3	-2	-1	0	1	2	3
High Vulnerability	0.017	-0.025	-0.012	-0.014	-0.026	-0.040	-0.054
	(0.029)	(0.030)	(0.019)	(0.016)	(0.022)	(0.024)	(0.027)
N	131090	131090	131090	131090	131090	131090	131090

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. See Figure 2.7.

Table B9: Elasticity with respect to the minimum wage - Sparse model

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
Low Vulnerability	-0.040	-0.010	-0.021	0.080	0.086	0.103	0.060
	(0.017)	(0.016)	(0.017)	(0.020)	(0.018)	(0.023)	(0.019)
High Vulnerability	-0.039	-0.044	0.004	0.188	0.236	0.204	0.204
	(0.015)	(0.018)	(0.014)	(0.019)	(0.022)	(0.026)	(0.028)
B: Employment	-3	-2	-1	0	1	2	3
Low Vulnerability	0.016	0.021	0.008	-0.017	-0.021	-0.024	-0.020
	(0.022)	(0.023)	(0.015)	(0.016)	(0.018)	(0.020)	(0.023)
High Vulnerability	0.045	0.015	0.015	-0.040	-0.039	-0.051	-0.049
	(0.032)	(0.017)	(0.022)	(0.018)	(0.017)	(0.023)	(0.027)
N	131264	131264	131264	131264	131264	131264	131264

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Time dummies not interacted by state. The coefficients show total elasticities (not relative to some other group). Low and high group estimates are jointly estimated. See Figure 2.8.

Table B10: Elasticity with respect to the Minimum Wage - Saturated model, greyzone vs the other groups.

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
Middle vs Low	-0.001	-0.029	-0.015	0.060	0.082	0.057	0.081
	(0.014)	(0.014)	(0.014)	(0.012)	(0.012)	(0.013)	(0.012)
Middle vs High	0.001	0.006	-0.031	-0.037	-0.052	-0.031	-0.043
	(0.013)	(0.017)	(0.012)	(0.016)	(0.018)	(0.014)	(0.018)
B: Employment	-3	-2	-1	0	1	2	3
Middle vs Low	-0.016	-0.015	-0.005	0.010	0.001	-0.023	-0.038
	(0.024)	(0.025)	(0.013)	(0.017)	(0.019)	(0.014)	(0.015)
Middle vs High	-0.025	0.006	0.010	0.018	0.025	0.001	-0.002
	(0.032)	(0.025)	(0.018)	(0.019)	(0.016)	(0.021)	(0.024)
N Middle Vs Low	153096	153096	153096	153096	153096	153096	153096
N Middle Vs High	188376	188376	188376	188376	188376	188376	188376

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. Greyzone and High vulnerability estimates derive from separate regressions. See Figure 2.10.

Table B11: Elasticity with respect to the minimum wage - Continuous interaction

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
log MW # share^{MEDIAN}	-0.030	-0.047	0.039	0.156	0.216	0.135	0.201
	(0.031)	(0.040)	(0.023)	(0.035)	(0.038)	(0.039)	(0.043)
N	235943	235943	235943	235943	235943	235943	235943
B: Employment	-3	-2	-1	0	1	2	3
log MW # share^{MEDIAN}	0.010	-0.051	-0.032	-0.027	-0.028	0.019	-0.017
	(0.052)	(0.045)	(0.029)	(0.036)	(0.038)	(0.040)	(0.052)
N	236443	236443	236443	236443	236443	236443	236443

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies. The coefficients show how the elasticities move with the median share affected variable, the intercept is not identified. See Equation 2.2 and Figure 2.11.

Table B12: Elasticity with respect to the minimum wage - Controls interacted by county

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
High Group	-0.008	-0.043	0.012	0.109	0.158	0.089	0.135
	(0.018)	(0.027)	(0.015)	(0.017)	(0.023)	(0.017)	(0.023)
Middle Group	-0.006	-0.033	-0.018	0.060	0.088	0.057	0.082
	(0.014)	(0.014)	(0.015)	(0.012)	(0.012)	(0.014)	(0.013)
B: Employment	-3	-2	-1	0	1	2	3
High Group	0.018	-0.027	-0.011	-0.011	-0.023	-0.027	-0.039
	(0.028)	(0.025)	(0.019)	(0.017)	(0.022)	(0.025)	(0.028)
Middle Group	-0.018	-0.015	-0.006	0.009	0.003	-0.022	-0.038
	(0.024)	(0.022)	(0.014)	(0.016)	(0.017)	(0.015)	(0.015)
N High Group	131090	131090	131090	131090	131090	131090	131090
N Middle Group	153096	153096	153096	153096	153096	153096	153096

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. These are estimated separately per county. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Middle and high group estimates derive from separate regressions. See Figure B17.

Table B13: Elasticity with respect to the minimum wage - Controls interacted by county and quarter of year

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
Middle Group	-0.009	-0.027	-0.018	0.058	0.091	0.065	0.084
	(0.015)	(0.014)	(0.015)	(0.012)	(0.012)	(0.013)	(0.013)
High Group	-0.011	-0.037	0.021	0.110	0.165	0.096	0.137
	(0.020)	(0.028)	(0.016)	(0.018)	(0.025)	(0.019)	(0.022)
B: Employment	-3	-2	-1	0	1	2	3
Middle Group	-0.014	-0.014	-0.002	0.010	0.009	-0.028	-0.033
	(0.024)	(0.025)	(0.015)	(0.016)	(0.018)	(0.018)	(0.017)
High Group	0.013	-0.032	-0.008	-0.003	-0.016	-0.018	-0.031
	(0.028)	(0.029)	(0.019)	(0.018)	(0.024)	(0.026)	(0.034)
N Middle Group	153096	153096	153096	153096	153096	153096	153096
N High Group	131090	131090	131090	131090	131090	131090	131090

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. These are estimated separately per county and quarter of year. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Middle and high group estimates derive from separate regressions. See Figure B18.

Table B14: Elasticity with respect to the minimum wage - Variable Group Membership

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
High Group	0.008	-0.041	0.013	0.091	0.135	0.083	0.125
	(0.021)	(0.023)	(0.017)	(0.021)	(0.020)	(0.020)	(0.026)
Middle Group	-0.017	-0.033	0.003	0.035	0.071	0.034	0.040
	(0.017)	(0.020)	(0.014)	(0.014)	(0.015)	(0.011)	(0.015)
B: Employment	-3	-2	-1	0	1	2	3
High Group	0.011	-0.042	-0.005	-0.004	0.012	0.015	0.008
	(0.031)	(0.033)	(0.024)	(0.018)	(0.025)	(0.024)	(0.024)
Middle Group	0.001	-0.010	0.002	0.017	0.017	-0.007	-0.024
	(0.019)	(0.023)	(0.013)	(0.013)	(0.013)	(0.015)	(0.016)
N High Group	144050	144050	144050	144050	144050	144050	144050
N Middle Group	148173	148173	148173	148173	148173	148173	148173

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent, changes in population and group membership dummies. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Middle and high group estimates derive from separate regressions. See Figure B14.

