Who's Afraid of the Minimum Wage? Measuring the Impacts on Independent Businesses Using Matched U.S. Tax Returns*

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Abstract

A common concern surrounding minimum wage policies is their impact on independent businesses, which are feared to be less able to either bear or pass-on cost increases. We examine how these typically small- and medium-size firms accommodate minimum wage increases along product and labor market margins using a matched owner-firm-worker panel dataset drawn from the universe of U.S. tax records over a 10-year period, and using state minimum wage changes as identifying variation. We find that, on average, firms in highly exposed industries do not substantially reduce employment — they do not layoff workers but moderately reduce part-time hiring. Instead, we find that these firms are able to fully finance the new labor costs with new revenues, such that owner profits remain unchanged on average. Higher wage floors, however, forestall entry, particularly of less productive firms, reducing the number of independent firms operating in these industries by roughly 2%. Yet, these industries do not shrink; instead, incumbent responses and strong positive selection among entrants reshape industries that rely heavily on low-wage workers, such that they feature fewer, more productive firms after the cost shock. We also take a worker-level perspective to examine how potentially vulnerable individuals are affected by minimum wage increases. Using panels of low-earning and young workers, we find that their average earnings rise substantially after the minimum wage increases, while they are no less likely to be employed. Worker transitions indicate that minimum wage increases boost worker retention and that worker reallocation from independent firms toward corporations buffers the disemployment impacts from reduced hiring at independent firms.

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1. Introduction

Proposals to raise the minimum wage are often met with arguments that independent businesses may be particularly vulnerable to wage floor increases.[1] Though recent research has found that minimum wage increases have had few deleterious aggregate employment impacts in the short-run,[2] fears that independent businesses operate on margins too slim to accommodate cost increases or face demand too elastic to pass costs through to consumers motivate small businesses exemptions and even wholesale opposition to raising wage floors. At the same time, surveys repeatedly find independent business owners divided on minimum wage policy, with large shares actually supporting higher wage floors.[3]

This study presents the first comprehensive examination of how independent firms accommodate minimum wage increases in the United States. To track independent firms, we construct a novel linked firm-worker-owner panel dataset created from the universe of U.S. tax returns. To measure responses in both product and labor markets, we match the tax returns of pass-through firms — the predominant organizational form of independent businesses — to the individual income tax returns of each of their workers and owners over a 10-year period. These data allow us to measure firm responses across a number of margins, including employment, compensation to different workers, expenditures on non-labor inputs, revenues, profits, as well as firm exit and entry. We complement our firm-level analysis with an individual-level analysis that follows low-earning and young individuals over time and across employers to understand how minimum wage increases affect those most likely to be impacted by wage floor policies.

To estimate the causal effects of minimum wage increases, we exploit subnational state policy changes, allowing us to compare treated firms to similar but wholly untreated firms in

[1] For example, see reporting in the Wall Street Journal and Forbes.

[2] See Cengiz et al. (2019), Dustmann et al. (2021), Dube et al. (2010); Allegretto and Reich (2018), and Harasztosi and Lindner (2019) plus Belman and Wolfson (2014) and Clemens (2021) for reviews of recent research.

[3] For example, see polls from Gallop, CNBC, the American Sustainable Business Council, the Society for Human Resource Management, and reporting in the Washington Post.

states that did not adjust their wage regulations, controlling flexibly for firm size, industry, and market characteristics. Our long panel helps to validate the causal interpretation of the findings by providing several years of data to assess pre-trends in treatment and control states and conduct placebo tests. The breadth of our data, which describe 271,000 firms, allows us to capture sector-level impacts and measure how responses vary by firm and worker characteristics.

Using these data and a difference-in-difference empirical strategy to compare firms in states that did and did not raise their minimum wage, we examine the various margins by which independent firms accommodate the cost shock associated with the minimum wage increases. First, we examine whether and how firms adjust employment when wage floors rise. We find that firms do not lay off employees but modestly reduce hiring by 1.5 part-time jobs per year; the reduction in hiring is fully concentrated in positions paying less than $4,000 per year, mostly held by teenagers.

Next, we examine how independent firms accommodate higher minimum wages since they do not substantially reduce employment. Higher minimum wages drive up the wage bills of exposed firms by 6% on average, with income gains concentrated among low-earning workers and no evidence of reduced pay to high-earning workers. By estimating changes in each element of firms' tax returns and net profits, we can determine who bears the economic burden of these labor cost increases. We find that independent firms finance their higher wage bills with new revenue, raising roughly 3.4% more revenue four years after the minimum wage increase. Revenue rises enough to leave profits fully intact, meaning that value-added per worker increases enough for consumers to entirely finance the higher pay to low-earning workers, while firm owners bear none of the economic burden.

Yet, minimum wage increases are not a free lunch for firms and their owners. In the years after a state raises its minimum wage, entry slows in highly exposed industries. Four years after a wage floor increase, independent firm entry is dampened by 2 percent, meaning that 2 percent fewer independent firms operate in these industries. Firms that do enter, however, are positively selected, featuring higher productivity and greater cost efficiency, but nonetheless dedicating higher shares of revenue to wage bills much like incumbent firms. Among incumbents too, the cost structure changes, leading them to spend proportionately less on non-labor inputs as they increase sales through greater value-added per worker.

The extensive margin dynamics and responses among incumbent firms combine to shape the cost structure and productivity of these sectors overall. Despite dampened entry, the positive selection of entrants facilitates a net shift in the productivity distribution of active firms. Further, we find that these sectors do not shrink — aggregate revenues across all independent firms in highly exposed industries rise by 2.61% (s.e.=0.0202) and aggregate profits are unchanged. Total pay to workers increases by 1.1% (s.e.=0.0065) of baseline revenues; as a result, workers receive a bigger slice of a larger revenue pie. To contextualize our findings, we present a simple conceptual framework of imperfect product market competition with fixed costs to highlight how extensive margin responses and heterogeneity can mediate observed responses to minimum wage increases.

While minimum wages only modestly impact employment from a firm perspective, reduced part-time hiring and reduced firm entry could potentially deny young workers a foothold into the labor market and decrease the number of jobs available to low-earning workers. Examining individual panels of low-earning workers and young individuals, we find that on average their earnings rise by $1,472 and $1,995 per year, respectively, in the years following the minimum wage increase and they are not less likely to be employed, relative to similar workers in untreated states. Additionally, retention rates rise; four years after the minimum wage increase, low-earning workers are 1.46% (s.e.=0.0044) more likely to still work for their baseline employers.

Minimum wage increases also alter worker transition patterns, making workers more likely to move to larger firms, especially large C-corporations. These transition patterns help reconcile the null employment effects we estimate in the individual panels with the modest reductions in employment relationships we document among independent firms in highly exposed industries: workers sustain employment in part by moving away from smaller independent firms to larger corporations. Higher retention rates also coincide with reductions in part-time hiring, indicating that reduced turnover in part explains the decline in the number of employment relationships at independent firms the we observe. Overall, we find that, on average, minimum wage increases have little effect on employment among potentially vulnerable firms and workers, which complements estimates from Card and Kreuger (1995) as well as recent studies finding small short-to-medium-run employment effects of the minimum wages.

This study makes three key contributions. First, we provide comprehensive documentation of how firms accommodate higher wage floors. Ours is the first study to trace the incidence of minimum wages for a large set of firms in the U.S., incorporating product market, labor market, and productivity response margins. We focus on independent firms, which are politically and economically important as their welfare is central to minimum-wage policy debates, and collectively they account for roughly half of employment in the United States. Drawing from the universe of administrative tax records, we have a long panel of firms that allows us to provide a detailed analysis of the joint responses to minimum wage increases along various margins, similar to Harasztosi and Lindner (2019).[4] Our detailed firm-worker link allows us to estimate how firms adjust employment relationships across types of workers, in addition to net employment changes. Because our data also comprise annual representative cross-sections of independent firms, we can track impacts on new firms and therefore measure effects on aggregates. To the deep literature on wage floors, we add fresh evidence that minimum wage policies in the United States mainly redistribute from customers to workers rather than within small firms from owners to workers, with some would-be entrepreneurs also bearing the costs of reduced entry.

[4] Prior work has assessed responses across specific margins such as revenue and profits (Draca et al., 2011), pass-through (Leung, 2021; Renkin et al., 2020), employment, worker reallocation (Dustmann et al., 2021), and exit (Luca and Luca, 2019). Other work has incorporated multiple response margins, but for establishments of a single firm (Coviello et al., 2022; Ashenfelter and Jurajda, 2021; Brummund, 2018), a set of franchisees (Hirsch et al., 2015), or a particular industry (Ruffini, 2022).

Second, our identification strategy, based on state minimum wage changes, allows us to estimate new dimensions of the impacts of minimum wage increases. The natural control group of untreated states provides a sector-level counterfactual that is key to measuring how higher wage floors affect firm entry and productivity dynamics.[5] In particular, because our identification does not depend on differences in pre-reform firm exposure, we can estimate how minimum wages affect firm entry and thereby how selection among entrants contributes to changes in the productivity distribution of highly exposed industries more broadly. Further, the policy variation we exploit also mirrors proposed incremental minimum wage increases where wage floors primarily affect the service sector.

Third, the breadth and duration of our individual panel data mean that we can precisely evaluate how well minimum wages benefit the individuals they aim to help. Our precise estimates of the employment and earnings responses even for subgroups of individuals allow us to measure zero and small employment impacts with tight confidence intervals. Because our data describe total earnings, which incorporates any reductions in employment and hours, our estimates capture how minimum wages affect the material well-being of low income individuals. Our panels track employment in any industry and firm type, linking workers to their employers before and after the minimum wage increase. The link between workers and employers allows us to contextualize the firm-level results, showing that minimum wage increases boost retention and reallocate low-earning and teen workers from independent businesses to C-corporations. These changes in transition dynamics buffer employment responses and reconcile the firm and individual results.

Our study establishes that even the small to medium-size firms that typify independent businesses are able to accommodate higher minimum wages with new revenues, leaving owners' profits unchanged. Workers receive larger paychecks and face flat employment rates in part due to worker reallocation from independent businesses to C-corporations. Higher worker retention and productivity allow incumbent owners to fully escape the economic

[5] We also side-step recently raised concerns (Haanwinckel, 2023) about identification drawing on national variation and using "fraction affected" and "effective minimum wage designs."

burden of minimum wage increases. More broadly, minimum wages reshape the productivity distribution of highly exposed industries. Beyond curtailing entry, by changing the cost structure of industries that heavily rely on low-wage labor, minimum wage increases also change the *types* of firms that enter with implications for existing firms and the industry overall.

2. Measuring Effects of the Minimum Wage with Tax Data

To estimate how independent businesses accommodate the increased labor costs associated with higher wage floors, we leverage administrative tax data to combine worker-level measures of earnings and employment with firm-level measures of revenues, costs and profits.

2.1. Independent Businesses

To study independent (i.e. privately owned), predominantly small and medium-size businesses, we focus on firms organized as "pass-throughs" for tax purposes. Pass-throughs are privately owned businesses with legal forms including S-corporations, Partnerships and Limited Liability Companies (LLCs).[6] Pass-throughs are a large part of the U.S. economy. In 2015, they comprised 78% of non-sole-proprietorship businesses and accounted for 46% of private-sector employment and 52% of business income. Pass-throughs are the predominant organizational form for small and medium-sized businesses in the United States. They represent 79% of firms with fewer than 20 employees, 72% with 20-99 employees, 61% with 100-500 employees, and 77% of all firms with fewer than 500 employees.[7] Pass-throughs are smaller than publicly traded firms on average, but they represent the majority business form in all two-digit NAICS industries except utilities and management of companies and enterprises.

[6] The name "pass-through" comes from the tax treatment. For these private firms, business income is not taxed at the entity level but is "passed through" to the tax returns of the firm owners. This contrasts with publicly traded C-corporations where business income is subject to the corporate income tax.

[7] See Appendix Table C.4 for corresponding statistics. See Smith et al. (2019), Yagan (2015), Cooper et al. (2016) and Risch (2024) for further descriptions of pass-throughs and their owners relative to other business forms in the United States.

In the sections that follow, we simply refer to these firms as independent businesses.[8]

We focus on independent businesses for two reasons. One, our linked firm-worker data are particularly well suited for clean estimation of firm responses among these businesses. For large corporations, it is difficult to consistently link workers to their employers because of complex parent-subsidiary relationships and the use of third-party payroll services, and, critically, it is difficult to precisely link the operations of a firm (or the resulting revenues, costs, profits and employment patterns) to a specific location, which is essential for our research design. Two, the effect of minimum wage policies on independent businesses is understudied relative to their importance to the economy and despite often being the center of discourse around the effect of minimum wages on businesses. The administrative tax data provide a unique opportunity to conduct a comprehensive analysis of how these businesses accommodate the cost shock associated with minimum wage increases.[9] Finally, we note that in the individual-level analyses, we sample all relevant workers regardless of whether they work at independent businesses or large corporations (Section 5).

2.2. Administrative Data: Firm-Worker Panel

Administrative tax data provide comprehensive information on all independent businesses and their workers during the study period. Firm and worker information is drawn from the universe of de-identified administrative tax data. We use a 100% sample of pass-through firms in "treatment" and "control" states (definitions to follow in Section 3) in each year from 2010 to 2019. For each firm-year, we collect information from the firm's annual income tax return, including revenues, cost of goods sold (COGS), various deducted operating expenses, net profits and industry. We use the variables to construct performance measures

[8]We refer to these businesses as "independent businesses" rather than "pass-throughs" because the salient feature for our purposes is that these are privately owned and operated firms, as opposed to large publicly-traded corporations with disperse ownership and access to public capital markets. Pass-through is terminology related to the tax treatment of the business's income, which is not central to the discourse around the impact of minimum wages on businesses.

[9]Industry data and data derived from financial statements generally cover large public corporations rather than small- and medium-sized firms. Scanner data similarly come from larger retailers, and household surveys rarely have information on employer size.

like value-added (revenues less COGS) and materials costs per dollar revenue (COGS plus Other Deductions scaled by revenue). We link each firm to all workers that receive wage and salary payments in that year (through Form W-2). The worker data are used to define employment and total wages and salaries paid (total wage bills) at the firm level, as well as to estimate within-firm changes in employment relationships (e.g. worker entrance and separations and distributions of earnings and ages).

The linked employer-employee data allow us to estimate various margins of response among affected firms and their workers to provide a comprehensive picture of how independent businesses respond to minimum wage increases. We are able to construct firm panels to credibly estimate how firms respond when minimum wages are raised, and having the universe of independent businesses also provides a representative cross-section of firms in each year allowing us to estimate entrance and exit effects and how the characteristics of all independent businesses evolve over time. In our primary analyses, we focus on firms in highly exposed industries (Section 2.3), which consists of 135,163 firms per year in the balanced panel and approximately 271,000 firms per year in the full sample. We use firms in non-highly exposed industries for placebo tests (approximately 1,386,000 firms per year).[10]

To complement our firm-level analyses, we also construct two individual-level panels to estimate impacts on potentially vulnerable workers. The first panel is a 2% random sample of all "low-earning" workers in treatment and control states in the year prior to the minimum wage increases. This dataset spans low-earning workers from all industries and all firms, including large C-corporations that are not included in the firm analysis. This helps provide a more comprehensive analysis of individual earnings and employment effects and to contextualize the firm-level effects observed among independent businesses. Second, we draw a 2% random sample of young individuals (ages 16 to 26) in treatment and control states in the year prior to the minimum wage increases, regardless of whether they are working prior to the reform or not. We discuss these individual data in more detail in Section 5 when we

[10] Further details of the data creation and sample are in Appendix L.

present the accompanying analyses.[11]

2.3. Identifying Industries Employing Minimum Wage Workers

Rather than spread through the economy, minimum wage workers in the U.S. are concentrated in a handful of industries. We focus on these "highly exposed" industries to measure how minimum wage increases affect firms and the workers they employ in the industries where wage floor policies are likely to have meaningful impacts on how firms operate.

We use publicly available data to determine which industries employ enough minimum wage workers to be impacted by policies to raise the minimum wage. We turn to public data because while the tax data afford detailed earnings histories for workers and comprehensive income statement descriptives for firms, they do not record hourly wages making it challenging to precisely estimate exposure at the firm-level. Specifically, we use the Current Population Survey Monthly Outgoing Rotation Groups (CPS MORGs), which describe the hourly wages of workers paid by the hour. Using the CPS MORGs data we calculate the share of non-allocated hourly wage workers in each 4-digit industry paid less than the prevailing minimum wage, pooling across all treatment states and cities in the year before each jurisdiction's policy change. As such, we effectively aggregate worker counts from different calendar years to appropriately measure industry shares just before the policy changes.

We focus on 4-digit industries employing at least 1% of minimum wage workers. These industries collectively account for more than two-thirds of minimum wage workers with "restaurants and other food services" alone accounting for 42% of workers paid the prevailing minimum wage or less. Additional details, along with a full list of industries used in the analysis, are provided in Appendix B.