Table B15: Elasticity with respect to the minimum wage - Joint Regressions

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
Greyzone	-0.002	-0.026	-0.016	0.061	0.083	0.060	0.084
	(0.014)	(0.014)	(0.015)	(0.013)	(0.012)	(0.013)	(0.013)
High Vulnerability	-0.005	-0.035	0.013	0.101	0.140	0.090	0.132
	(0.016)	(0.025)	(0.015)	(0.018)	(0.023)	(0.019)	(0.021)
B: Employment	-3	-2	-1	0	1	2	3
Middle	-0.008	-0.012	-0.003	0.008	0.000	-0.024	-0.040
	(0.023)	(0.026)	(0.013)	(0.016)	(0.019)	(0.014)	(0.016)
High Vulnerability	0.016	-0.017	-0.010	-0.012	-0.024	-0.028	-0.041
	(0.028)	(0.029)	(0.019)	(0.016)	(0.021)	(0.022)	(0.025)
N	236443	236443	236443	236443	236443	236443	236443

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to the low vulnerability counties. Middle and high group estimates were jointly estimated.

Table B16: Sector Characteristics

Code	Description	Earnings	Employment	Locations	Observations
451	Recreational stores	230	546 541	58 805	124 416
452	General merchandise stores	237	2 569 922	32 888	170 320
722	Food services and drinking places	161	6 299 949	360 691	242 927
4471	Gasoline stations	217	906 668	95 654	276 784
4481	Clothing stores	227	963 181	84 346	122 963
4482	Shoe stores	227	224 505	24 864	69 533
4531	Florists	193	135 011	25 861	72 371
4533	Used merchandise stores	233	57 229	10 485	58 869
4854	School and employee bus transport	215	119 709	4 415	16 466
5617	Services to buildings and dwellings	238	1 177 959	112 579	184 575
6116	Other schools and instruction	236	95 644	13 542	67 204
6233	Community care facilities (elderly)	237	316 927	9 057	59 451
6244	Child day care services	175	391 203	35 256	134 049
7139	Other amusement and recreation	216	679 114	44 991	84 413
7212	Rv parks and recreational camps	219	35 726	5 135	17 950
8121	Personal care services	208	418 621	82 364	135 487
8134	Civic and social organizations	200	348 733	28 846	106 922
8141	Private households	179	265 783	214 394	149 353

Data from the QCEW. Earnings, employment and locations are reported for 1990. Earnings refer to average weekly earnings, locations to the number of establishments.

Table B17: Elasticity with respect to the minimum wage - Multiple sectors

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
High Group	-0.005	0.016	0.013	0.050	0.077	0.076	0.087
	(0.013)	(0.016)	(0.014)	(0.019)	(0.021)	(0.019)	(0.020)
Middle Group	0.009	-0.003	0.013	0.021	0.038	0.034	0.049
	(0.007)	(0.009)	(0.010)	(0.012)	(0.011)	(0.013)	(0.011)
B: Employment	-3	-2	-1	0	1	2	3
High Group	0.018	0.014	0.009	-0.004	-0.012	-0.009	-0.012
	(0.014)	(0.017)	(0.010)	(0.011)	(0.017)	(0.012)	(0.019)
Middle Group	0.005	0.010	0.006	0.004	-0.002	-0.005	-0.005
	(0.012)	(0.009)	(0.008)	(0.008)	(0.010)	(0.011)	(0.012)
N Sectors	18						
N Observations	1 628 730						

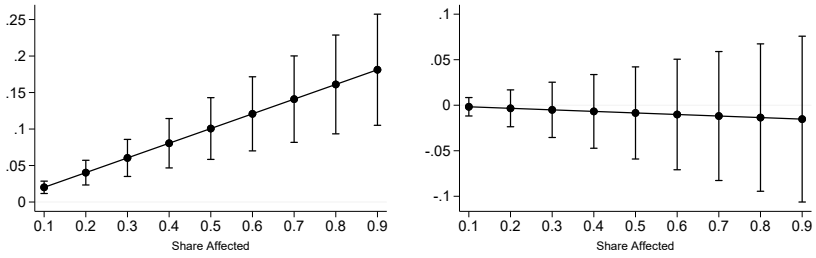
Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent, changes in population and group membership dummies. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Mean and confidence intervals over sectors were calculated using a weighted least squares estimation using the inverse variance as weight. Middle and high vulnerability estimates derive from separate regressions. See Figure 2.16.

Table B18: Elasticity with respect to the minimum wage - Long Differences

	⟵ Pre			Post ⟶			
A: Earnings	-3	-2	-1	0	1	2	3
High Group	0.119	0.150	0.109	0.107	0.143	0.125	0.125
	(0.013)	(0.018)	(0.020)	(0.019)	(0.018)	(0.015)	(0.016)
Middle Group	0.065	0.070	0.064	0.060	0.068	0.061	0.065
	(0.009)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)
B: Employment	-3	-2	-1	0	1	2	3
High Group	-0.037	-0.034	-0.014	-0.014	-0.027	-0.051	-0.050
	(0.018)	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)	(0.023)
Middle Group	-0.011	-0.002	0.006	0.010	0.003	-0.012	-0.025
	(0.014)	(0.017)	(0.016)	(0.017)	(0.019)	(0.015)	(0.018)
N Middle Group	153096	153096	153096	153096	153096	153096	153096
N High Group	131090	131090	131090	131090	131090	131090	131090

Standard errors in parentheses, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The coefficients show how the elasticities differ relative to low vulnerability counties. Middle and high group estimates derive from separate regressions. See Figure B2.

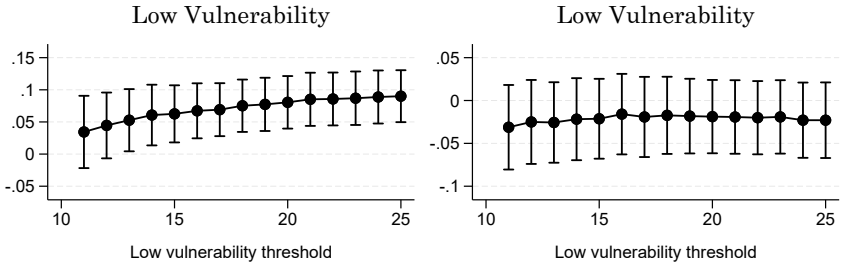
6 Figures



(a) Earnings **(b)** Employment

The markers show the relative size of minimum wage effects after three quarters for increasingly vulnerable counties. The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies.

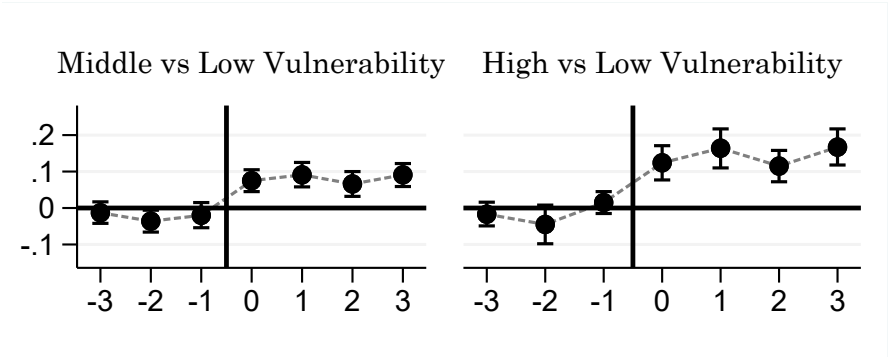
Figure B11: Elasticity with respect to the minimum wage, saturated model, continuous interaction, predicted minimum wage effects after three quarters.



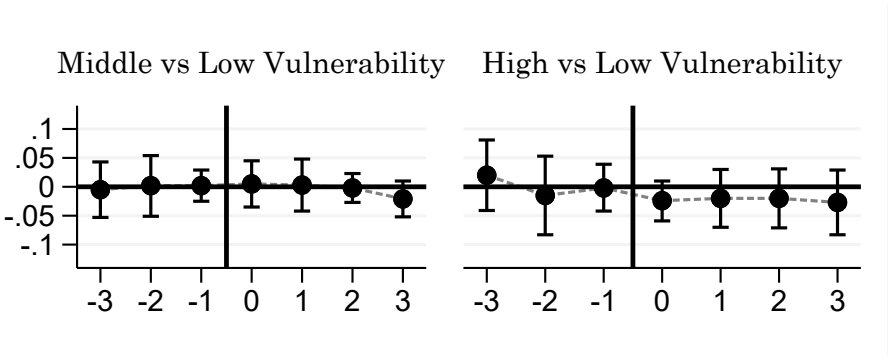
(a) Earnings **(b)** Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year to allow for local seasonality. Time dummies not interacted by state. The coefficients show total elasticities.

Figure B12: Elasticity with respect to the minimum wage, sparse model, bottom threshold variation, effect in low vulnerability group.



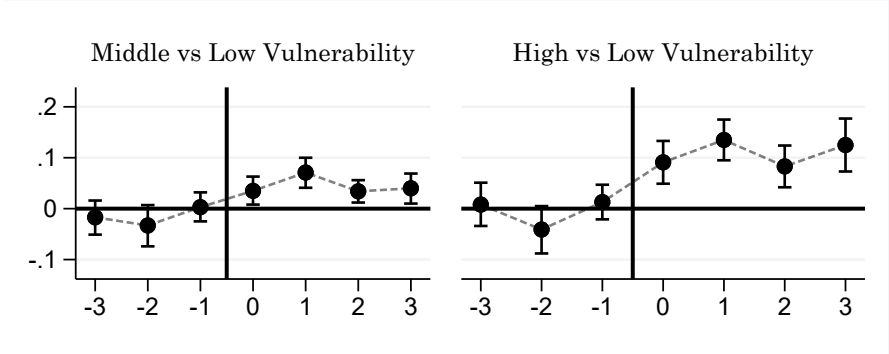
(a) Earnings



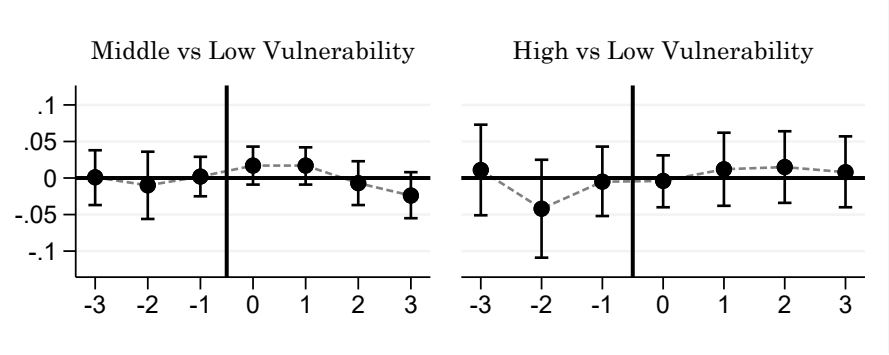
(b) Employment

The markers show how the elasticities differ relative to the low vulnerability group. Vulnerability based on mean rather than median over time. The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year to allow for local seasonality. Includes state-time dummies. Exact numbers are available on request.

Figure B13: Elasticity with respect to the minimum wage, saturated model, mean instead of median over time.



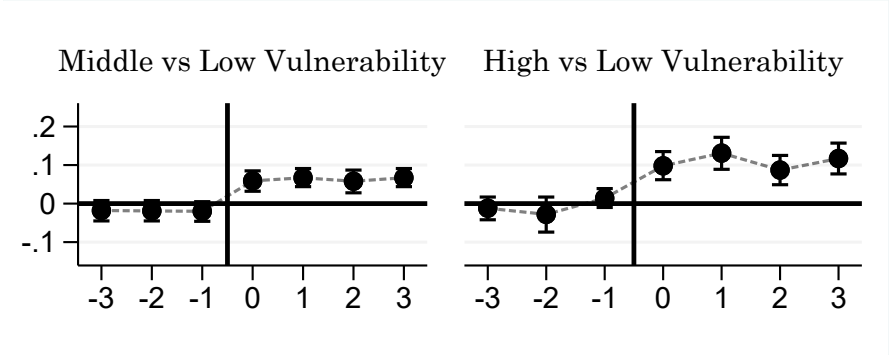
(a) Earnings



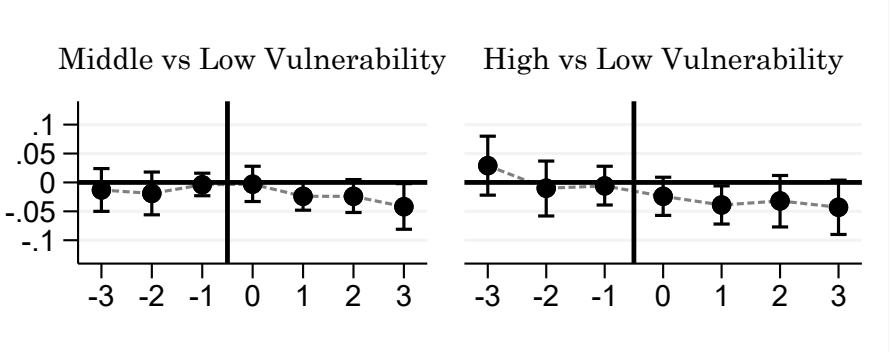
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent, changes in population and group membership dummies. County dummies are interacted by quarter of year. Includes state-time dummies and group dummies. The coefficients show how the elasticities differ relative to the low vulnerability counties.Exact numbers are available in Table B14 in the Appendix.