[11] We also construct a repeated, randomly sampled, 2% cross section of teens in each state and each year to understand how teens entering the labor market after the minimum wage change might be affected by policy. The samples are detailed further in Appendix L.

3. State Minimum Wage Changes and Empirical Strategy

3.1. Policy Details

Our analysis examines the impacts of 19 minimum wage increases that began between 2013 and 2016 and did not exempt small firms, which includes increases among 17 states, Washington, DC, and the city of Chicago. We focus on minimum wage increases of at least $1 that began between 2013 and 2016 to ensure our 2010 to 2019 sample period spans enough years before and after the policy change to assess pre-trends and measure outcomes years after the event.[12] Figure 1 depicts the changes in minimum wages from these 19 policies over our sample period; Appendix Table A.1 shows the dollar and percentage changes in minimum wages from the pre-reform year to four years following, and Appendix Table A.2 provides additional policy details for each increase.[13]

For most reforms, the initial minimum wage increase was the first part of a larger, phased-in minimum wage hike. These phase-ins mirror federal proposals to raise the minimum wage over several years. The minimum wage increases were substantial. The average minimum wage increase across policies was $2.59 or 34.1% ($2.64 or 34.6% firm weighted) four years after the initial increase. Across all treated states, in the year prior to the policy changes, approximately 31.4% of hourly workers (or 18.46% of all workers) were paid wages below the new minimum wage.[14] Our event study analysis traces the collective impact of these wage floor changes over time with the estimates four years after the initial increase providing our most complete measure of the cumulative effect. Because firms may anticipate scheduled or potential future minimum wage increases, our estimates may attribute too strong a response

[12]This is a particularly clean period to estimate the effects of state minimum wage changes because it offers sufficient years following the last federal minimum wage increase in 2009 and sufficient years after the state minimum wage changes and before the COVID pandemic began to affect labor markets in 2020.

[13]We treat Maine's 2017 minimum wage increase as starting in 2016 because Portland, Maine's largest metropolitan area, increased its minimum wage from $7.50 to $9.75 in 2016 in anticipation of the state-level change. The results are qualitatively and quantitatively robust to treating Maine's minimum wage increase as occurring in 2017 or dropping Maine from the sample. We exclude Arizona's 2017 $1.95 increase in its minimum wage as the Arizona wage floor does not apply to firms with revenues less than $500K.

[14]We use the CPS MORGS data to calculate the share of affected workers. To incorporate phase-ins, we use the minimum wage in time $s + 4$ as the new minimum wage for each jurisdiction.

to the actual change in minimum wages in any given year.

3.2. Identification

Using the policy variation described above, we define a treatment and control group of states based on whether they raised their minimum wages during our sample period. The treatment states are those depicted in Figure 1 and the control group consists of 22 states that did not legislate a minimum wage increase anytime between 2011-2019.[15] Our control group thus represents a set of "clean controls" that had no changes over this period and therefore facilitate unbiased estimation in a stacked event-study (Cengiz et al. (2019)).

To estimate the effect of the minimum wage increases, we use an event-study design to compare outcomes of firms and workers in treated and untreated states over time. The event-study style design helps to assess the validity of comparing firms in treated and untreated states by estimating whether trends moved in parallel prior to the reforms. Our workhorse model is a panel stacked difference-in-differences (DD) specification of the form:

$$y_{jct} = \alpha + \sum_{s=-4, s\neq-1}^{4} (\beta_s treat_{jc} + \Gamma_s X_{jc}) \times year_{s=t} + \delta_{ct} + \psi_{jc} + \nu_{jct} \tag{1}$$

where j indexes the firm, t year, and c the "cohort" associated with the year of the initial minimum wage increase. We use a stacked design, meaning that for each treatment cohort, there is a full set of control firms from all control states and estimates are in event time, s, relative to the cohort year.[16]

[15] The control states are Alabama, Georgia, Idaho, Illinois (excluding Chicago), Indiana, Iowa, Kansas, Kentucky, Louisiana, Mississippi, Nevada, New Hampshire, New Mexico, North Carolina, North Dakota, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Utah, and Virginia. Note, Illinois and Nevada had small minium wage increases in the first year of our sample period, 2010, of \$0.25 and \$0.70, respectively. In our regression specifications, we include an indicator variable for these state-years, meaning that these control states do not contribute to the counterfactual for event-year $s-4$ for the 2013 cohort of minimum wage increases.

[16] That is, there is a set of firms from all control states for each of the 2013-2016 treatment cohorts. Each set of control firms has a unique event time relative to the associated cohort, spanning 9 years around each cohort year. For example, the 2014 cohort treatment firms are those that first raise the minimum wages in 2014, which we call event time *s=0*, and the base year is the year prior to the minimum wage changes, 2013, or year *s-1*. For the associated control firms in this cohort, event time is assigned accordingly, 2014 is *s=0* and 2013 is *s-1*. For the 2015 cohort, control firms are from the same set of states, but event time is around

The difference-in-difference estimator β_s represents the differential in the average outcome (y_{jct}) between firms in treated and untreated states relative to the pre-reform base year, $s-1$; $treat_{jc}$ is an indicator for the firm operating in a state with a minimum wage increase. The main specification includes firm-cohort fixed effects (ψ_{jc}) and cohort-by-year fixed effects (δ_{ct}). A vector of fixed firm and market characteristics (X_{jc}) is included to ensure comparisons among similar firms in similar markets, and are flexibly interacted with year to allow for differential time trends among different types of firms. In the firm-level analyses, the controls included in X_{jc} are categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates, unless otherwise stated. Standard errors are conservatively clustered at the state-by-cohort level.[17]

For our main analyses in Sections 4.2 and 4.3, we estimate the effect on firm outcomes, z_{jct}, scaled by baseline revenues, i.e. $y_{jct} = z_{jct}/revenue_{jc,s-1}$. These regressions are weighted by baseline log revenues. Occasionally, we estimate percent changes in a specific firm outcome, in which case we scale the contemporaneous value by the base year value, i.e. $y_{jct} = z_{jct}/z_{jc,s-1}$, and regressions are weighted by the log of the baseline value of the corresponding variable. As such, all regressions are dollar or employee weighted, presenting the policy relevant estimates of the average effects.

When conducting individual-level analyses (Section 5), we use the same regression framework, with slight modifications:

$$y_{ict} = \alpha + \sum_{s=-4, s\neq -1}^{4} (\beta_s treat_{ic} + \Gamma_s V_{ic}) \times year_{s=t} + \delta_{ct} + \rho_{ic} + \nu_{ict} \qquad (2)$$

First, for individual analyses we use a similar set of controls (V_{ic}), but drop the controls for firm size, industry and value-added, and instead include controls for individual age and

the 2015 minimum wage increase, so 2015 is *s=0* and 2014 is *s-1*.

[17] We winsorize all raw reported firm values from above and below at the 1% level. Winsorization is standard when working with the population business tax returns. See, for example, Yagan (2015), DeBacker et al. (2019) and Kline et al. (2019).

age squared. Second, we include an individual-cohort fixed effect (ρ_{ic}) rather than a firm fixed effect. Additionally, we use a number of linear probability models (LPMs) where the outcome is an indicator variable for being employed, or being employed in a certain type of firm, in which case the coefficients are interpreted as percentage point changes for the treatment group relative to the control group.

Table 1 shows key characteristics of the sampled firms. Means are presented in the left panel and medians in the right. The summary statistics show that the firms in treatment and control states are quite similar in terms of observable characteristics prior to any regression adjustments. The firms are relatively small, with average revenues of about $1.75 million, net income of about $120,000 and 40-50 workers per year on average. The medians are below the means highlighting the predominance of small firms in these industries, but with a tail of larger independent businesses.

3.2.1. Implications of the Research Design for Understanding Minimum Wage Responses

Our identification strategy compares firms and individuals in states that increase their minimum wages to similar firms and individuals in states that left their wage floors unchanged. Since our estimation strategy does not rely on exploiting variation by baseline firm-level exposure, we are able to separately estimate effects of increased minimum wages on firms that were active in the pre-period and how the minimum wage shapes aggregate outcomes and firm characteristics in exposed markets through firm entrance and exit dynamics.

The principal threat to identification is policy endogeneity[18] One form of endogeneity is if states that elect to raise minimum wages at a given time are on different paths than those that do not, for example, if minimum wages are increased in states with faster growing economies. This would be a violation of the standard parallel trends assumption for difference-in-difference designs and is a legitimate concern. We use pre-trend analyses for all of the primary outcomes to assess the plausibility of this assumption in our setting. The

[18] Research centered on minimum wage increases during recessionary periods finds somewhat larger employment impacts (Clemens and Wither, 2019) and effects on firm finances and exit rates (Chava et al., 2019).

second identifying assumption is that there are no other concurrent changes in the economy that would differentially affect outcomes in treated and control states. This is a general challenge for designs with state-level variation. We provide evidence that our estimates are consistent with responses to minimum wage policies, for example, by investigating the distribution of wage changes and types of workers that are affected by firm-level employment adjustments, and by showing that differential outcomes between treatment and control states are concentrated only in industries highly exposed to minimum wage changes.

4. Firm Adjustments to Higher Minimum Wages

Our firm-level analysis provides granular insight into how independent businesses adjust to higher wage floors and the ultimate impact on highly exposed industries. First, we use a balanced panel to learn how existing firms accommodate the new labor cost shock. We examine how firms adjust their labor inputs, estimating changes in the number of employment relationships and the types of workers and jobs that are affected, and analyze how firms finance the new labor costs to understand who bears the burden of the wage increases (Sections 4.1 - 4.3). Then, we use the full unbalanced panel of all active firms in exposed industries in all years to assess how minimum wages affect the viability of independent firms and how the resulting selection shapes productivity overall (Sections 4.5 and 4.6).

4.1. Minimum Wages and Firm Employment

Our firm-worker panel tracks employment relationships between firms and all workers that receive wage and salary compensation from the firm in a given year, including part-time or part-year employees.[19] The first panel of Figure 2 traces the average change in the net number of employment relationships, as well as the associated changes in new (entrants) and existing employment relationships (separations), as estimated using Equation (1). The average independent business subject to a higher wage floor does not lay off workers it em-

[19] This is typically the case with administrative data that describes only employment and compensation rather than hours worked by employees over the reporting period (here one year).

ployed prior to the minimum wage increase. Firms in treated states, however, reduce hiring. By four years after the minimum wage increases, firms in highly exposed industries hire, and thus have employment relationships with, roughly 1.5 fewer workers a year than similar firms in control states on average. This amounts to an own-wage employment elasticity of -0.245 (s.e.=0.130), which is small in magnitude and in line with previous studies that do not focus on the small and medium-sized firms that typify independent businesses (see Appendix Figure D.1).[20,21]

Reduced hiring is highly concentrated among teenagers and low-earning jobs. Panels B and C of Figure 2 decompose the change in employment relationships by employee age and earnings. Firms subject to higher minimum wages primarily hire fewer workers under 20 years of age. The missing hires consist entirely of workers who would have been paid less than \$3,900 annually — with the majority (67%) earning less than \$1,000 per year. In short, higher minimum wages lead these firms to reduce very part-time, less-experienced labor inputs.[22]

4.2. Financing Higher Pay to Low-Wage Workers with Added Revenue

As firms make only minor employment adjustments, we expect wage bills to rise sharply following the minimum wage increase. Panel A of Figure 3 plots coefficients from a firm-level event study regression of Equation (1) where the dependent variable is the ratio of the firm's wage bill to its base year revenue. The annual estimates measure the average scaled wage bill increase among firms in highly exposed industries operating in states that raised their minimum wages compared to similar firms in control states.

The estimates prior to the reform year ($s=0$) show that firms in treated and untreated

[20] We estimate firm-level own-wage elasticities (OWEs) by separately estimating the change in log employment and the change in log average wages using Equation (1), then dividing the estimated change in log employment by the change in log average wages. Standard errors are calculated using the Delta method.

[21] It is worth noting that these changes in the number of W-2s issued by highly exposed independent businesses may overstate the true change in the number of jobs at these firms, as minimum wage increases affect retention and worker churn. We consider these dimensions of minimum wage impacts in Section 5.4

[22] These results align with Wursten and Reich (2023) who find that the minimum wage's only disemployment effects are among teenagers at small businesses and Dube and Reich (2007) who study restaurants and find no evidence of employment losses.

states had similar wage bill trends in the years preceding the minimum wage increases. Average wage bills then rise sharply among treated firms. Wage bills grow each year as minimum wage increases phase-in; four years down the line, the average treated firm pays 1.46% more in wages as a share of baseline revenues.[23,24]

Raising the minimum wage directly increases the hourly pay of workers for whom the floor binds but may also indirectly affect the incomes of other workers. Firms may partially offset the added costs of higher minimum wages by reducing the compensation of higher-earning workers. Alternatively, increased pay to the lowest earners may boost pay higher up the distribution as firms maintain wage differentials by occupation or seniority.[25] Panel B of Figure 3 examines how higher minimum wages affect the distribution of compensation across workers within a firm. Annual compensation rises substantially for workers earning between $3,900 and $35,000 per year, with the largest gains accruing to workers earning the equivalent of working full-time at the minimum wage. There is no evidence of reduced earnings for those higher up in the earnings distribution, ruling out redistribution from middle- or high-income employees. Instead, earnings also rise for workers earning slightly above full-time at the new minimum wage, suggesting potential spillovers to low-earning workers above the statutory wage floor.[26]

Since independent firms do not substantively shed workers or reduce compensation to

[23]The observed wage bill change subsumes any changes to their input mix that firms make in response to the higher wage floor. As we do not observe large employment changes, these behavioral responses may be less substantial. They will, however, include any other changes firms make in the hours employees work and the type and effort of work they do.

[24]To benchmark the estimated effect size, Appendix Table H.16 shows that this corresponds to a 6% wage bill increase by year $s + 4$. On average, approximately $54,000 of the wage bills of treatment firms are from earnings paid to low-earning workers at baseline. On average minimum wages increase by 36.4% by year $s + 4$ (Appendix Table A.1). A 34.6% increase in low-earning worker earnings is $54,000*0.346=$18,684, which is a 5.5% increase in the total average baseline wage bill from Table 1, quite similar to the estimated 6% increase.

[25]In the spirit of Clemens et al. (2018) we use measures of compensation outside of wages and salary, to learn if minimum wage increases reduce non-cash compensation. These estimates, reported in Appendix Table E.6 show that higher minimum wages have no discernible impact on firm deductions for other worker benefits, which include health insurance and other in-kind benefits but very slightly raise pension contributions.

[26]This pattern also serves as a validity test for the results, showing that the wage bill increases are driven by the low-earning workers most likely to be affected by minimum wage policies, and not by high-earning workers where changes may be more likely to be driven by other policies or market shocks.

higher-income workers in response to minimum wage increases, they must find other ways to accommodate the wage bill increases caused by higher wage floors. Prior work suggests that firms are often able to pass the cost of minimum wage increases on to consumers through higher prices (Harasztosi and Lindner, 2019; Ashenfelter and Jurajda, 2021; Leung, 2021; Renkin et al., 2020; Link, 2019; Sorkin, 2015) although there is also evidence of reduced profits (Draca et al., 2011; Harasztosi and Lindner, 2019; Ganapati and Weaver, 2017).[27] Tax data do not allow us to separate quantities and prices, but we can examine how revenues evolve to offset higher labor costs after the minimum wage increases.

Figure 4 plots the average yearly change in revenue among firms subject to higher minimum wages relative to firms in control states. After following a pre-trend similar to that of untreated firms, revenues among treated firms rise starting the year after the minimum wage increase. Four years later, the revenue of the average firm subject to a higher wage floor grew roughly 3.37% more than the revenue of firms in states that did not raise their minimum wages.

These added revenues shape the ultimate impact of minimum wage increases on owner profits. Figure 5 plots the evolution of owner profits as a share of baseline revenues among firms subject to higher minimum wages relative to firms that were not. Owner profits captures the total return to owners of the business, combining business profit with salary payments to owner-operators. Four years after the minimum wage increases, average owner profit is unchanged with a point estimate of 0.0001 (s.e.=0.0030), ruling out losses larger than 0.58% of baseline revenues with 95% confidence.

4.3. Understanding the Economic Burden of Minimum Wage Increases

Beyond simply measuring how wage bills, revenue and owner profits evolve after the minimum wage increase, we can assemble these estimates along with changes in other expenditures

[27] Studies using scanner data such as Leung (2021) and Renkin et al. (2020) naturally focus on products made and retailed by the large firms that typically report their data to Nielsen, Kantar, or other scanner data aggregators. As such, these estimates may be less applicable to small and medium-size firms or service industry firms that account for the majority of minimum wage workers.

reported in the tax data to trace the incidence of the minimum wage. Table 2 presents estimates of the four-year impact of minimum wages on different income statement items: the components of variable costs, revenues, and owner profits, all scaled by baseline revenue to facilitate comparisons. For wage bills, revenues, and owner profits, these estimates match the $s + 4$ estimates from Figures 3 to 5.