Figure B14: Elasticities with respect to the minimum wage, variable group membership.



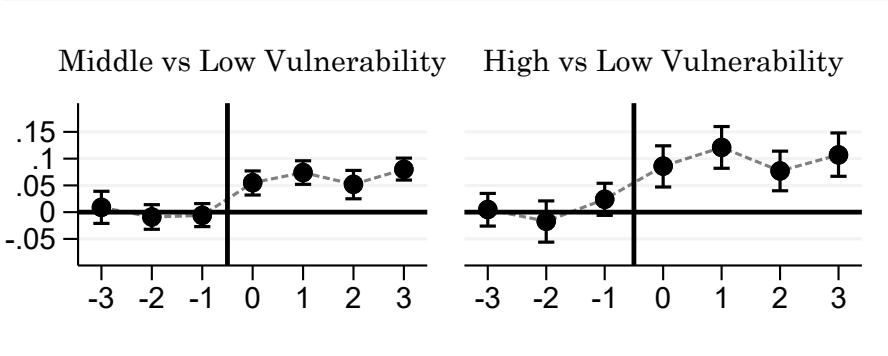
(a) Earnings



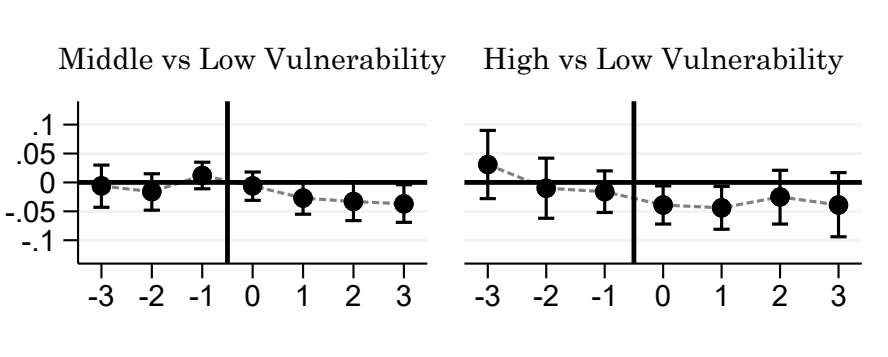
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request.

Figure B15: Elasticity with respect to the minimum wage, saturated model, raw shares.



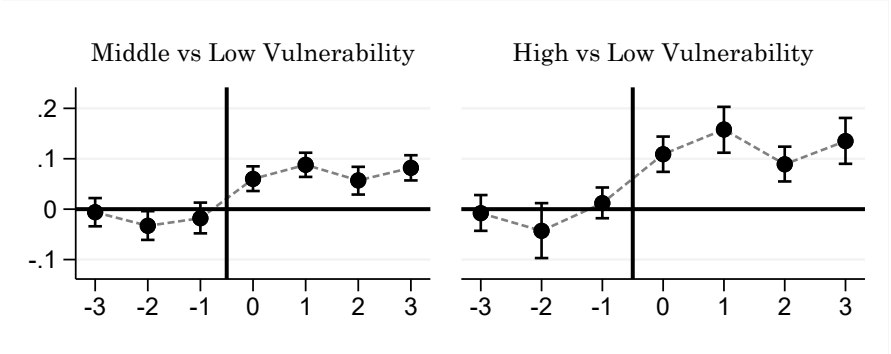
(a) Earnings



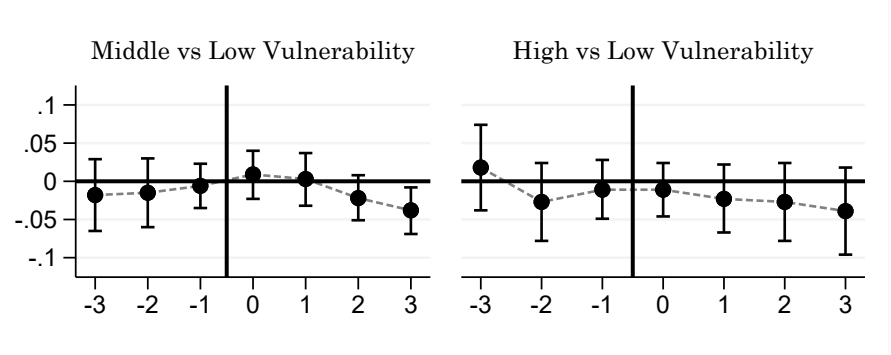
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Exact numbers are available on request.

Figure B16: Elasticity with respect to the minimum wage, saturated model, shares excluding MW episodes.



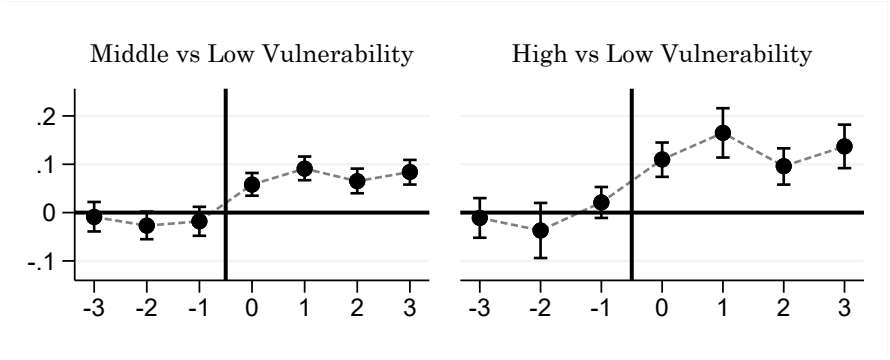
(a) Earnings



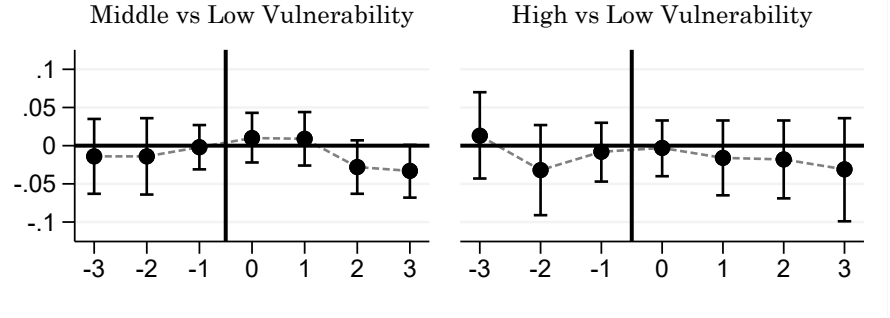
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. These are estimated separately per county. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to the low vulnerability counties. Exact numbers are available in Table B12 in the Appendix.