In addition to wage bills, our data also track expenditures on other non-labor inputs, such as the COGS, which capture intermediate goods, and "other deductions", which capture spending on other non-labor inputs.[28,29] Minimum wage increases can affect spending on non-labor inputs as firms substitute away from labor or face higher prices from upstream firms. Higher spending on non-labor inputs may also reflect added productivity from minimum wage increases as documented by Coviello et al. (2022) and Emanuel and Harrington (2025) with spending on COGS and equipment rising with added sales.

The average independent business operating in a highly exposed industry not only spent more on wages after the minimum wage increase, but also increased its expenditures on COGS by 1.32% of baseline revenue. Higher spending on non-labor inputs is differentially concentrated across industries. Labor is a relatively more important input among restaurants, which collectively employ 42% of all minimum wage workers, while intermediate materials costs (ex. inventories) are a larger share of variable costs for highly exposed industries outside of restaurants, largely retail. For restaurants, wage payments are 26% and COGS are 41% of revenues at baseline; for other industries, wages are only 12% of revenues and COGS are 55% (Appendix Table G.13). As such, minimum wage increases raise restaurant wage bills substantially as share of baseline revenue, by 2.06% four years after the wage floor increase, while COGS rises by only 0.5% of baseline revenues. In contrast, among highly exposed industries outside of restaurants, COGS rise by 3.63% as a share of baseline revenue,

[28] Other deductions include "total allowable trade or business deductions that aren't deductible elsewhere." Some of the eligible deductions include: supplies used and consumed in the business, certain business start-up and organizational costs, insurance premiums, legal and professional fees, and utilities.

[29] We also estimate the impact of minimum wage increases on investment and find no effect as reported in the depreciation line item of Table E.6, which includes immediately expensed investment (Section 179).

while labor costs increase by 0.78% of baseline revenue.[30]

Regardless of industry, higher minimum wages increase variable costs by similar amounts, roughly 2.6% to 2.8% of baseline revenue. As Table 2 shows, this increase in costs is fully financed with new revenues such that owners bear no economic burden. For a treated company, per dollar of baseline revenue, labor costs rose by 1.46 cents while COGS rose by 1.32 cents; this led to a revenue increase of 3.37 cents, leaving profits wholly unchanged. In short, even for the small- and medium-sized firms that characterize independent businesses, consumers fully finance the higher pay to workers caused by minimum wage increases.[31]

4.4. Robustness of Firm Analysis

In the Online Appendix we examine the robustness of the firm estimates that underlie the preceding analyses. First, we show that the results are not overly sensitive to any specific controls used in the firm-level regressions; the controls correct for some selection in pre-trends but do not drive the qualitative results (Appendix Figure H.4). Appendix Table E.7 shows that the firm results are very similar when using an unbalanced panel allowing firms to exit, which is unsurprising given the lack of differential exit rates, as documented in the following subsection. Appendix Table H.16 and Appendix Figures H.5 and H.6 show that the results and pre-trends are consistent when estimating percent changes, rather than outcomes scaled by revenue. We examine the heterogeneity of our estimates by baseline size (Appendix Table F.8) and show the results — modest employment adjustments and revenue increases sufficient to fully cover wage and other cost increases — are consistent across the firm size distribution, including for the smallest independent firms. While there are differences in the magnitudes of revenue and cost effects, revenues rise sufficiently to fully offset cost increases

[30] Appendix Table H.16 shows the effects on firm outcomes in percent changes. While wage bills only rise modestly as a share of baseline revenues among non-restaurants, this is associated with a 4.81% increase in wage bills on average. The modest change relative to revenues reflects the low baseline share of wage bills to revenues. For restaurants, wage bills rise 6.63%. Appendix Tables G.14 and G.15 provide a further break-down by industry.

[31] Independent firms hire fewer part-time workers after minimum wage increases; whether this group bears a portion of the costs depends on whether affected workers find employment elsewhere and is examined in detail in Section 5.

across the baseline productivity distribution (Appendix Table F.10) and among firms with different degrees of baseline reliance on low-earning labor (Appendix Table F.9).[32] Roth et al. (2023) show that even with parallel trends conditional on covariates X_j, linear regressions can yield inconsistent estimates if treatment effects are heterogeneous in X_j. Appendix Table J.20 shows that the results are very similar using "regression adjusted" estimators of the treatment effects, which are consistent under weaker homogeneity assumptions.[33]

As a further robustness test, we present results from a design using border counties as in Card and Krueger (2000), Dube et al. (2010, 2016) and Allegretto et al. (2017) (Appendix M, Appendix Table M.24). While the magnitudes of the estimated effects on firm outcomes are somewhat attenuated, the qualitative results are very similar when using the border firms as when using the full sample. Revenues rise enough to offset the new higher wage and non-labor costs such that owner profits do not decrease. The estimated employment effects and corresponding OWEs are very similar to those estimated using the full sample.

Finally, as a placebo test, we estimate effects of the minimum wage on independent firms in industries largely unexposed to the minimum wage increases (Appendix Figure H.7). We find no differential trends in firm outcomes before or after the minimum wage changes. There is no effect on the average number of employment relationships among firms in these industries either, with a point estimate of -0.167 (s.e.=0.170) as of year $s+4$. Together, this supports the claim that observed effects in highly exposed industries are driven by minimum wage policies and not differential concurrent economic changes in treatment and control states.

4.5. Extensive Margin Firm Responses

Although minimum wage increases have minimal employment effects for incumbent independent firms in highly exposed industries, and firm owners fully escape the economic burden of higher labor costs, we may worry that slimmer margins and unchanged fixed costs may lead

[32] Own-wage elasticities are somewhat higher among larger firms and less productive firms.

[33] Our use of non-parametric controls also mitigates this concern, as does the robustness across subsamples and when including various controls. Details of the "regression adjusted" estimation are in Appendix J.

some firms to shutter or opt to not enter. To measure changes in the number of independent firms operating in treatment and control states, we create a collapsed dataset that allows us to estimate differential entry and exit rates while controlling for relevant market characteristics. Concretely, we collapse the firm-level dataset by cohort, year, treatment status, industry, and market characteristics. The collapsed data contain counts of active firms in each year that did and did not operate in the base year *s-1*, summing to the total number of active firms in each cell in each year. Using these data and event-study regressions in the style of Equation (1) we estimate the differential change in the number of active firms in treatment and control states and decompose that change into firm entry and exit.[34]

Figure 6 plots annual changes in exit, entry, and the number of firms, relative to base year, $s-1$. Firm exit, where a firm that operated in pre-reform base year $s-1$ does not operate in year s, is flat following the minimum wage increase. Our reasonably precise zero estimate rules out exit increases larger than 0.53% with 95% confidence. Ultimately, existing owners are able to finance higher pay to low-wage workers with new revenues, avoiding both profit decreases and threats to viability.

Exit may be the less sensitive extensive response margin. Firms often sign long-dated leases, rendering their largest fixed costs sunk and leaving them willing to operate as long as sales offset their operating costs. However, potential entrants may balk at the higher costs of operating in low-wage industries and opt not to enter. Figure 6 also reports the impact of the minimum wage increases on entry, where an entrant is a firm that is active in year s but did not operate in the pre-reform year $s-1$. Although raising the minimum wage has no impact on exit decisions, it meaningfully reduces entry. Four years after the minimum wage is raised, entry into industries highly exposed to wage floor policy is 2.28 percent lower than in control states. Slower entry meaningfully impacts these industries, ultimately reducing the number of firms by 1.9 percent.[35]

[34] Appendix L.5 contains further details of the analysis using the collapsed data.

[35] For exit rates, which can be estimated using firm-level regressions, we validate the results using Equation (1) with the standard set of firm and market controls and the outcome variable being an indicator for the firm operating in year s. Appendix Figure H.9 shows the results of this exercise, which are very similar to

4.6. Selection and Productivity

Beyond curtailing entry, by changing the cost structure of industries that heavily rely on low-wage labor, minimum wage increases also change the *types* of firms that enter with implications for existing firms and the industry overall. Table 3 examines how the distributions of performance measures change among firms in treated states relative to similar firms in states that did not adjust their wage floors. We use the baseline distribution of key performance measures, such as the wage bill-to-revenue ratio or value-added per worker, to define the top and bottom quartiles of labor use, other input efficiency, and productivity. We then estimate the probability that a firm's performance four years after the minimum wage increase would rank in the top or bottom quartile of the baseline distribution.[36] Differential probabilities map how the distribution of firm characteristics evolves as a result of the minimum wage increases. Table 3 reports these estimates for entering firms, incumbents, and highly exposed industries overall.

The distributions of costs and productivity change as a result of the minimum wage, driven, in part, by strong selection of entrant firms. Both entrant and incumbent firms are more likely to have high wage bill-to-revenue ratios after the minimum wage increase. New firms, in particular, are also less likely to rank among the lowest wage bill-to-revenue ratio firms. This evidence suggests that higher labor costs do not necessarily lead entrants to design their facilities to rely less on labor; instead entering firms are precisely those that can cover the higher labor costs of a post-minimum wage increase market.

Firms offset the higher labor costs of the minimum wage increase in part by becoming more efficient. Measuring material costs as COGS plus Other Deductions, we find that both incumbent firms and entrants are leaner after the minimum wage increase. Incumbent firms are 2% more likely to be low-cost, while entrants are 2.46% less likely to rank among the least cost-efficient firms. Firms finance higher minimum wages by generating more revenue

the main results using the collapsed data.

[36] Concretely, we estimate regression Equation (1) where the outcome variable is an indicator for the firm being in quartile q of a specific measure in year t.

without proportionately increasing input costs.

In addition to making firms leaner, minimum wage increases result in higher labor productivity, measured as value-added per worker, making it easier for firms to finance higher pay for workers. We see that existing and new firms are both more likely to have high value-added per worker after the minimum wage increase. Entrants, in particular, are sharply selected; the average entering firm is 4.28% more likely to be high productivity and 5.25% less likely to be low productivity. Active firms, both incumbents and entrants, are precisely those able to generate sufficient revenue per worker to cover the new higher labor costs.

Lower entry and strong selection among entrants after the minimum wage increase shape how revenues are distributed in these highly exposed industries. We create a simple state-by-industry collapsed dataset to estimate how minimum wages affect aggregate measures across all independent firms in highly exposed industries. The upper row of Table 4 reports the effect of the minimum wage on aggregate outcomes scaled by baseline revenues. Many of the estimates are not statistically significant as they compare state aggregates and thus draw on relatively few observations; nonetheless, they provide suggestive evidence on the aggregate impacts of minimum wage increases. Total revenues rose by 2.61% four years after the minimum wage increases, meaning that reduced entry did not fully offset higher revenues at operating firms. In aggregate, wage bills rise as a share of collective revenue, indicating that raising the minimum wage ultimately results in workers receiving a larger slice of a larger revenue pie. Material costs rise along with wage bills, but overall productivity improves enough to keep aggregate profits unchanged. These estimates confirm that, in aggregate, total payments to workers rise and the burden is borne by consumers rather than owners.

The lower row of Table 4 reports estimates of average log impacts using a firm-level unbalanced panel that includes all active independent firms in highly exposed industries in all years. These estimates subsume changes in the composition of firms due to differential entry. Estimates using the full population of firms confirm the findings using the balanced panel and the collapsed aggregates. Average revenues and wage bills rise substantially, by roughly 4% and 7.5% respectively, and value-added per worker rises enough on average to fully protect firm profits.[37]

5. Minimum Wage Increases and Low Income Workers

The firm-level analysis presented in Section 4 shows that firms adjust to higher minimum wages by modestly reducing part-time hiring and largely by using new revenues to offset higher pay to low-income workers. The lack of meaningful employment impacts at independent businesses raises the possibility that higher wages lead to higher incomes for the lowest-paid workers. However, depressed entry of independent businesses, reduced part-time hiring of teenagers, and potentially different adjustments at corporations will also shape the income trajectories of workers. To understand how workers are impacted by wage floor policies, we turn from our firm-level analysis to panels of individuals that include workers employed by corporations as well those with periods of non-employment.

We construct two individual-level panels describing the trajectories of 1) low-earning workers, where low-earners are those earning less than \$20K in total individual wages working in any industry or type of firm the year prior to the minimum wage increase ($s-1$) and are either not working or earning less than \$25K in year $s-2$; and 2) young individuals between ages 15 and 26 in the year prior to the minimum wage increase ($s-1$), who may be working in any kind of industry or firm, or not employed. Unlike our firm analysis, these panels span employment at all types of firms, including corporations, and all industries, including those less exposed to wage floor policies. As such, the earnings and employment impacts reported in this section reflect the economy-wide effects of minimum wage policies on these subsets of workers.

Each panel provides specific insights, but also has specific limitations. The panel of low-earning workers shows us how the earnings and employment of typical low-wage workers are

[37] Coefficients in row 2 of Table 4 are from a regression specification similar to Equation (1), but with no firm fixed effect so estimates trace changes in the differential characteristics of active firms in treatment relative to control states in each year relative to the timing of the minimum wage increases.

affected by minimum wage policies, but as it conditions on employment in the year before the policy change, it cannot capture impacts on worker entry. The panel focused on young individuals, on the other hand, will tell us how the entry and earnings of less experienced workers and non-workers, who may lose their first foothold in the workforce as independent businesses hire fewer part-time teenagers, are affected by wage floors. Of course, a panel of young people fails to describe the experience of other minimum wage workers. Together, these panels and the subpopulations therein provide a detailed picture of how the incomes and work opportunities of the people most likely to be impacted by minimum wages evolve in the years after the minimum wage is raised.

In each regression, specified in Equation (2), the earnings and employment trajectories of low-earning or young individuals in control states effectively stand in for how earnings and employment may have evolved in treatment states had they not raised their minimum wage. As such, these control trajectories help account for any mean reversion or macroeconomic trends affecting the earnings of low-earning or young individuals similarly in control and treatment states. Earnings and employment patterns that track each other in the years prior to the policy change bolster the exclusion restriction — that outcomes would have been similar in treatment and control states in the absence of any policy change.

5.1. Earnings

Earnings are a key measure of how minimum wages affect the material well-being of low-income individuals, as they reflect the combined impacts on wage rates, employment, and hours worked. Figure 7 examines how higher minimum wages affect the earnings of low-earning workers and young individuals who may or may not be employed at baseline. In each panel, the estimates in the years prior to the minimum wage increase are precise zeroes, indicating that individuals in our treatment and control states had very similar earnings trajectories before the policy change.

Panel A plots the evolution of average earnings for low-earning workers in treated states relative to similar workers in control states for the full panel of low-earning workers, and separately for workers employed in highly-exposed industries at baseline. Earnings rise substantially following the minimum wage hike with an average increase across all low-earning workers of \$1,472 per year four years later, and an average earnings gain of \$1,533 for low-earning workers initially employed in highly exposed industries. As a share of prior earnings, these gains are meaningful, amounting to a 19.6% increase for low-earning workers overall and a 14.6% increase for workers initially employed in highly exposed industries. These estimates do not condition on being employed in any year other than year $s-1$, and as such include spells of non-employment when workers' earnings are recorded as zeroes.

We turn to the panel of young individuals to explore whether the reduction in the hiring of young, part-time workers at independent firms might lead minimum wage increases to actually dampen the earnings trajectories of young workers. Panel B of Figure 7 tracks the earnings of our panel of young individuals overall, and by age and employment at baseline. Young workers benefit quite quickly and meaningfully with a relative increase of \$1,995 per year four years post. Panel C shows that teenagers see a similar average increase of \$1,845. These panels also show that young and teen individuals not employed at baseline saw similar boosts in average earnings four years out, though the gains are somewhat slower to develop, particularly for teens.

5.2. Employment

Figure 8 reports the employment effects of the minimum wage hike for low-earning and young individuals. Specifically, the estimates measure the change in employment rates over the five-year period between the year prior to and four years after the minimum wage increases. The estimated employment effects are flat for all groups of low-earning workers. Young individuals, on the other hand, are generally slightly more likely to be employed after the minimum wage increase, with the average baseline 15 to 26 year old having a 1.39% higher probability of employment. Teenagers overall are similarly more likely to be employed, though those not employed at baseline see no differential employment rates.[38]

5.3. Robustness.

The results show that four years after the wage floor increases were legislated, ostensibly vulnerable workers saw substantial earnings gains on average and no lower employment rates. Appendix Figures I.14 and I.15 show that these findings hold when focusing specifically on workers employed by highly exposed independent businesses at baseline. Appendix Tables I.18 and I.19 show that the controls included in regression specification (2) attenuate the quantitative estimates but do not drive the qualitative results and demonstrate that the results are very similar using "regression adjusted" estimators of the treatment effects.

Also, we conduct placebo tests where we estimate differences in worker outcomes between treatment and control states around a placebo minimum wage increase in year 2010, (i.e. at least three years prior to the first minimum wage increases). Similar to Dustmann et al. (2021) and Paul-Delvaux (2024), this helps establish that the observed differences in worker outcomes after the minimum wage increases were a result of the policy changes and not because workers in these states would have differential outcomes over time independent of the policy changes. Reassuringly, the individual earnings paths are very similar in treatment and control states before and after the placebo minimum wage increases, as are employment outcomes (Appendix Figures I.10 and I.11).