Figure B17: Elasticities with respect to the minimum wage, controls interacted by county.



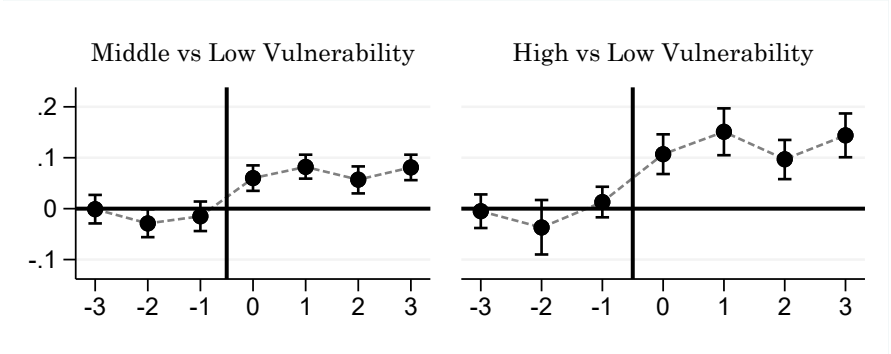
(a) Earnings



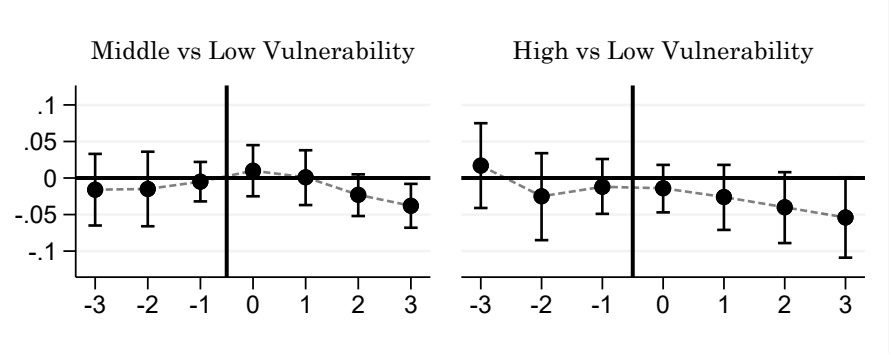
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. These controls are interacted by county and quarter of year. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to the low vulnerability counties. Exact numbers are available in Table B13 in the Appendix.

Figure B18: Elasticity with respect to the minimum wage, saturated model, county by quarter of year controls.



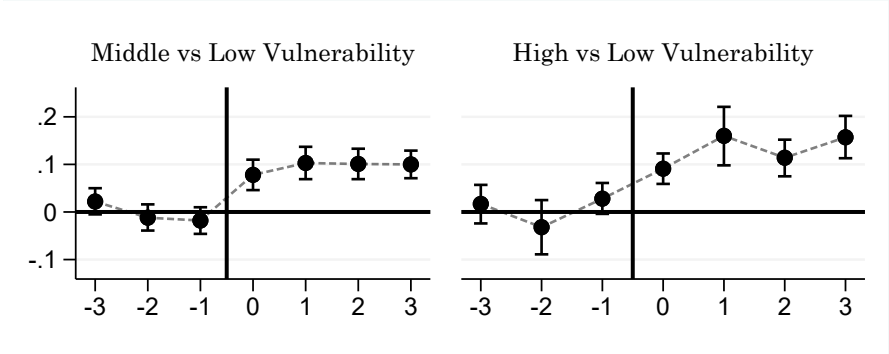
(a) Earnings



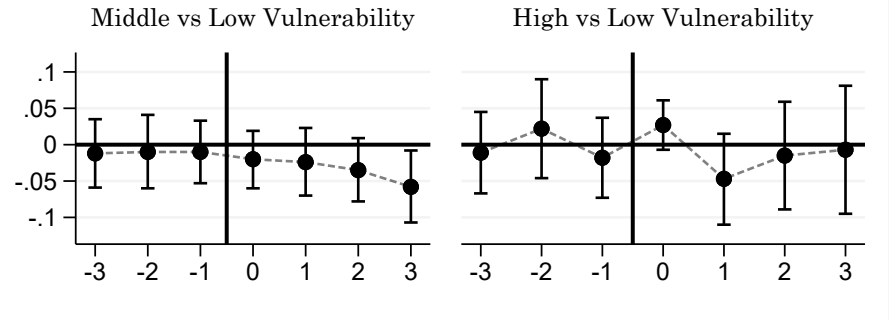
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Excludes the 10% least densely populated counties. Exact numbers are available on request.

Figure B19: Elasticity with respect to the minimum wage, saturated model, excluding 10% least densely populated counties.



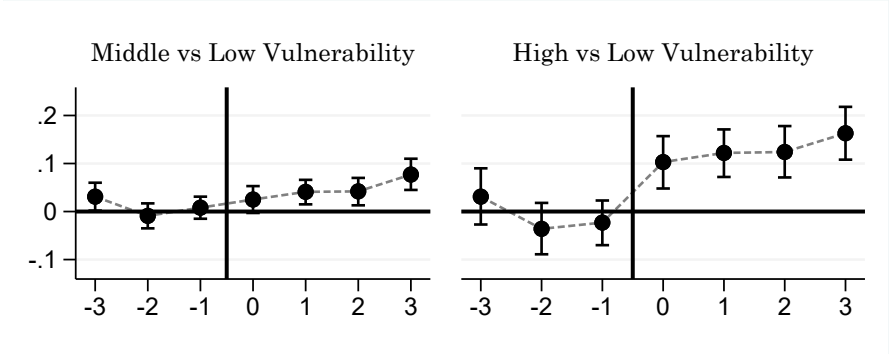
(a) Earnings



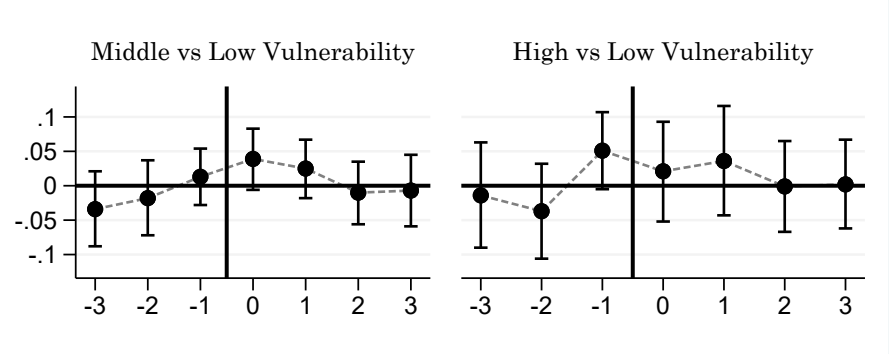
(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Restricted to limited service restaurants (NAICS 7222).