5.4. Retention

In addition to earnings and employment, minimum wages may affect where low-earning workers work. We start with retention rates, defined as the probability that a worker re-

[38] We estimate own-wage elasticities (OWEs) for these panels as the percent change in employment over the percent change in average annual earnings. For employment, the percentage point changes displayed in Figure 8 are converted to percent changes, evaluated using the observed employment rates among control firms as a counterfactual. The percent change in annual earnings is estimated using Equation (2) where the outcome is log individual annual earnings. Standard errors are calculated using the Delta method. The associated OWEs are: -0.013 (s.e.=0.036) for all low-earning workers, 0.026 (s.e.=.044) for low-earning workers in highly exposed industries, 0.064 (s.e.=0.032) for all young individuals, and 0.038 (s.e.=0.019) for teens.

mains employed by their baseline firm after the minimum wage increases. Figure 9 plots estimates of the differential five-year retention rate for the full set of low-earning workers and subsets conditional on baseline employer type.[39] Retention rates increase significantly for low-earning workers in states that raise the minimum wage, driven by the 1.46% increase among workers in highly exposed industries. Retention rates rise similarly for low-earning workers employed by independent businesses and large corporations in highly exposed industries. These findings mirror the turnover reductions previously demonstrated for specific employers (Michael Reich and Jacobs, 2004; Coviello et al., 2022) or by worker rather than firm characteristics (Dube et al., 2016). Retention does not in rise unexposed industries that employ small shares of minimum wage workers.

Higher retention could benefit firms through reduced direct costs (search, recruitment, and training of new workers) and indirect gains, such as a more experienced and efficient workforce (Jäger et al., forthcoming). Changes in retention rates also inform the interpretation of estimated firm-level employment responses. The tax data report the number of W-2s issued by a firm to distinct workers in a given year, capturing a firm's total number of employment relationships, not the number of jobs at a firm at a given time. Let us denote this employment concept E_{jt}.

We can define the average number of "jobs" in a firm in a year as N_{jt}.[40] Let $n_{jt} \geq 1$ represent the average number of workers needed to fill a job in a year. If all positions at a firm are filled by one worker for the entire year, then n_{jt}=1; if there is worker turnover for some positions within a year, $n_{jt} > 1$. Then, $E_{jt} = N_{jt} n_{jt}$ and $ln(E_{jt}) = ln(N_{jt}) + ln(n_{jt})$. An estimated log change in employment relationships is the sum of the log change in jobs and the log change in workers needed to fill a given job. As such, an estimated reduction in employment relationships can come from a reduction in the number of jobs and/or a

[39]To estimate differential retention rates, we estimate Equation 2 using an LPM specification where the outcome is an indicator for working at the same firm in year *s* as in year *s-1*. We focus on the low-earning panel because measuring retention requires employment in the base year.

[40]A job can be defined arbitrarily and the number and composition of jobs will vary across firms. That is, N_{jt} could represent some number of full-time and/or part-time positions that a firm needs to fill to operate at its given production level.

reduction in turnover (a decrease in n_{jt}).

Higher retention implies that the estimated reduction in E_{jt} reported in Section 4 likely overstates the reduction in jobs from a firm perspective. If we assume that one less separation offsets the need for one other worker to fill that job, then the estimated 1.68% reduction in separations[41] would imply that the estimated 2% reduction in employment relationships is associated with only a 0.32% reduction in jobs. Without information on hours or tasks, we cannot directly estimate the effect on n_{jt}. As the job losses we observe are each roughly equal to one-quarter of a year of full-time work at the minimum wage, and retention rises over multiple years, it is plausible that each retained worker supplants at least one missing employment relationship such that there is no meaningful reduction in jobs at highly exposed independent businesses.

5.5. Worker Transitions

In addition to boosting worker retention, minimum wage increases affect the way workers move between employers and industries. The upper panel of Table 5 compares low-earning worker transitions in states that raised their minimum wages to transitions in control states.[42]

Low-earning workers who leave highly exposed firms ("movers") are significantly less likely to work at independent firms in highly exposed industries four years after the minimum wage increase, but are substantially more likely to work in large C-corporations in these industries. Appendix Table K.21 shows that, correspondingly, workers are more likely to move to large firms in highly exposed industries and somewhat less likely to move to small firms. These patterns are consistent with a reduction in positions available at independent businesses because of the reduced entrance rates of smaller, less productive firms and resulting reallocation toward larger firms. These offsetting effects, plus the impact of higher retention, leave low-earning workers no less likely to remain employed in highly exposed

[41] The 5-year separation rate for workers in highly exposed industries in control states is 86.9%, so the 1.46 pp estimate is associated with an estimated 1.68% reduction in reduction in separation rates in treatment states.

[42] See Appendix L for details on classifying employer types in the tax data.

industries.[43]

This direct evidence of reallocation in response to increases in the minimum wage complements the findings of Dustmann et al. (2021). We also highlight another channel by which the minimum wage affects worker flows by showing the increase in retention rates among both independent firms and C-corporations in highly exposed industries.

5.6. Reconciling Firm- and Individual-Level Employment Impacts

The transitions reported in Table 5 can help explain how the missing employment relationships at highly exposed independent firms we document in Section 4 do not translate into negative employment impacts at the individual level. The reallocation patterns indicate that the decline in employment in independent businesses is generally offset by worker movements to large C-corporations in highly exposed industries. These transitions across firm types broadly reconcile the reductions associated with fewer employment opportunities at independent businesses, but stable employment rates at the worker-level.

We can further examine the relationship between the observed effects on independent businesses and the individual employment patterns. We estimate a 3.3% aggregate reduction in employment relationships among low-earning workers at independent firms. Of these, 2% arise from less part-time hiring among incumbents and 1.3% result from reduced firm entry.[44] Table 5 reports that following the minimum wage increases, low-earning workers previously employed in highly exposed industries are 0.87 percentage points less likely to work at highly exposed independent firms. This is translates to a 1.2% reduction in employment relationships between low-earning workers and independent firms,[45] which corresponds

[43] Appendix Table K.22 shows the differential two-year transition rates. Retention effects are decreasing over time, as we would likely expect given the high turnover in these industries and among low-wage workers. Transitions from smaller (independent) firms in highly exposed industries to larger firms (C-corporations) happen fairly quickly, generally having already materialized by year $s + 2$.

[44] Based on the aggregate analysis with collapsed data described in Section 4.6, we estimate that reduced hiring at incumbent firms is associated with a 2% net reduction in employment, and reduced firm entry leads to a 4.8% overall decline in employment relationships. On average 28% of employees at highly exposed independent businesses are low-earning workers, so we estimate that $4.8\% * 28\% = 1.3\%$ of losses from lower entry are applicable low-earning workers. We know from the micro analysis in Section 4.1 that 73% of the 2% reduction in employment relationships among incumbent firms, or 1.4%, are missing hires of teenagers.

[45] The estimated 1.2% reduction in employment relationships is a weighted average of i) the 3.8% (s.e.=0.87

closely with the 1.3% decline in employment relationships arising from reduced entry of independent firms into highly exposed industries. Effectively, as workers churn within highly exposed industries (albeit at a reduced rate due to higher retention), they are more likely to re-match with a C-corporation rather than an independent business as reduced entry curtails the number of independent firms.

Reduced hiring among incumbent firms largely affects teenagers. It is important to note that the hiring reductions we document at the firm level pertain to *contemporaneous* teenagers — those who are ages 15 to 19 in year $s+4$, while our teen panel follows those who were ages 15 to 19 in year $s-1$; in effect, our teen panel estimates also include the impacts of these individuals growing older over the analysis period. To estimate contemporaneous teen employment changes between years $s-1$ and $s+4$, we complement our analysis using a repeated cross section of teenagers in treated and controls states.[46] The difference-in-difference estimates of the impact of the minimum wage increases in these repeated cross-sections is reported in the lower panel of Table 5.[47]

The analysis shows that teenagers are not significantly less likely to be employed after the minimum wage increases, and employed teens are significantly less likely to work in independent firms in highly exposed industries and significantly more likely to work at large C-corporations in these industries. The results suggest that teens have roughly 1.3% fewer employer relationships at highly exposed independent businesses after the minimum wage increase, which nearly matches the 1.4% reduction in teen hiring we estimate in the firm data.[48] We also see evidence of less multiple job holding as working teens have 2.8% fewer

p.p.) decline in the probability that low-earning workers previously employed in highly exposed industries work at highly exposed independent businesses and ii) the effectively unchanged odds low-earning workers previously employed in unexposed industries work at highly exposed independent businesses. As 32% of low-earning workers in our sample are employed by highly exposed industries at baseline, the weighted average yields a 1.2% decline in employment relationships between low-earning workers and highly exposed independent firms ($0.32 * 3.8 + 0.68 * 0 = 1.2\%$).

[46]Details of the repeated cross-section are in the data Appendix L.4

[47]It is worth noting that the point estimates of the earnings impact for contemporaneous teens are much smaller than our panel estimates, indicating that aging contributes to the panel estimates.

[48]The cross sectional analysis shows teenagers (whether working or not at baseline or in any year) have a 1 percentage point, or 7.25%, lower probability of working at highly exposed independent businesses. As teenagers are approximately 18% of workers in highly exposed independent firms, the 7.25% reduction in

employment relationships in a given year, which can further buffer estimated employment effects at the firm-level.[49]

In Appendix N, we validate these findings using public data from the American Community Survey (ACS). While the tax data are not designed to estimate employment rates *per se*, the ACS is. The tax data have the advantage of linking worker and firms over time, allowing us to estimate the dynamics across types of firms that underlie the aggregates, while the ACS provides a representative cross-section of the population regardless of employment status and allows for direct identification of minimum wage workers. The ACS data confirm the main findings derived from the tax data of negligible individual employment effects across worker types and in highly exposed industries, and show similar wage increases as in our low-earning workers panel and the repeated cross-section of teenagers.

6. Discussion

Our empirical results show that incumbent independent businesses accommodate minimum wage increases without laying off workers or facing lower profits. Instead, revenues rise enough to fully offset the added labor costs such that consumers rather than owners bear the burden of higher wage floors. Higher wage floors forestall entry, such that firms that enter despite the added costs are positively selected with higher value-added per worker and lower non-labor costs. Incumbent firms, too, become more productive and efficient after the minimum wage increase. At the aggregate level, summing across all independent businesses in highly exposed industries, workers receive a larger share of expanded revenues generated by fewer, more productive firms whose profits remain unchanged.

Appendix O presents a model of imperfect product market competition with fixed costs and heterogeneous production technology to highlight how extensive margin responses and

teen workers is associated with a 1.3% decline in total employment relationships at highly exposed firms. This corresponds closely to with the estimated reduction in employment associated with teenagers from the firm-level analysis – 73% of the estimated 2% employment reduction is from teen hiring, or $0.73 * 2\% = 1.4\%$.

[49] We estimate teens with any employment in treatment states hold 0.045 (s.e.=0.015) fewer jobs in *s+4*, evaluated at the average and median number of jobs held of 1.6.

heterogeneity can mediate observed responses to minimum wage increases. Specifically, reduced entry in our Cournot framework reallocates revenues to the more productive and efficient firms that operate in these highly exposed industries despite the minimum wage, facilitating firm revenue increases large enough to mute profit impacts. This exercise is in the style of Besley (1989) who shows that when firms can exit under Cournot competition, a specific (per unit) tax on output can increase output per firm and total welfare, depending on the shape of market demand. Unlike a specific tax, minimum wages raise the price of labor inputs so the firm's cost shock will depend directly on its production function. Firms whose production technologies rely more or less heavily on minimum wage labor, or use minimum wage labor more or less efficiently, will experience different costs shocks even if producing the same product. For this reason, we augment our framework to account for heterogeneous technologies across firms, as is conceptually and empirically relevant in our setting.

Under Cournot competition, market shares are proportional to margins, with the most efficient firms enjoying both the largest margins and market shares. Even without entry impacts, a cost shock will lead to reallocation to *ex ante* more efficient or less shocked firms. Reduced entry will further reallocate demand from firms that do not enter to operating firms, facilitating revenue increases for each firm and dampening the profit impacts.

Our estimates show that the minimum wage increases reduce firm entry by 2%, consistent with narrower margins leaving some firms unable to cover fixed costs. Entry declines among lower-productivity and less-efficient firms, meaning that entrants are positively selected. We also observe that incumbent firms become more productive and efficient. This smaller set of more productive firms sees substantial average revenue growth of 4.15% (s.e.=0.00475). Revenue growth is particularly strong among incumbents with higher baseline value-added (Appendix Table F.10). This reallocation of revenues, along with productivity gains, helps independent businesses maintain their profits despite the minimum wage. It is worth noting that the market price increase is eased by the commonality of the cost shock. While firms may vary in their reliance on low-wage labor, to the degree that all firms in highly exposed industries employ some low-wage workers, minimum wage regulations precipitate an at least partly common cost shock. In contrast to a firm unilaterally choosing to raise prices, the commonality of the minimum wage-based cost shock in these industries eases pass-through as the elasticity mediating the price increase is the (lower) elasticity of market rather than firm demand. Any reduction in customer antagonization from price increases if they can be attributed to policy rather than firm choices, will only make price increases easier by reducing the elasticity of demand.

Our framework considers how imperfect product market competition can rationalize our empirical findings. This complements models of imperfect competition in labor markets, which can also explain reallocation after a cost shock. Bhaskar and To (1999) show that free entry and monopsony imply that increases in the minimum wage raise employment per firm, cause firms to exit, and may increase or decrease industry employment and welfare. Building on Butcher et al. (2012) and like Bassier (2023), Dustmann et al. (2021) show that the reallocation of workers from smaller, less productive, lower paying firms to larger, more productive, higher paying firms is consistent with a model of monopsony power in the labor market and heterogeneous marginal costs of production. Reallocation occurs as the least efficient firms exit, while employment rises among more efficient surviving firms that paid wages below the new minimum and must now raise wages, narrowing the gap between the wage and the marginal product of labor. Potentially consistent with these models, we find that that low-earning workers and teens reallocate from independent businesses to C-corporations (Table 5) and that it is lower value-added independent businesses that reduce employment relationships (Appendix Table F.10).

Taken together, our findings and framework highlight the importance of considering firm heterogeneity, market imperfections, and extensive margin dynamics for understanding the effects of the minimum wage.

7. Conclusion

The twenty-first century has seen renewed global interest in minimum wages as a tool for intervening in low-wage labor markets and addressing rising inequality. Countries like the United Kingdom and Germany have reintroduced minimum wages, while others such as Hungary and Brazil have significantly raised their wage floors (Dube and Lindner, 2025). In the United States, 30 states have raised their minimum wages in the last decade, reflecting the broad popularity of using price floors to boost the wages of low-skilled workers.

While an extensive literature demonstrates that minimum wage increases have minimal employment impacts in the short to medium term, there is still limited understanding of how markets adjust to accommodate these wage hikes. Our study demonstrates how independent firms in the United States cope with minimum wage increases. Contrary to concerns that such wage increases might imperil small firms, our comprehensive analysis reveals that independent firms demonstrate remarkable adaptability. Despite wage pressures, these businesses do not layoff existing workers; instead, they pare-back part-time hiring as worker retention rises. They mainly accommodate the higher labor costs through added revenues, and are successful enough in generating new sales that profits are unchanged.

Our findings also highlight sector-wide implications, where minimum wage increases lead to a modest reduction in new firm entry, favoring more productive and efficient entrants. Among incumbent firms, value-added per worker rises on average and revenues grow more among firms with higher baseline value-added. As such, minimum wages reshape industries that rely on low-wage workers, such that they feature fewer, more productive firms following the cost shock. Through entry, and demand reallocation, the minimum wage shapes the productivity distribution of independent firms in a manner akin to how the emergence of new technologies (Collard-Wexler and De Loecker, 2015), exposure to international trade (Melitz, 2003), and recessions (Osotimehin and Pappadà, 2017) can shift the productivity distribution in an industry.

At the individual level, low-earning and young individuals experience overall earnings gains and stable employment levels in the wake of minimum wage increases. There are also worker transitions toward larger C-corporations. These shifts illustrate how worker mobility can mitigate potential employment effects as rising retention and reduced part-time hiring shrink job openings at independent firms.

It is important to note that our findings center on the short-to-medium-run impact of phased-in minimum wage increases. The longer-run impacts could be different if entrants eventually utilize production methods that rely less on low-wage labor and incumbents reconfigure their inputs away from the workers most affected by these policies. In addition, it is possible that firms respond to the increased labor costs along margins not estimated here, such as reducing workplace amenities or delaying equipment maintenance.

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Tables and Figures

Table 1: Summary Statistics - Independent Businesses in Highly Exposed Industries

	Means (base year)		Medians (base year)	
	Treatment	Control	Treatment	Control
Revenue ($)	1,818,455	1,738,397	784,640	766,210
Wage Bill ($)	341,419	312,114	115,780	113,280
Value-Added ($)	989,376	925,816	427,130	406,850
Owner Income ($)	124,508	121,324	54,280	51,550
Employees	52.4	43.9	14	16
Young Workers (16-26)	26	20	3	4
Share low earning workers	0.28	0.30	0.26	0.30
Wage bill / revenues (dollar weighted)	0.19	0.18	0.18	0.18

Note: This table reports summary statistics for our estimation sample of 135,163 independent businesses in highly exposed industries in treatment and control states measured in the base year, defined as the year prior to the minimum wage increase for that state. For confidentiality, dollar values of medians are rounded to $100 or $10 and statistics on the number of workers include more than 10 firms with the reported value of workers.

Table 2: Incidence of Minimum Wage Increases

Cost burden to firm:	All exposed	Restaurants	Other/retail
Wage bill	0.0146***	0.0206***	0.0078***
Financed by consumers:			
Revenue	0.0337**	0.0295**	0.0363*
COGS	0.0132***	0.0053	0.0209***
Other deductions	0.0036	0.0027	0.0041
Expensed investment	0.0007	0.0003	0.0010
Financed by owners:			
Owner Profits	0.0001	-0.0001	0.0005
Net share by consumers:	100%	100%	100%
Net share by owners:	0%	0%	0%

Note: This table compares estimated changes in wage bills and other input costs to estimated revenue increases to understand who bears the burden of higher wage floors. Net shares of the burden of the higher labor costs borne by consumers and owners are calculated using the regression estimates. Each estimate is from a firm-level regression of Equation (1) where the outcome is the variable defined in each row scaled by baseline ($s-1$) revenue. The coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. Standard errors are clustered at the state-by-cohort level. All raw reported firm values are winsorized from above and below at the 1% level. Regressions are weighted by log baseline revenue.

Table 3: Cost Structure and Productivity Impacts of Minimum Wage Increases

	Wage bill/revenue		Material costs/revenue		Value added/worker	
	Low (Q1)	High (Q4)	Low (Q1)	High (Q4)	Low (Q1)	High (Q4)
Entrants	-0.0219***	0.0274***	0.0329	-0.0246**	-0.0525***	0.0428***
	(0.00806)	(0.00722)	(0.0203)	(0.0121)	(0.0144)	(0.0140)
Incumbents	0.00121	0.0302***	0.0200***	0.00186	-0.00749***	0.0269***
	(0.00256)	(0.00460)	(0.0057)	(0.0030)	(0.00248)	(0.00899)
All firms	0.000775	0.0227***	0.0184***	-0.00109	-0.0164***	0.0207***
	(0.00243)	(0.00419)	(0.0061)	(0.0023)	(0.00239)	(0.00693)

Note: This table describes changes in distribution of labor shares, materials cost efficiency and productivity among incumbent firms and entrants in treatment states relative to control states after the minimum wage increases. Materials costs per dollar revenue are the sum of COGS and Other Deductions scaled by revenue. Value-added per worker is revenue less COGS scaled by the number of employees. Using the baseline distributions of wage bill per revenue dollar, materials costs per revenue dollar and value-added per worker, we define the top and bottom quartiles of labor use, other input efficiency, and productivity. We then estimate regression equation (1) where the outcome variable is an indicator for the firm being in quartile q of a specific measure in year $s+4$. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include indicators for two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. Standard errors are clustered at the state-by-cohort level.

Table 4: Aggregate Effects on Independent Businesses in Highly Exposed Industries

	Revenue	Wage bill	Material costs	Profits	Value added/worker
Aggregates	0.0261	0.0111*	0.0133	-0.0013	0.0763*
(share of baseline revenue)	(0.0202)	(0.0065)	(0.0131)	(0.0026)	(0.0462)
Averages	0.0415***	0.0751***	0.0214***	0.0124*	0.0443***
(logs)	(0.00475)	(0.00536)	(0.00577)	(0.0069)	(0.0043)

Note: This table presents estimates from the full, unbalanced panel of independent businesses in highly exposed industries (≈271,308 firms/year). Materials costs are the sum of COGS and Other Deductions. Value-added per worker is revenue less COGS scaled by the number of employees. The first row presents aggregate statistics summing over all independent firms in highly exposed industries. These estimates use collapsed state-by-industry data with state-by-industry totals of income statement items scaled by base year ($s-1$) total revenue as the outcome variables. Appendix L details the specification. All aggregate regressions are weighted by baseline total revenue. The lower row reports average log impacts in the unbalanced panel. The estimates are from a specification similar to Equation (1), which includes cohort-by-year fixed effects and indicators for two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. All standard errors are clustered at the state-by-cohort level.

Table 5: Employment Patterns of Low-Earning Workers and Teenagers

Panel A: Low Earning Workers Panel

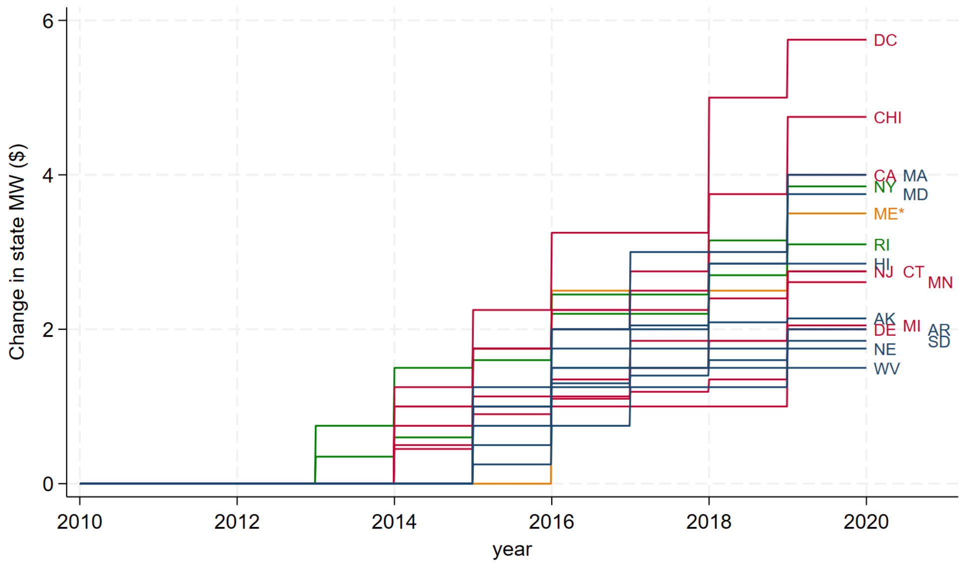
Baseline Industry	Retain	Exposed Industry	Exposed Industry Independent Business	Exposed Industry Large C-corp.	Unexposed Industry Independent Business	Unexposed Industry Large C-corp.
All Workers						
Exposed industries	0.0146***	0.0071	-0.0087	0.0128*	0.001	-0.0043
	(0.0044)	(0.0044)	(0.0061)	(0.0072)	(0.0022)	(0.0034)
Unexposed industries	-0.0044	-0.0035	-0.0013	-0.0020*	0.0085**	0.0039
	(0.0044)	(0.0030)	(0.0020)	(0.0010)	(0.0040)	(0.0051)
Movers						
Exposed industries	.	-0.0058	-0.0189**	0.0120*	0.0019	-0.0048
	(.)	(0.0047)	(0.0076)	(0.0076)	(0.0026)	(0.0038)
Unexposed industries	.	-0.0036	-0.0009	-0.0023**	0.0056	0.0042
	(.)	(0.0037)	(0.0026)	(0.0011)	(0.0046)	(0.0057)

Panel B: Teens, repeated cross-section

	Earnings ($)	Employed	Exposed Industry Any	Unexposed Industry Any	Exposed Industry Independent Business	Exposed Industry Large C-corp.
All Teens	112	-0.0050	-0.0094	0.0044	-0.0102**	0.0028
	(98)	(0.0061)	(0.0065)	(0.0028)	(0.0040)	(0.0026)
Employed teens	305	.	-0.0057	0.0057	-0.0121*	0.0088**
	(197)	(.)	(0.0096)	(0.0096)	(0.0071)	(0.0044)

Note: The table above reports employment patterns for individuals from our low-earning panel and repeated cross-sections of teenagers. Employment patterns are tracked by industry and firm type. Panel A describes the impact of minimum wage increases on employment transitions between years $s-1$ and $s+4$ for a panel of low-earning workers. These workers are all employed at baseline in either exposed or unexposed industries and the estimates report the change in their transition patterns relative to similar workers in untreated states. Each row describes transition patterns conditional on the industry in which low-earning workers were employed in the base year. Movers columns focus on transition patterns of workers no longer employed by their baseline $(s-1)$ employer in year $s+4$. Estimates are from a linear probability model specification of Equation (2) where the dependent variable is an indicator for a worker being employed at a given type of firm in year $s+4$. The lower panel describes employment patterns for randomly sampled repeated cross-sections of teenagers. Appendix L.4 details the construction of these data. The reported estimates convey five-year changes in average employment rates in different industries and firm types measured between years $s-1$ and $s+4$. All standard errors are clustered at the state-cohort level.

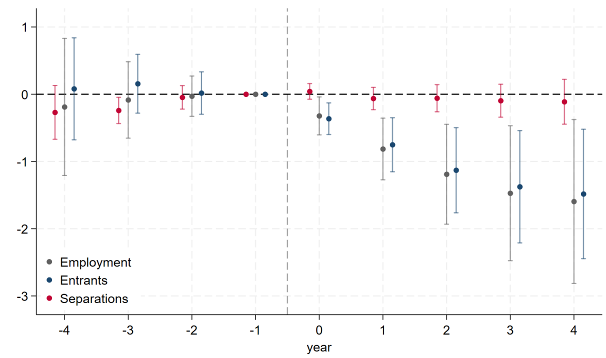
Figure 1: State Minimum Wage Changes



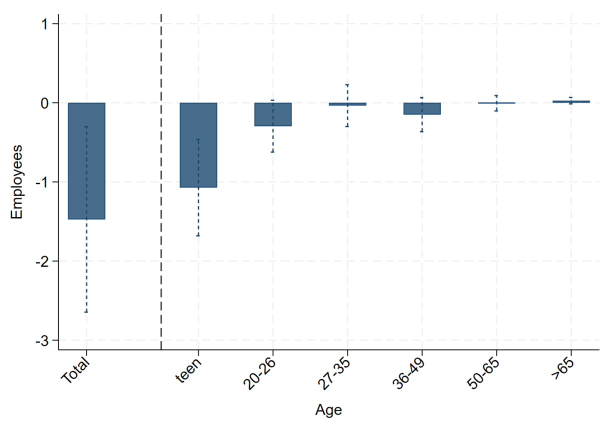
Note: This figure tracks changes in the statutory minimum wage in the 18 states and the city of Chicago whose policy changes provide the identifying variation for the analysis. These states all increased their minimum wages starting between 2013 and 2016. Most minimum wage increases were phased-in as indicated by the step-wise changes over time. *Maine's state minimum wage increased in 2017, but Portland, Maine's largest city, increased it's minimum wage in 2016 in anticipation of the state change so we treat the pre-reform period for Maine as the years before 2016.

Figure 2: Change in Employment Relationships Due to Minimum Wage Increases

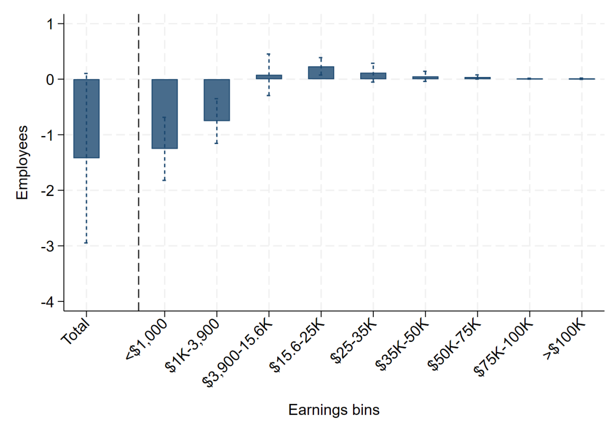
(a) Change in Number of Employment Relationships



(b) Employment Impact by Age



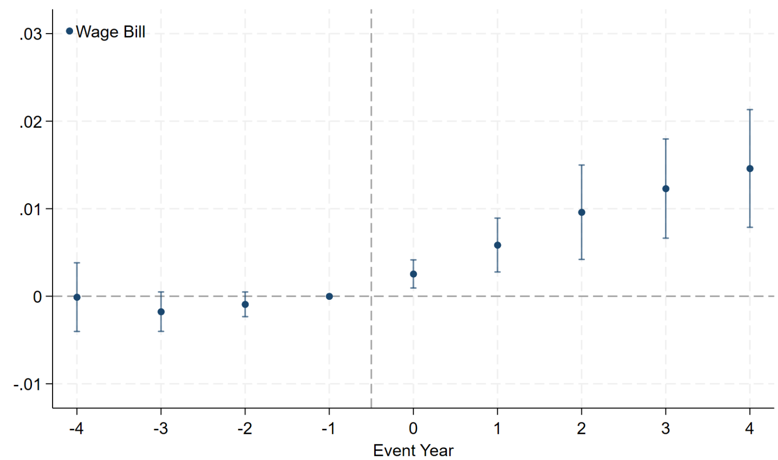
(c) Employment Impact by Earnings



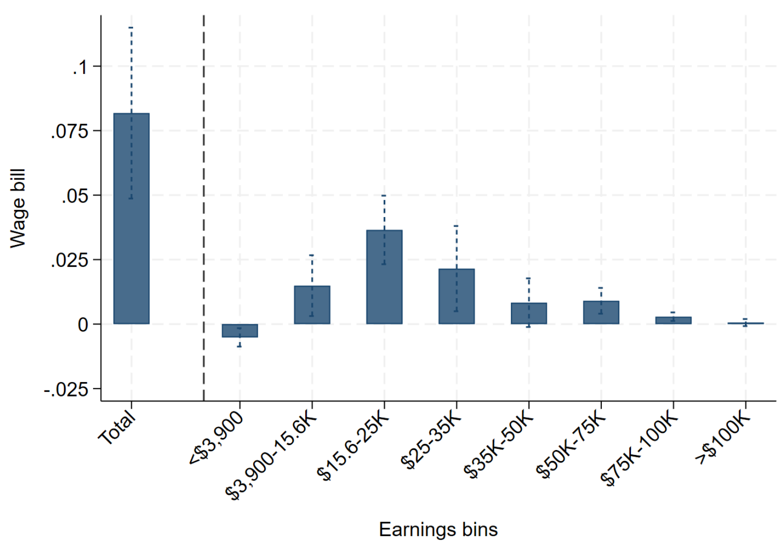
Note: This figure describes the change in the number and composition of employment relationships due to the minimum wage. Our employment measure counts the number of W-2s attached to a firm each year. Time $s = -1$ is the year prior to the minimum wage increase for each cohort of states. Panel (a) traces how minimum wage increases affect the number of employment relationships of the average firm as well as the number of new (entrants) and departing (separations) workers. Plotted coefficients are estimates of β_s from (1), which convey the change employment relationships relative to base year $s-1$, for the average treated firm compared to the average control firm. Entrants capture the number of new hires, defined as employees of the firm in year s that were not with the firm in year $s-1$ while separations describe those at the firm in year $s-1$ that were not with the firm in year s. This decomposition is such that the coefficients from the entrants and separations specifications sum to the net employment response. Panels (b) and (c) decompose the $s-1$ to $s+4$ change in total employment relationships by worker age, and annual earnings, respectively. Each bar is the difference-in-difference estimate from a regression where the outcome is the five-year change in the number of workers in each category relative to the base year number of workers for firm j. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Employment counts are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure 3: Impact of Minimum Wage Increases on Firm Wage Bills