Figure B20: Elasticity with respect to the minimum wage, saturated model, limited service restaurants (NAICS 7222).



(a) Earnings



(b) Employment

The handles show 95% confidence intervals, errors are clustered at the state level. All regressions include controls for changes in their total private sector equivalent and changes in population. County dummies are interacted by quarter of year. Includes state-time dummies. The markers show how the elasticities differ relative to low vulnerability counties. Restricted to full service restaurants (NAICS 7221).

Figure B21: Elasticity with respect to the minimum wage, saturated model, limited service restaurants (NAICS 7221).

Appendix to Chapter 3

1 Additional context

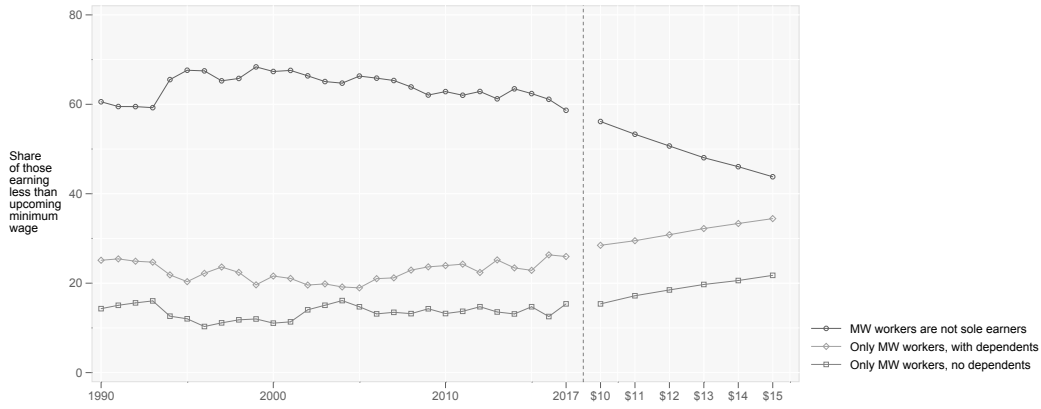


Figure C1: Share of affected workers by (modified) minimum wage worker family type. Historical and projected.

Shares by family type. In Figure C1 we turn to more personal traits by illustrating the position of the minimum wage worker within their household. Historically, 64% of minimum wage workers lived with at least one family member that earned more than the minimum wage. The remaining 36% are the sole breadwinners of their family. Similar statistics are often quoted as proving that minimum wages are an ineffective policy instrument, benefiting mainly the proverbial high school child of a wealthy family. The right pane shows this is mainly due to the low level of the minimum wage, rather than fundamental to the policy tool itself. At $15 an hour, 57% of earners would be the only labour market participants of their family. The majority of those would also have dependants (e.g. children) to take care of (62%).

These characteristics matter for the welfare effects of minimum wage policy. If families experience decreasing marginal utility of income, then both the earnings gain and the potential employment losses associated with minimum wage policy

have stronger effects in families entirely dependent on minimum wage workers. Additionally, public opinion and policymakers may be more concerned about supporting the hard working American family (wo)man supporting their family than the high schooler boogeyman.

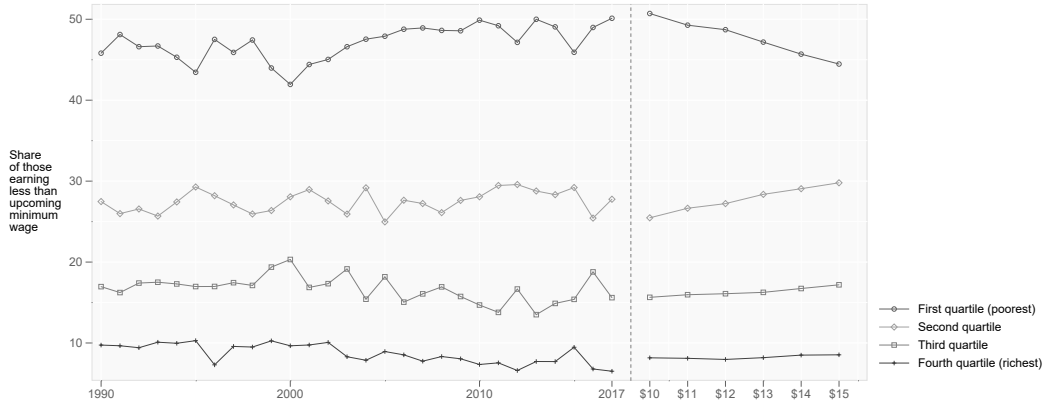


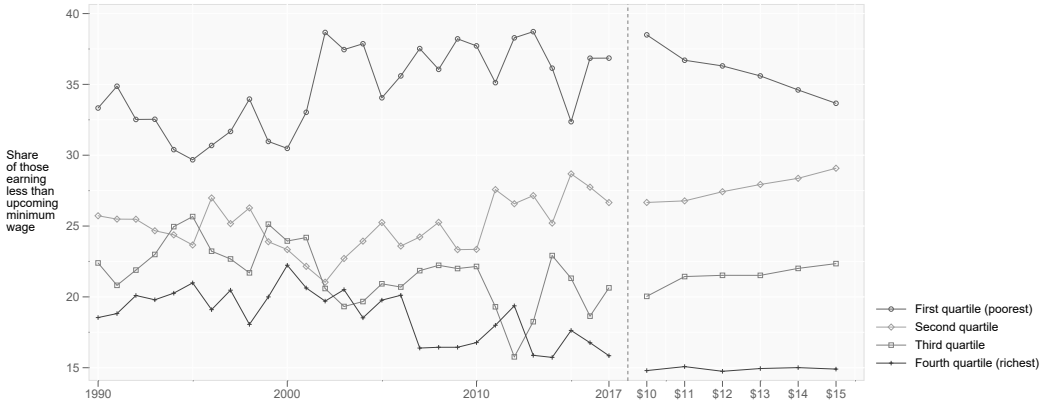
Figure C2: Share of affected workers by income quantile (working families only). Historical and projected.