(a) Changes in Average Wage bill

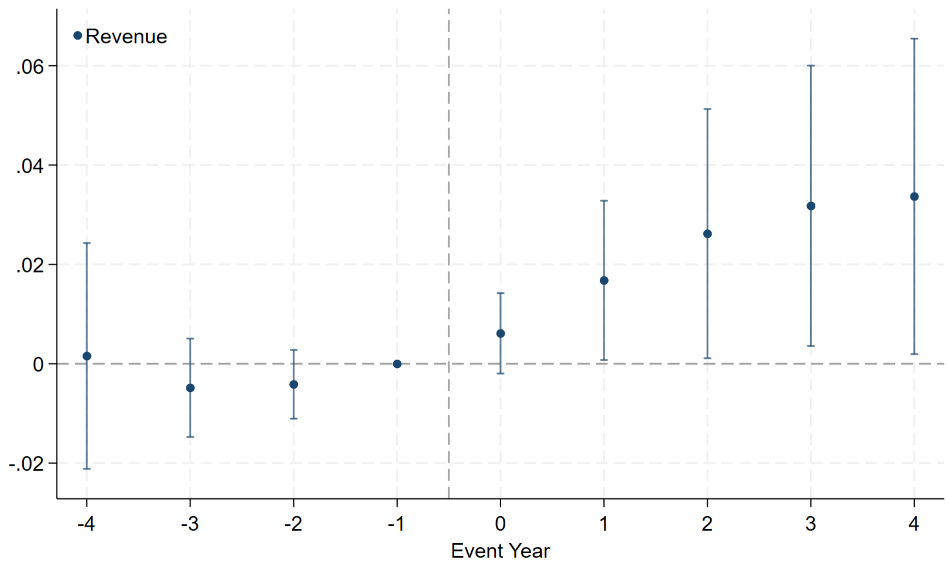


(b) Wage Bill Increases by Employee Income, 2013 to 2018



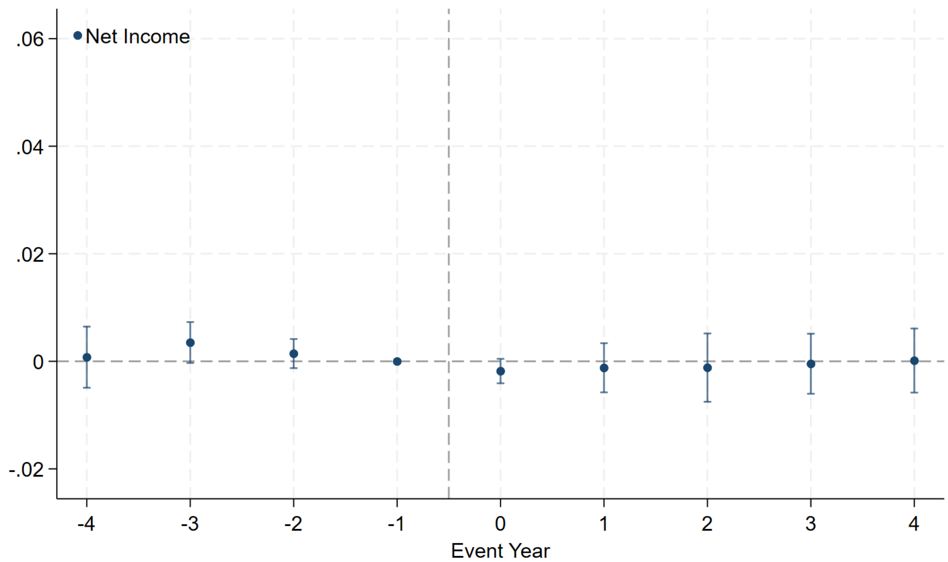
Note: This figure presents the estimated effects of the state minimum wage increases on average firm wage bills overall (Panel (a)) and by worker earnings (Panel (b)) for independent businesses in highly exposed industries. Panel (a) traces out the difference over time in average scaled wage bills relative to the base year $s-1$ between firms in treated and untreated states. Estimates are from a regression of Equation (1) where the dependent variable is the firm wage bill scaled by baseline revenue in year $s-1$ and plotted coefficients are estimates of β_s. Panel (b) shows the decomposition of the total five-year wage bill response from year $s-1$ to year $s+4$ across the worker earnings distribution. The total wage bill effect is shown on the left, and the gains by earnings range are on the right. Each bar is the difference-in-difference coefficient estimate from a regression where the outcome is the five-year change in the wage payments in each earnings bin scaled by baseline revenue for firm j. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and wage bill-to-base year revenue ratios are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure 4: Impact of Minimum Wage Increases on Firm Revenues



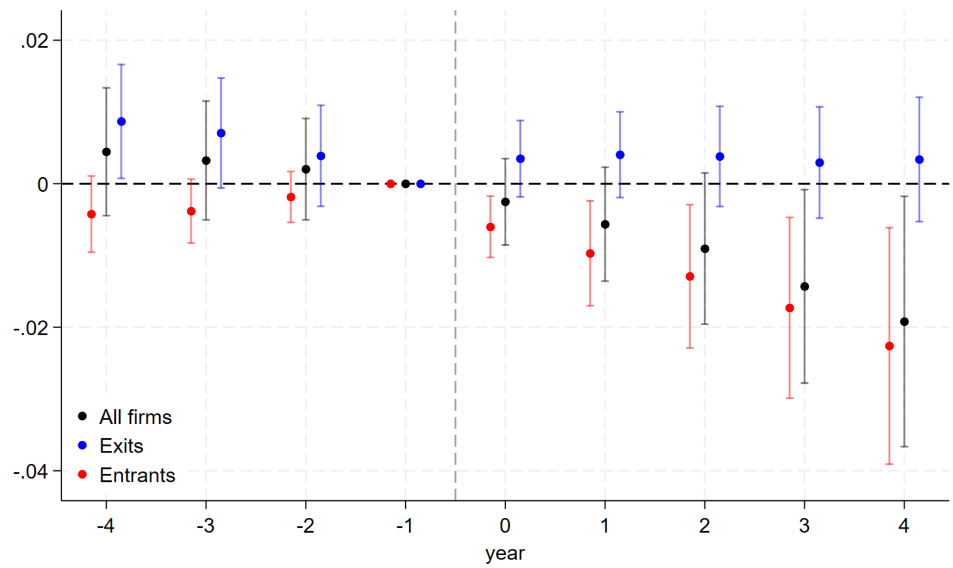
Note: This figure plots the estimated effects of the state minimum wage increases on firm revenue. Plotted coefficients are estimates of β_s from (1) where the outcome is firm revenue scaled by revenue in base year $s-1$. The coefficients trace the difference over time in scaled revenue relative to the base year $s-1$ between firms in treated and untreated states. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and scaled revenue ratios are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure 5: Impact of Minimum Wage Increases on Firm Profit



Note: This figure above plots the estimated effects of the state minimum wage increases on firm profits (net business income) scaled by base year revenue. Plotted coefficients are estimates of β_s from (1), which trace out the difference over time in average scaled profit relative to the year prior to the policy change between firms in treated and untreated states. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and profit-to-base year revenue ratios are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

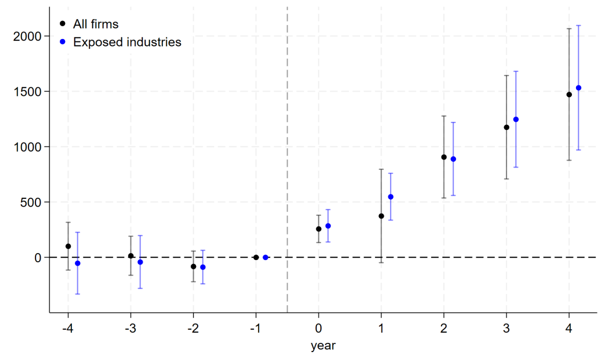
Figure 6: Impact of Minimum Wage Increases on Firm Entry and Exit



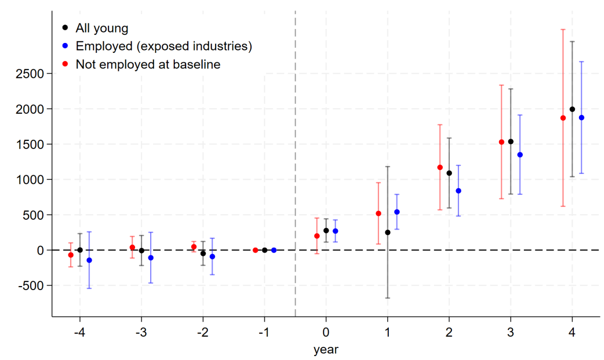
Note: This figure plots the impact of minimum wage increases on firm exit, entry and firm count. Section 4.5 and Appendix L.5 describe the collapsed data used for this analysis as well as the regression specification. Plotted coefficients are estimates of β_s and trace out the difference over time in the number of firms, entrants and exits relative to the year prior to the policy change between treated and untreated states. In addition to cohort-by-year fixed effects, the specifications include baseline two-digit industry, quintiles of county density, quintiles of county employment rates, and quintiles of commuting zone pre-period firm churn – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by base year number of firms in each collapsed cell and standard errors clustered at the state-by-cohort level.

Figure 7: Impact of Minimum Wage Increases on Earnings Trajectories

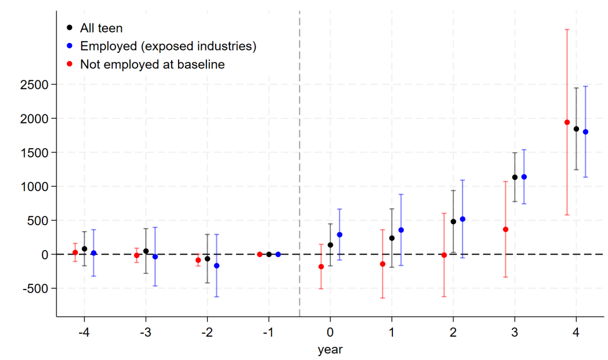
(a) Low-Earning Individuals



(b) Young Individuals, Ages 15-26 years



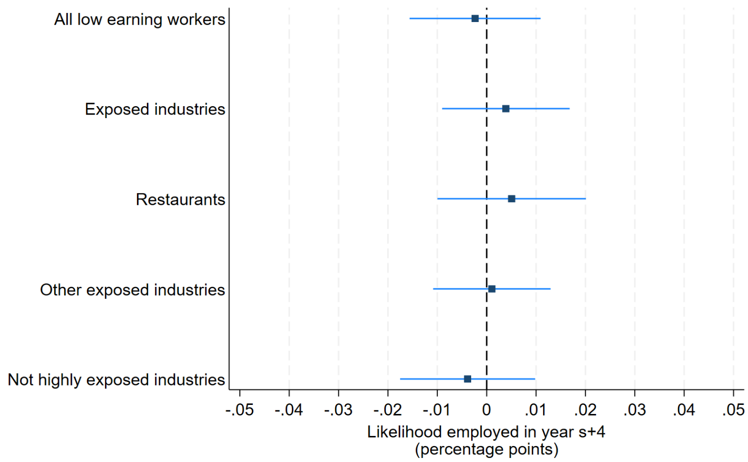
(c) Teenage Individuals, Ages 15-19 years



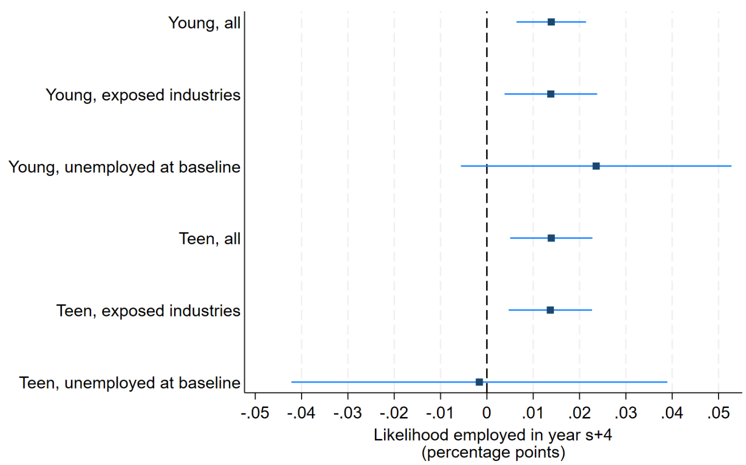
Note: This figure demonstrates the impact of minimum wage increases on the evolution of average earnings for low-earning and young workers. Plotted coefficients are estimates of β_s from Equation (2) where the dependent variable is annual earnings. Panel (a) plots the difference in the earnings trajectories of low-earning workers (<20,000 but >0 in year $s-1$, and <25,000 in year $s-2$) in treatment relative to control states. Trajectories are plotted separately for all low-earning individuals and those employed in highly exposed industries in base year $s-1$. Panel (b) plots the impact on earnings trajectories for young individuals ages 15-26 in base year $s-1$. These individuals may or not be working at baseline and their earnings trajectories are plotted separately by base year employment status. Panel (c) focuses on individuals who were teen age and may or may not have been working at baseline. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure 8: Impact of Minimum Wage Increases on Individual Employment

(a) Low-Earning Workers

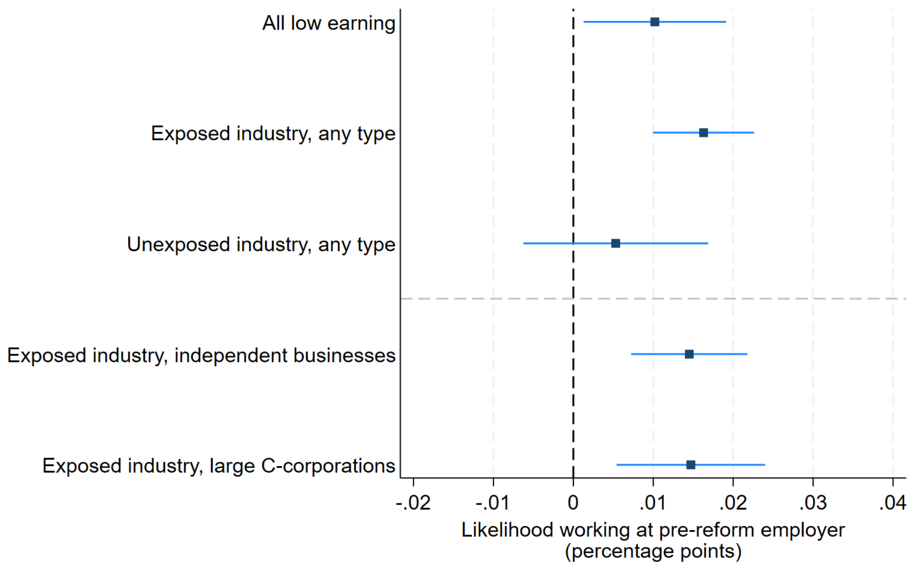


(b) Young Individuals, Ages 15-26 years



Note: This figure demonstrates the impact of minimum wage increases on employment four years after the minimum wage increases for baseline low-earning workers and young individuals. Estimates are from linear probability model regressions of (2) where the dependent variable is an indicator if the individual receives a W-2 in year s. Plotted coefficients are estimates of the average change in employment probability for each group between the base year prior to the policy change $s-1$ and four years hence $s+4$. Panel (a) plots the employments impacts on low-earning workers overall and for subgroups divided by baseline employer type. Panel (b) describes employment effects for young individuals ages 15-26 in base year $s-1$. These individuals may or not be working at baseline and estimates are reported separately by baseline employment and age. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure 9: Impact of Minimum Wage on Worker Retention



Note: This figure demonstrates the impact of minimum wage increases on retention rates for low-earning workers. Low-earning workers are those employed at baseline but earning <\$20K and also either not working or earning <\$25K the year prior in $s-2$. Plotted coefficients are from linear probability model regressions of (1) where the dependent variable is an indicator equal to one if a worker has the same employer in year $s+4$ as at baseline $s-1$. Estimates are reported separately by industry and type of employer at baseline. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Online Appendix

This appendix includes several sections of supplementary information and results. Appendix A describes the details of the minimum wage increases we study. Appendix B explains how we screen for highly exposed industries using public data from the Current Population Survey. Additional summary statistics are provided in Appendix C. We compare our own-wage elasticities estimates to estimates from previous studies in Appendix D. Appendix E reports detailed estimates for all firm outcomes and impacts on firm deductions outside of wage bills and materials costs. Appendix F examines the heterogeneity of firm responses. Appendix G breaks down firm responses by industry. The robustness of the firm results is discussed in H, while the robustness of the individual results is examined in I. Appendix J reports alternative firm and individual estimates using regression adjusted estimators. Additional worker transition analysis is presented in Appendix K. We detail the sources and construction of our IRS data samples in Appendix L. Appendix M reports estimates from an alternative border design. Appendix N presents results from a parallel analysis of individual earnings and employment responses using public ACS data. Finally, Appendix O presents a theoretical framework consistent with our empirical results.

A. Minimum Wage Changes and Control States

Our analysis examines the impacts of the 19 state and city minimum wage increases that began between 2013 and 2016 and did not exempt small firms. Along with 17 state-level changes, we also include Chicago's minimum wage increase from 2015 and Washington D.C.'s increase from 2014. We exclude Arizona's 2017 \$1.95 increase in its minimum wage as the Arizona wage floor does not apply to firms with revenues less than \$500K.

Our set of control states comprises all states that did not adjust their minimum wages between 2011 and 2019. Appendix Table A.1 lists the treatment and control states used in our analysis along with details of the size of each minimum wage increase. Note, Illinois and Nevada had small minium wage increases in the first year of our sample period, 2010, of 25 cents and 70 cents respectively. In our regression specifications, we include an indicator variable for these state-years, meaning that these control states do not contribute to the counterfactual for event-year $s-4$ for the 2013 cohort of minimum wage increases. The timing and details of the policy changes are further described in Appendix Table A.2 below.

Table A.1: States used for Minimum Wage Analyses

Treatment				Control
state	cohort	MW increase ($)	MW increase (%)	state
NY	2013	$2.45	33.8%	AL
RI	2013	$2.20	29.7%	GA
CA	2014	$3.00	37.5%	ID
CT	2014	$1.85	22.4%	IL (non-Chi)
DC	2014	$5.00	60.6%	IN
DE	2014	$1.00	13.8%	KS
MI	2014	$1.85	25.0%	LA
MN	2014	$2.40	33.1%	MS
NJ	2014	$1.35	18.6%	NC
AK	2015	$2.14	29.5%	ND
AR	2015	$2.00	27.6%	NH
HI	2015	$2.85	39.3%	NV
Chicago,IL	2015	$4.75	57.6%	OK
MA	2015	$4.00	50.0%	PA
MD	2015	$3.75	51.7%	SC
NE	2015	$1.75	24.1%	TN
SD	2015	$1.85	25.5%	TX
WV	2015	$1.50	20.7%	UT
ME	2016	$3.50	46.7%	VA
avg (firm weighted)		$2.64	34.6%	WI
avg (unweighted)		$2.59	34.1%	WY

Note: The table above lists the treatment states and localities with minimum wage increases and the control states used in the analysis. Each minimum wage increase is grouped into a cohort based on the initial year of the increase. The table details the size of the increase four years after the policy change ($s+4$) in dollar terms and as a percentage increase relative to the year prior to the minimum wage increases ($s-1$).

Table A.2: Details of Minimum Wage Increases Used in Analysis

State	Enactment	Effective	Enacted MW	Prior MW	Phase-In	Exclusions	Next MW Change	
							Date	Details
NY	03/29/13	12/31/13	\$8.00	\$7.25	\$8.75 on 12/31/14 and \$9.00 on 12/31/15	–	12/31/16	MW in NYC for firms with more than 10 employees increases to \$11.00 on 12/31/16, to \$13.00 on 12/31/17, to \$15.00 on 12/31/18. MW increases were lower outside NYC, and fast food workers were subject to higher MWs.
RI	06/11/12 07/16/13 07/03/14 06/22/15	01/01/13 01/01/14 01/01/15 01/01/16	\$7.75 \$8.00 \$9.00 \$9.60	\$7.40 \$7.75 \$8.00 \$9.00	–	–	09/03/17	MW increases to \$10.10 on 01/01/18 and to \$10.50 on 01/01/19
CA	09/25/13	07/01/14	\$9.00	\$8.00	\$10.00 on 01/01/16	–	04/04/16	MW increases to \$10.50 on 01/01/17 and \$11 on 01/01/18 for employers with 26+ employees, then increases by \$1.00 a year through 2022 when it reaches \$15 and is then inflation indexed; employers with 25 or fewer employees have a 1-year delay for all increases

CT	06/06/13	01/01/14	$8.70	$8.25	$9.00 on 01/01/15	–	03/27/14	MW increases to $9.15 on 01/01/15, then to $9.60 on 01/01/16, and finally to $10.10 on 01/01/17
DC	01/15/14	07/01/14	$9.50	$8.25	$10.50 on 07/01/15 and $11.50 07/01/16	–	06/27/16	MW increases to $12.50 on 07/01/17 and gradually increase to $15.00 in 2020
DE	01/30/14	06/01/14	$7.75	*$7.25*	$8.25 on 06/01/15	–	07/01/18	MW increases to $8.25 on 10/01/18, to $9.25 on 10/01/19, to $9.75 on 10/01/20 and then to $10.25 on 10/01/21
MI	05/27/14	09/01/14	$8.15	$7.40	$8.50 on 01/01/16, $8.90 on 01/01/17 and $9.25 on 01/01/18	Firms with only a single employee	12/04/18	MW increases to $9.45 on 03/29/19, then potentially rises to $12 in 2030
MN	04/14/14	08/01/14	$8.00	*$7.25*	$8.00 on 08/01/14, $9.00 on 08/01/15 and $9.50 on 8/01/16	Firms with gross revenues less than $500K required to pay roughly 80% of standard minimum wage	12/17/17	MW increases with cost of living beginning on 01/01/18
NJ	11/05/13	01/01/14	$8.25	*$7.25*	Indexed to inflation	–	02/04/19	MW increases to $10.00 on 07/01/19, $11.00 on 01/01/20, with $1.00 increases until 01/01/24 when MW will be 2024

AK	11/04/14	01/01/15	$8.75	$7.75	$9.75 on 01/01/16, and increased by cost of living or to $1.00 above the federal minimum wage if it is higher thereafter.	–	–	–
AR	11/06/14	01/01/15	$7.50	$7.25	$8.00 on 01/01/16 and $8.50 on 01/01/17	Firms with fewer than four employees	11/04/18	MW increases to $9.25 on 01/01/19, to $10.00 on 01/01/20, and to $11.00 on 01/01/21
HI	05/23/14	01/01/15	$7.75	$7.25	$8.50 on 01/01/16, $9.25 01/01/17 and $10.10 01/01/18	–	09/29/22	MW increases to $12.00 on 10/01/22, to $14.00 on 01/01/24, to $16.00 on 01/01/26, and to $18.00 on 01/01/28
IL-Chicago	12/02/14	07/01/15	$10.00	$8.25	$10.50 on 07/01/16, $11.00 on 07/01/17, $12.00 on 07/01/18, and $13.00 on 07/01/19	–	06/06/23	MW increases to $15.80 for employers with 21 or more employees and $15.00 for employers with 4 to 20 employees on 07/01/23
MA	07/06/14	01/01/15	$9.00	$8.00	$10.00 on 01/01/16, $11.00 on 01/01/17	–	06/29/18	MW increases to $12.00 on 01/01/19, $12.75 on 01/01/20, $13.50 on 01/01/21, $14.25 on 01/01/22, $15.00 on 01/01/23
MD	05/05/14	01/01/15	$8.00	$7.25	$8.25 on 07/01/15, $8.75 on 07/01/16, $9.25 on 07/01/17 and $10.10 on 07/01/18	–	03/28/19	MW increases to $11.00 on 01/01/20, to $11.75 on 01/01/21, to $12.50 on 01/01/22, to $13.25 on 01/01/23, to $14.00 on 01/01/24

NE	11/04/14	01/01/15	$8.00	$7.25	$9.00 on 01/01/16	–	11/03/20	MW increases to $10.00 on 01/01/23, to $12.00 on 01/01/24, to $13.50 on 01/01/24, to $15.00 on 01/01/26, then increases with cost of living beginning on 01/01/27
SD	11/04/14	01/01/17	$8.50	$7.25	Increased by cost of living thereafter	–	–	–
WV	02/05/14	01/01/15	$8.00	$7.25	$8.75 on 01/01/16	–	–	–
ME	11/08/16	01/01/17	$9.00	$7.50	$10.00 on 01/01/18, $11.00 on 01/01/19, $12.00 on 01/01/20, and increased by cost of living beginning on 01/01/21	–	–	–

Note: The table above details the minimum wage increases used to identify our estimates. The first column lists the enactment date and the second column lists the date the policy went into effect. The third and fourth columns show the new minimum wage and the prevailing prior minimum wage. The next column describes any phase-in of minimum wages legislated on the enactment date. The next column describes any exclusions and the final columns detail any further minimum wage policies enacted subsequent to the studied policy.

B. Screening Industries Using CPS Data

While the IRS data comprehensively describe the revenues, costs and profits of firms and the income histories of their workers, tax data do not include wage measures. Wage data are necessary to identify the industries in which minimum wage workers are most prevalent — and thus where minimum wage increases will have discernible impacts.

To screen for industries with relatively high shares of minimum wage workers, we turn to public Current Population Survey (CPS) data, which records the wage rates of surveyed workers. Specifically, we use the Monthly Outgoing Rotation Groups of the CPS to measure the share of minimum wage workers in each four-digit industry. We focus on treatment states and localities in the year before each jurisdiction's minimum wage increase to measure which industries were most likely to be impacted by the legislation. For each industry i in state or locality j that raised its minimum wage in event time s, we calculate:

$$share_{is-1} = \frac{\sum_j n_{ijs-1}}{\sum_i \sum_j n_{ijs-1}} \tag{B.1}$$

where n_{ijs-1} is the number of non-allocated workers employed in industry i in state j paid the prevailing state minimum wage or less in the year before the state or locality raises its minimum wage. The numerator sums across jurisdictions to determine the total number of minimum wage workers in the industry across all treated states and localities in the relevant base years. The denominator reports the aggregate total number of non-allocated workers paid the minimum wage or less in treated jurisdictions in the base year. It is worth noting that these state-level worker counts come from different calendar years as we aim to measure the share of potentially affected workers in the year before the minimum wage increase, that is in event time, -1.

The CPS uses Census 2007 industry codes and we calculate the above minimum wage worker shares at the 4-digit Census Industry Code level. The IRS data, on the other hand, characterizes firm industries according to the 2017 NAICS industry classification system. As such, we map the minimum wage worker shares we construct from CPS data from Census 2007 industry codes to 4-digit 2017 NAICS codes. To do so we first map the CPS industries codes to the 2012 NAICS codes using this crosswalk, then map 2012 NAICS codes to the 2017 NAICS codes used by the IRS. Our analysis includes all industries where the share of minimum wage workers in the industry exceeds 1%, which are listed in Table B.3. We describe these industries as "highly exposed" to minimum wage policies. Collectively they account for more than two-thirds of minimum wage workers.

Table B.3: Industries with at least 1% of minimum wage workers, Current Population Survey (Monthly Outgoing Rotation Groups)

Census 2007	Industry Name	Share of MW Workers	2017 NAICS
290	Support activities for agriculture and forestry	0.01001	1151, 1152, 1153
8690	Drinking places, alcoholic beverages	0.01019	7224
7890	Other schools and instruction, and educational support services	0.01044	6116, 6117
7860	Elementary and secondary schools	0.01129	6111
7870	Colleges and universities, including junior colleges	0.01289	6112, 6113
7690	Services to buildings and dwellings	0.02138	5617
1680	Cut and sew apparel manufacturing	0.01685	3152, 3159
8660	Traveler accommodation	0.02056	7211
5170	Clothing stores	0.02401	4481
5380	Department stores and discount stores	0.02771	4522
4970	Grocery stores	0.04102	4451
8590	Other amusement, gambling, and recreation industries	0.04607	7131, 7132, 7139
8680	Restaurants and other food services	0.42242	7223, 7225
Total		**0.67484**	

Note: This table lists four-digit NAICS industries that employ at least 1% of workers paid at or below the state minimum wage in the year prior to the minimum wage increase. Tabulations are from the Current Population Survey Monthly Outgoing Rotation Groups (CPS MORGS) and describe the ratio of non-allocated workers paid at or below the minimum wage in an industry divided by the total non-allocated number of workers paid at or below the minimum wage in aggregate (across all industries). The state-level worker counts come from different calendar years as the minimum wage increases begin in different years. Because the CPS MORGS uses Census 2007 industry codes while the IRS data uses 2017 NAICS codes we use a crosswalk to map between the datasets. We do not include employment services in our set of analyzed industries as we do not know whether the industry actually employing these temporary workers is highly exposed. We also exclude crop production due to substantial labor market differences relative to other industries, such as strong seasonality, and because the impact on this industry would be almost solely identified by the California minimum wage increase.

C. Additional Summary Statistics

Appendix Table C.4 reports the share of firms that are pass-throughs by size. The two right-most columns present the overall size distribution of all firms, pass-throughs and non-pass-throughs, as well as the percentage of total employment accounted for by firms within each size range. The first two columns show the percentage of firms and employment within each size range specific to pass-through firms. Summary statistics for the individual panels of low-earning workers and young individuals are presented in Appendix Table C.5.

Table C.4: Pass-Through Businesses and All Businesses by Size

	Pass-through shares		Shares of total by size	
Number of employees	share of firms	share of employment	share of firms	share of employment
0-4	84%	83%	62%	5%
5-9	81%	80%	17%	6%
10-19	78%	78%	10%	7%
20-99	76%	75%	9%	17%
100-499	68%	67%	1%	13%
500+	53%	24%	0.4%	52%
<20	83%	80%	89%	18%
<100	82%	78%	98%	35%
<500	82%	75%	99.6%	48%
Total	82%	49%	100%	100%

Source: Authors' calculations using the *Census County Business Patterns tables by Legal Form of Organization (LFO), Employment Size and NAICS Sector*, the SOI Integrated Business Data and Census's SUSM Annual Data Tables by Establishment Industry.

Table C.5: Summary Statistics - Individual Panels (base year means)

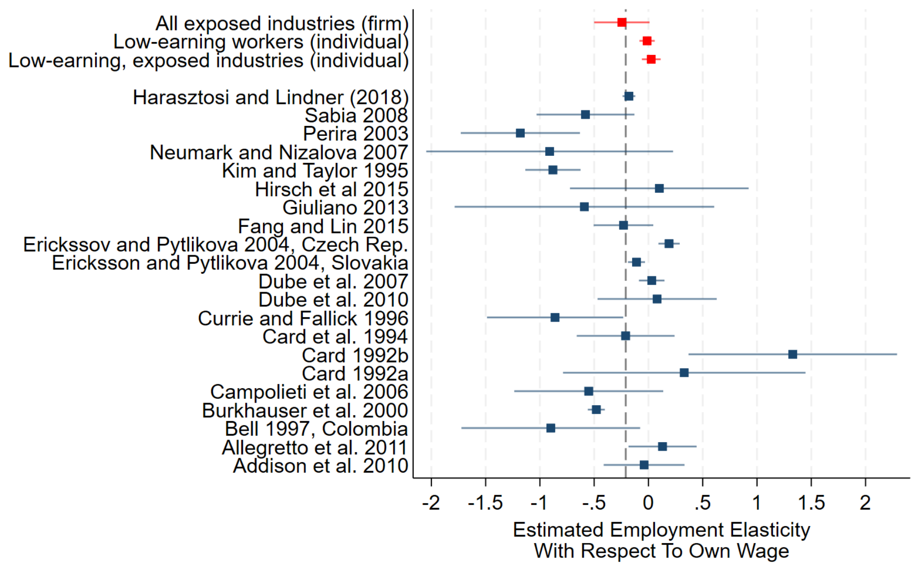
	Low earning workers panel		Young Individuals panel			
	Low earning all	Low earning exposed	Young all	Young exposed	Teen all	Teen exposed
Earnings ($)	8,244	7,760	16,153	11,369	5,565	5,563
Age	31.5	27.70	22.28	21.317	18.05	18.02
young share	0.50	0.64	1	1	1	1
teen share	0.16	0.25	0.18	0.29	1	1
N jobs	1.6	1.6	1.6	1.8	1.5	1.6

Note: This table presents baseline means for the low-earning worker panel and the panel of young individuals in treatment states in pre-reform year $s-1$. Means are reported for all industries and highly exposed industries. Earnings represent the sum of wage and salary earnings across all jobs; young and teen shares are the percent of each sample that are 15-26yrs old and 15-19yrs old, respectively; "N jobs" is the number of distinct employers in the year.

D. Comparison of OWE estimates with the Literature

We generate own-wage elasticity (OWE) estimates from both the firm data describing employment at highly exposed independent businesses, and the individual data tracking low-earning workers. We plot these OWE estimates in red alongside OWE estimates from previous studies in blue.

Figure D.1: Employment Elasticity Comparison



Note: This figure compares the own-wage employment elasticities (OWEs) estimated in this study to those from previous studies. The point estimates and 95% confidence intervals from this study are at the top in red. The top estimate is the OWE from the main sample of independent businesses in highly exposed industries (from the firm-level analysis). The second two are OWE estimates from the low-earning worker panel (individual-level analysis) — one for all baseline low-earning workers and one for low-earning workers employed in highly exposed industries at baseline. We estimate firm-level OWEs by separately estimating the change in log employment and the change in log average wages, using Eq. (1), and dividing the change in log employment by the change in log average wages. Individual panel OWEs are analogously calculated as the percent change in employment over the percent change in average annual earnings. For employment, the percentage point changes displayed in Figure 8 are converted to percent changes, evaluated using the observed employment rates among control firms as a counterfactual. The percent change in annual earnings is estimated using Specification (2) where the outcome is log individual annual earnings. Each estimate represents the change in employment four years after the minimum wage increases. The standard errors for each are calculated using the Delta method. The comparisons to previous literature are direct replicates of Appendix Figure A.6 from Harasztosi and Lindner (2019).

E. Impacts on Other Costs and Detailed Estimates

Appendix Table E.6 reports estimates of the impact of higher state minimum wages on other deductions such as officer compensation or depreciation expenses among pass-through firms in highly exposed industries. While wage bills and COGS rise with the minimum wage as reported in the main text, no other costs are meaningfully impacted by the minimum wage increases, though pension and interest expenses exhibit a statistically significant increase.

Appendix Table E.7 reports four-year estimates of all firm outcomes for the balanced panel of independent businesses in all highly exposed industries and by industry group, as well as for an unbalanced panel.