... by income quantiles. In Figure C2 we show the family income quartile shares to evaluate the targeting efficiency of the minimum wage more directly. We restrict our sample to families with at least one employed member and calculate quartiles within states. We find that almost half of minimum wage workers belong to families in the poorest quartile. Another 28% are in the second quartile. Only 8% belong to the richest quartile of families. These proportions would remain fairly stable even for a $15 dollar minimum wage, with the biggest shift going from the first (44%) to the second quartile (29%). Our results by income quartile contrast greatly with those presented in MaCurdy (2015), who finds that the pre-tax benefits of the 1996 minimum wage were equally distributed across family quintiles. The difference is due to two factors. The first is methodological: to the best of our knowledge, MaCurdy (2015) uses total family income whereas we adjust for family size.[29] In the top panel of Figure C3 in the Appendix, we follow their specification by calculating quartiles nationwide, considering all families and using total (not size-adjusted) family income.[30] The average shares are now rather close together at 35/25/22/19% respectively from poorest to richest. In the bottom panel we show graphs for size-adjusted family income. The average share in the richest quartile in particular drop from 19 to 12%.

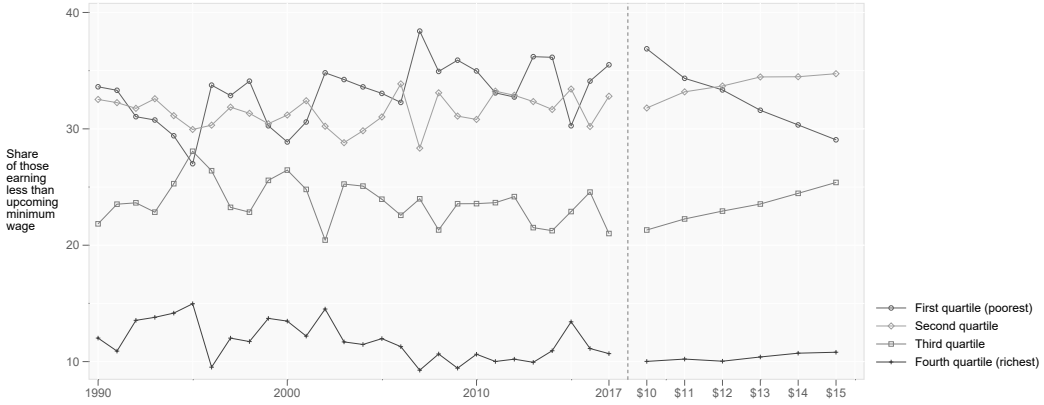
The second factor is fundamental. Minimum wages only apply to working people

[29] We follow the standard approach described in Rojas (2014) where each additional adult counts for 0.7 persons and each child as 0.5 persons.

[30] Two differences remain: their analysis is based on the SIPP and where we look at share affected, they explicitly calculate the annual earnings gain. If there are substantial differences in the initial wages and the hours worked across income quartiles, this could explain the remaining differences.

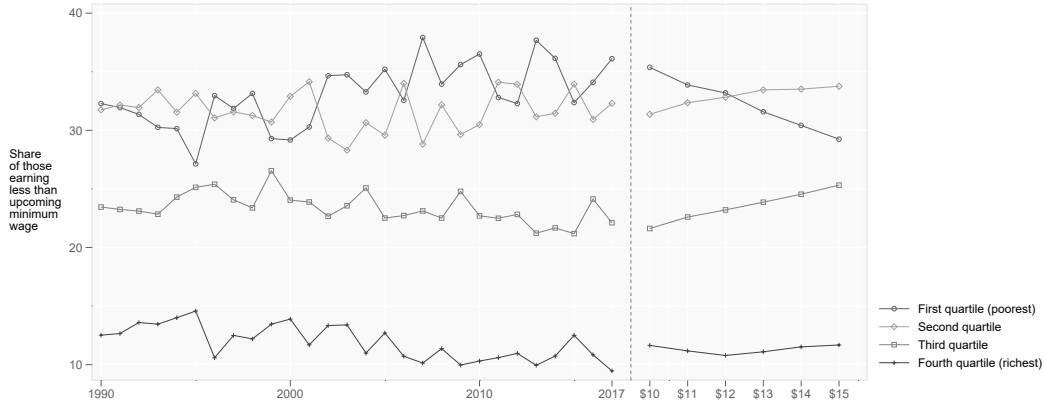


Quartiles calculated **nationwide**, **all** families considered, ***total*** family income.

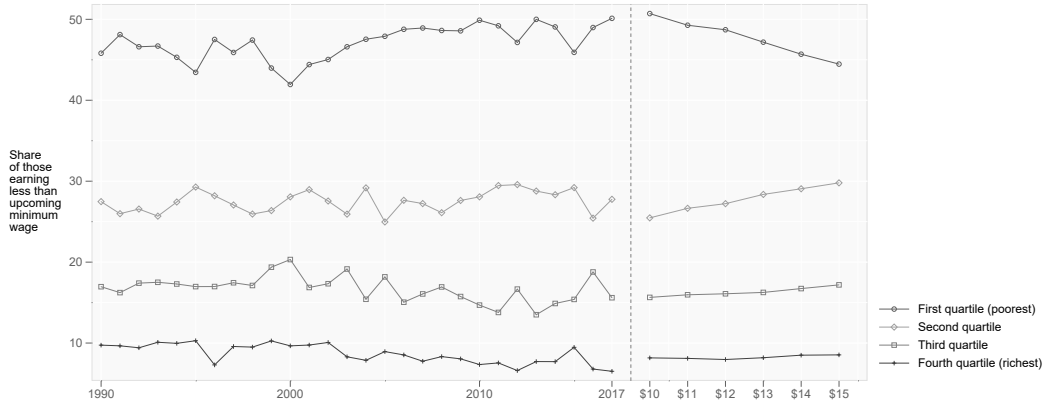


Quartiles calculated **nationwide**, **all** families considered, ***size-adjusted*** family income.

Figure C3: Share of affected workers by income quantile. Historical and projected. Effect of family size adjustment.



Quartiles calculated **statewide**, ***all*** families considered, **size-adjusted** family income.



Quartiles calculated **statewide**, ***only working*** families considered, **size-adjusted** family income.

Figure C4: Share of affected workers by income quantile. Historical and projected. Effect of working family restriction.

and employment is a major source of family income. If you then find that many minimum wage workers are in the richer quantiles, this partially reflects that by definition those workers are in a family with at least one employed member. Figure C4 in the Appendix illustrates this concept. In the top panel we repeat the share of minimum wage workers by unconditional quartile (all families), where up to 40% of minimum wage earners are in the richest half.[31] In the bottom panel we calculate quartiles within the population of families with at least one employed member (also depicted in the main text as Figure C2). This drops the share of minimum wage workers in the top half to 30% or less. Phrased reversely, the bottom half now contains over 70% of minimum wage workers. Which result is more relevant depends on the objective of the policymaker. The MaCurdy (2015) results show that in 1996, the federal minimum wage did target poverty very well. On the other hand, if the minimum wage serves to rebalance some bargaining power from employers to employees, then our results show that it is exactly the workers in the poorest families that benefit.

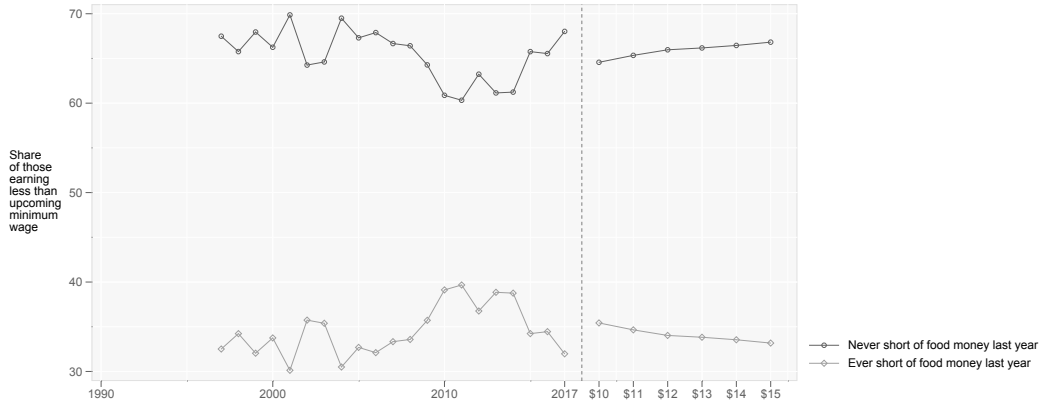


Figure C5: Share of affected workers by food security. Historical and projected.

Food Security. As an alternative angle to study the distributional effects of the minimum wage, we look at the share of affected workers that had insufficient funds to purchase food in the preceding year (Figure C5). Almost one in three minimum wage workers has experienced being short on food money, versus 23% in the general working population (a 50% difference). This ratio stays fairly constant even up to a $15 minimum wage.

[31] We also calculate the quartiles by state to factor in differences in cost of living. This change does not substantially alter any results.

2 Tables

Age	Occupation	< 15%	15-29%	30-49%	> 50%
Teen (15-19)	Non-professional services	0	1	1	7
	Professional services	0	0	0	0
	Manufacturing and agriculture	0	0	0	1
Younger adult (20-29)	Non-professional services	2	5	5	10
	Professional services	1	1	1	1
	Manufacturing and agriculture	2	2	2	3
Adult (30+)	Non-professional services	5	7	7	13
	Professional services	1	2	1	1
	Manufacturing and agriculture	3	4	3	5
	Total	14	23	21	42

Table C1: Share in $15 population by age, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample.

Age	Occupation	< 15%	15-29%	30-49%	> 50%
Teen (15-19)	Non-professional services	1	2	5	5
	Professional services	0	0	0	0
	Manufacturing and agriculture	0	0	1	1
Younger adult (20-29)	Non-professional services	5	6	9	4
	Professional services	1	1	1	0
	Manufacturing and agriculture	2	2	3	1
Adult (30+)	Non-professional services	8	8	11	4
	Professional services	2	1	1	0
	Manufacturing and agriculture	4	4	5	1
	Total	24	23	36	17

Table C2: Share in $13 population by age, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample.

Tenure	Occupation	< 15%	15-29%	30-49%	> 50%
< 1 year	Non-professional services	2	3	5	10
	Professional services	0	1	1	1
	Manufacturing and agriculture	1	2	1	2
1-5 years	Non-professional services	3	6	5	11
	Professional services	1	1	1	1
	Manufacturing and agriculture	2	3	2	3
> 5 years	Non-professional services	4	5	4	6
	Professional services	1	1	1	1
	Manufacturing and agriculture	2	3	2	3
	Total	16	24	22	38

Table C3: Share in $15 population by tenure, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample.

Tenure	Occupation	< 15%	15-29%	30-49%	> 50%
< 1 year	Non-professional services	4	5	9	4
	Professional services	1	1	1	0
	Manufacturing and agriculture	2	2	2	1
1-5 years	Non-professional services	7	6	10	4
	Professional services	1	1	1	1
	Manufacturing and agriculture	3	2	3	1
> 5 years	Non-professional services	5	5	6	2
	Professional services	1	1	1	0
	Manufacturing and agriculture	3	2	3	1
	Total	26	25	36	13

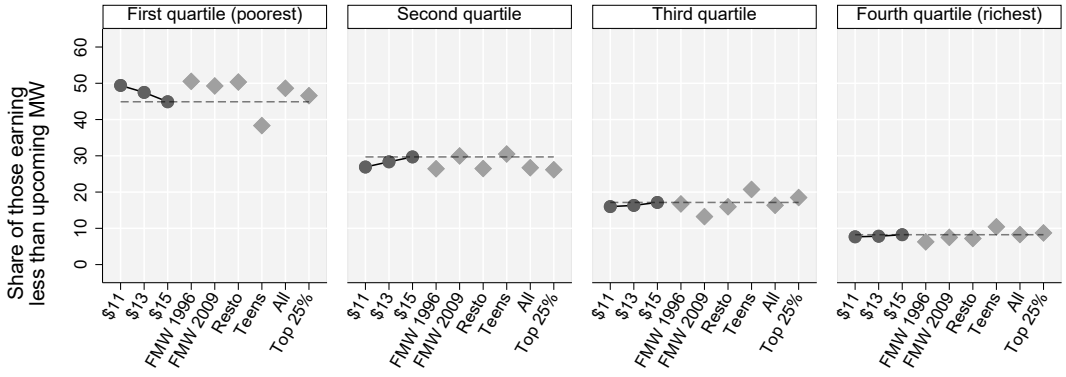
Table C4: Share in $13 population by tenure, occupation and expected raise. Grey cells contain less than 100 treated observations in historical sample.

3 Figures



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	$15
Similarity	0.93	0.97	0.91	0.83	0.94	0.94	1

Figure C6: Similarity By Family Type



Type	FMW96	FMW09	Resto	Teens	All	Top 25%	$15
Similarity	0.99	1.00	0.99	0.99	1.00	1.00	1

Figure C7: Similarity By Income Quartile

Doctoral dissertations from the Faculty of Economics and Business, see:
http://www.kuleuven.ac.be/doctoraatsverdediging/archief.htm.

Department of Managerial Economics, Strategy and Innovation (MSI)
Naamsestraat 69
3000 LEUVEN, België
tel. + 32 16 32 67 00
fax + 32 16 32 67 32
www.kuleuven.be