Table E.6: Effect on Other Income Statement Items

			Outcomes scaled by baseline revenues, Δ *s-1* to *s+4*					
	Officer comp	Rent deductions	Tax deductions	Repairs	Pensions	Benefits deductions	Interest	Depreciation
All exposed	-0.0001	0.0011	0.0013	-0.0004	0.00002**	-0.0001	0.0009***	0.0007
	(0.0009)	(0.0009)	(0.0012)	(0.0003)	(0.00001)	(0.0001)	(0.0003)	(0.0005)

Note: This table presents estimated effects of the minimum wage increases on independent businesses in highly exposed industries (main sample). Each column represents different firm costs (reported deduction on the firms' tax returns). Depreciation includes Section 179 expensing, which allows for immediate expensing of up to $500,000 in new investment from 2010-2017, and up to $1 million in 2018 and 2019, for small and medium sized firms, defined as those with less than $2 million (2010-2017) or $2.5 million (2018 and 2019) in new purchases in the year. Therefore, for the majority of firms in this sample, new investment could be expensed immediately as a Section 179 deduction. Each estimate is from a firm-level regression of Equation (1); the coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s + 4$, relative to the base year, $s - 1$. Outcomes are scaled by baseline revenue and are weighted by log baseline revenue. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. Standard errors are clustered at the state-by-cohort level.

Table E.7: Detailed Firm Outcomes - by Industry and for an Unbalanced Panel

	Outcomes scaled by baseline revenues, Δ *s-1* to *s+4*			
	All exposed	Restaurants	Other/retail	Unbalanced (all)
Revenue	0.0337**	0.0295**	0.0363*	0.0394**
	(0.0162)	(0.0136)	(0.01197)	(0.0186)
Wage bill	0.0146***	0.0206***	0.0078***	0.0130***
	(0.0034)	(0.0044)	(0.0024)	(0.0029)
COGS	0.0132***	0.0053	0.0209***	0.0136**
	(0.0040)	(0.0037)	(0.0049)	(0.0054)
Other deductions	0.0036	0.0027	0.0041	0.0042
	(0.0039)	(0.0033)	(0.0048)	(0.0033)
Variable costs	0.0305***	0.0281***	0.0310***	0.0300***
	(0.0109)	(0.0107)	(0.0116)	(0.0099)
Expensed investment	0.0007	0.0003	0.0010	0.0007
	(0.0005)	(0.0004)	(0.0005)	(0.0005)
Value added	0.0226*	0.0248**	0.0187	0.0268**
	(0.0122)	(0.0102)	(0.0150)	(0.0125)
Owner Profits	0.0001	-0.0001	0.0005	0.00435
	(0.0030)	(0.0019)	(0.0049)	(0.0049)
Employment	-1.474**	-2.687***	-0.509*	-0.405
	(0.598)	(1.054)	(0.273)	(0.338)
Own wage elasticity	-0.245*	-0.335**	-0.073	-0.261*
	(0.130)	(0.125)	(0.172)	(0.148)

Note: This table presents estimated effects of the minimum wage increases on independent businesses across subsamples of the highly exposed industries, and for an alternate sample. The first column shows outcomes for all highly exposed industries, Col. (2) for restaurants only, and Col. (3) for other, non-restaurant, highly exposed industries (detailed further in Appendix Tables G.14). The fourth column presents results for an unbalanced sample, consisting of firms active in the pre-reform year *s-1*, that may exit after that year (and may have entered after year *s-4*); in years where the firm is inactive, outcomes are coded as zeros. In the first 8 rows, the outcome is the variable defined in each row scaled by baseline $(s-1)$ revenue. The own-wage elasticity (OWE) is calculated as the estimated change in log employment over the estimated change in log average wages. Each estimate is from a firm-level regression of Equation (1); the coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. Regressions where outcomes are scaled by baseline revenue are weighted by log baseline revenue; when estimating log changes, regressions are scaled by the baseline value of the outcome. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. Standard errors are clustered at the state-by-cohort level.

F. Heterogeneity of Firm Results

The firm impacts reported in the main text describe how minimum wage increases affect the average firm in a highly exposed industry. This appendix explores the heterogeneity of firm responses by baseline characteristics and treatment. Appendix Tables F.8 through F.10 report estimates by baseline revenue, low-earning labor share of variable costs, and baseline value-added per worker. Appendix Table F.11 examines heterogeneity by treatment year cohort and when excluding California, while Appendix Table F.12 compares the impacts of small and large minimum wage increases.

Table F.8: Heterogeneity in Firm Outcomes by Firm Size

	Baseline Revenue Quartiles			
	Q1	Q2	Q3	Q4
Revenues	0.0325	0.0315*	0.0218	0.0296**
	(0.0233)	(0.0187)	(0.0158)	(0.0144)
Wage bill	0.00964**	0.0184***	0.0151***	0.0124***
	(0.00388)	(0.00353)	(0.00319)	(0.00392)
Profits	0.0005	0.0020	-0.0012	0.0001
	(0.00664)	(0.00320)	(0.00291)	(0.00228)
Employment (levels)	-0.299*	-0.329	-1.644***	-3.958**
	(0.174)	(0.205)	(0.548)	(1.803)
Own Wage Elasticity	-0.121	-0.164	-0.344**	-0.336**
	(0.175)	(0.169)	(0.124)	(0.149)
Firm exit	0.0113	0.00983	-0.0028	-0.0007
	(0.00809)	(0.0179)	(0.0124)	(0.0067)

Note: This table presents estimated effects of the minimum wage increases on independent businesses by baseline revenue quartiles. Firms are categorized by their position in the pre-reform (year $s-1$) revenue distribution; the average revenues by quartile are Q1=$198,240, Q2=$543,746, Q3=$1,156,525, Q4=$8,741,782. In the first three rows the outcome is the variable listed in each row scaled by baseline ($s-1$) revenue. In the next two rows the outcomes are scaled by the firm's baseline value of the listed variable. The employment outcome is the number of distinct employment relationships at the firm. The own-wage elasticity (OWE) is calculated by dividing the estimated percent change in employment by the percent change in average wages, with standard errors calculated using the Delta method. "Firm exit" is a binary outcome equal to one if a firm filed a tax return in year $s-1$ but did not in year $s+4$; as such negative coefficients correspond with increased exit. Each estimate is from a firm-level regression of Equation (1) with a linear probability model used to estimate exit effects; the reported coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. Regressions where outcomes are scaled by baseline revenue are weighted by log baseline revenue; when estimating percent changes for the OWE, regressions are scaled by the baseline value of the outcome. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. OLS outcomes are winsorized from above and below at the 1% level. Standard errors are clustered at the state-by-cohort level.

Table F.9: Heterogeneity in Firm Outcomes - Low-Earning Labor Share of Variable Costs

	Quartiles: Low wage wage bill / variable costs			
	Q1	Q2	Q3	Q4
Outcome as share of baseline revenue				
Revenues	0.0391*	0.0249	0.0318**	0.0361*
	(0.0204)	(0.0153)	(0.0154)	(0.0193)
Wage bill	0.0051*	0.0116***	0.0199***	0.0216***
	(0.0030)	(0.00392)	(0.0044)	(0.0043)
Profits	0.0007	-0.0002	0.0002	-0.0006
	(0.0037)	(0.0023)	(0.0032)	(0.0048)
Percent changes				
Wage bill	0.0330	0.0403***	0.0916***	0.0891***
	(0.0205)	(0.0140)	(0.0190)	(0.0203)
Employment	-0.0201**	-0.0111	-0.0186	-0.0408**
	(0.0085)	(0.0097)	(0.0143)	(0.0156)
OWE	-0.269*	-0.145	-0.195	-0.358**
	(0.156)	(0.133)	(0.154)	(0.148)
Other				
Employment	-1.193**	-1.242**	-0.532	-2.555***
(levels)	(0.590)	(0.602)	(0.957)	(0.858)
Firm exit	0.00987**	-0.00241	0.00540	0.00555
	(0.00485)	(0.00656)	(0.0109)	(0.0130)

Note: This table presents estimated effects of the minimum wage increases on independent businesses by baseline reliance on low-earning labor. The low-earning labor share of variable costs is the ratio of the firm's low-earning wage bill to its total variable costs. The low-earning wage bill is the total wages paid by each firm to employees that earn $< \$20,000$ across all jobs in the base year *s-1* and $< \$25,000$ (including not working) in year *s-2*. Variable costs are defined as $COGS + wagebill$. In the first three rows the outcome is the variable listed in each row scaled by baseline $(s-1)$ revenue. In the next two rows the outcomes are scaled by the firm's baseline value of the listed variable. The own-wage elasticity (OWE) is calculated by dividing the estimated percent change in employment by the percent change in average wages, with standard errors calculated using the Delta method. The employment outcome is the number of distinct employment relationships at the firm. "Firm exit" is a binary outcome equal to one if a firm filed a tax return in year $s-1$ but did not in year $s+4$; as such negative coefficients correspond with increased exit. Each estimate is from a firm-level regression of Equation (1) with a linear probability model used to estimate exit effects; the reported coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. Regressions where outcomes are scaled by baseline revenue are weighted by log baseline revenue; when estimating percent changes regressions are scaled by the baseline value of the outcome. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. OLS outcomes are winsorized from above and below at the 1% level. Standard errors are clustered at the state-by-cohort level.

Table F.10: Heterogeneity in Firm Outcomes by Baseline Productivity

	Baseline Value-added per worker quartiles			
	Q1	Q2	Q3	Q4
Revenues	0.0258	0.0203	0.0277*	0.0456**
	(0.0162)	(0.0124)	(0.0143)	(0.0223)
Wage bill	0.0211***	0.0189***	0.0091***	0.0124***
	(0.0041)	(0.0045)	(0.0032)	(0.0030)
Profits	0.0004	-0.0010	-0.0026	0.0025
	(0.0023)	(0.0017)	(0.0037)	(0.0056)
Employment	-2.347**	-3.300***	-1.149**	-0.315
	(1.172)	(1.170)	(0.499)	(0.290)
Firm exit	0.0018	-0.0161	0.0082	0.0057
	(0.0132)	(0.0114)	(0.0095)	(0.00736)
Own wage elasticity	-0.330**	-0.417**	-0.181	0.092
	(0.158)	(0.160)	(0.146)	(0.262)

Note: This table presents estimated effects of the minimum wage increases on independent businesses by baseline quartile of value-added per worker. Value-added per worker is revenue less COGS scaled by the number of employees. In the first three rows the outcome is the variable listed in each row scaled by baseline ($s-1$) revenue. In the next two rows the outcomes are scaled by the firm's baseline value of the listed variable. The employment outcome is the number of distinct employment relationships at the firm. The own-wage elasticity (OWE) is calculated by dividing the estimated percent change in employment by the percent change in average wages, with standard errors calculated using the Delta method. "Firm exit" is a binary outcome equal to one if a firm filed a tax return in year $s-1$ but did not in year $s+4$; as such negative coefficients correspond with increased exit. Each estimate is from a firm-level regression of Equation (1) with a linear probability model used to estimate exit effects; the reported coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. Regressions where outcomes are scaled by baseline revenue are weighted by log baseline revenue; when estimating percent changes for the OWE, regressions are scaled by the baseline value of the outcome. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. OLS outcomes are winsorized from above and below at the 1% level. Standard errors are clustered at the state-by-cohort level.

Table F.11: Robustness Estimated Effects of Minimum Wage by Event Years and Subsets of Treatment States

	Event year (of Min Wage change)				All event years,
	2013	2014	2015	2016†	excluding CA
Outcome as share of baseline revenue					
Revenues	0.0152	0.0491	0.0197	0.0630**	0.137*
	(0.0142)	(0.0288)	(0.0147)	(0.0206)	(0.0077)
Wage bill	0.0158***	0.0166***	0.0114***	0.0256***	0.0107***
	(0.0023)	(0.0057)	(0.0041)	(0.0046)	(0.0028)
Intermediate Costs	0.0086	0.0209	0.097	0.0292***	0.0072
	(0.0085)	(0.0141)	(0.0088)	(0.0058)	(0.0045)
Profits	-0.0082*	0.0062	-0.0016	0.0058	-0.0028
	(0.0045)	(0.0039)	(0.0030)	(0.0053)	(0.0021)
Employment					
Levels	-4.156***	-1.065	-0.894	-1.045	-1.394*
	(0.584)	(1.047)	(0.740)	(1.820)	(0.757)
Logs	-0.0391**	-0.0109	-0.0277	-0.0007	-0.0171
	(0.0132)	(0.0201)	(0.0182)	(0.0115)	(0.0115)
Own wage elasticity	-0.387**	-0.145	-0.299	-0.005	-0.242
	(0.133)	(0.273)	(0.201)	(0.074)	(0.169)

Note: This table presents estimated effects of the minimum wage increases on independent businesses for subsets of the treatment group. The first four columns correspond to the estimated effects on firms in states that initially increased their minimum wage in the given year, as depicted in Figure 1 and detailed in Appendix Table A.1. The last column shows the estimated effects when excluding firms in California, the largest state in the sample ($\approx$ 24% of firms in the treatment group). Employment effects are estimated in levels, natural logs and as own-wage elasticities (OWEs), where the OWE is estimated by dividing the estimated change in log employment by the estimated change in log average earnings, with standard errors calculated using the Delta method. Each estimate is from a firm-level regression of Equation (1); the coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. Regressions where outcomes are scaled by baseline revenue are weighted by log baseline revenue; when outcomes are in logs, regressions are scaled by the baseline value of the outcome. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. All outcomes are winsorized from above and below at the 1% level. Standard errors are clustered at the state-by-cohort level.

†The coefficients for the 2016 events are estimated for year *t+3* as year *t+4* would be 2020 which is outside of our sample period. The only treatment state for the 2016 event year is Maine.

Table F.12: Estimated effects, Large v. Small Minimum Wage Increases

	Small	Big
Outcome as share of baseline revenue		
Revenues	0.0055	0.0454**
	(0.0114)	(0.0228)
Wage bill	0.0056	0.0189***
	(0.0047)	(0.0031)
Intermediate Costs	0.0038	0.0211**
	(0.0047)	(0.0057)
Profits	0.0002	0.0011
	(0.0022)	(0.0047)
Employment		
Levels	-0.445	-2.263***
	(1.367)	(0.750)
Logs	-0.0143	-0.0277***
	(0.0289)	(0.0096)
Own wage elasticity	-0.348	-0.264**
	(0.709)	(0.094)
Percent changes		
Wage bill	0.0308**	0.109***
	(0.0108)	(0.0149)
Intermediate Costs	0.0061	0.0295*
	(0.0108)	(0.0167)
Wages	0.0410***	0.105***
	(0.0092)	(0.093)

Note: This table examines the impact of minimum wage increases on independent businesses by size of the minimum wage increase. Small minimum wage increases raise the minimum wage by less than 30% over four years, whereas large increases raise it by 30% or more over four years. Employment effects are estimated in levels, natural logs and as own-wage elasticities (OWEs), where the OWE is estimated by dividing the estimated change in log employment by the estimated change in log average earnings, with standard errors calculated using the Delta method. Each estimate is from a firm-level regression of Equation (1); the coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. Regressions where outcomes are scaled by baseline revenue are weighted by log baseline revenue; when outcomes are in logs, regressions are scaled by the baseline value of the outcome. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. All outcomes are winsorized from above and below at the 1% level. Standard errors are clustered at the state-by-cohort level.

G. Firm Analysis by Industry

The firm impacts presented in the main results largely average over all independent businesses, though Table 2 reports incidence analysis separately for restaurants and other highly exposed firms. This appendix presents more results broken down by industry. Appendix Table G.13 reports summary statistics for restaurants and other highly exposed independent firms while Appendix Figures G.2 and G.3 trace the impacts of the minimum wage increases on revenues, wage bills and profits for restaurants and other highly exposed independent firms.

Appendix Table G.14 further breaks down the "other highly exposed independent firms" category, reporting outcomes by four-digit NAICS industry. The corresponding summary statistics by four-digit NAICS industry are reported in Appendix Table G.15.

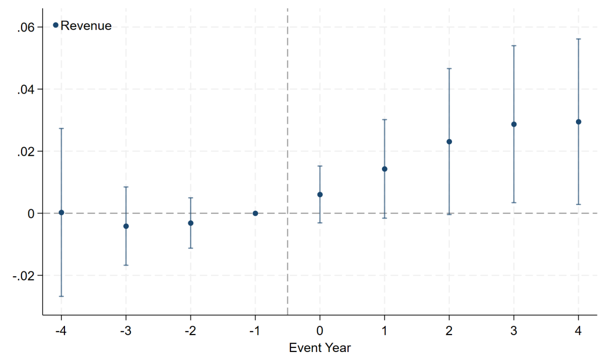
Table G.13: Summary Statistics - Industry Comparison

	Means (base year)	
	Restaurants	Other/retail
Revenue ($)	1,664,379	1,980,059
Wage Bill ($)	421,200	257,740
Owner Income ($)	126,350	122,576
Employees	51	36
Cost structure:		
Share low earning workers	0.276	0.261
Wage bill / revenues	0.26	0.12
COGS / revenues	0.41	0.55
Variable costs / revenues	0.83	0.81
Low earning labor / variable costs	0.042	0.018

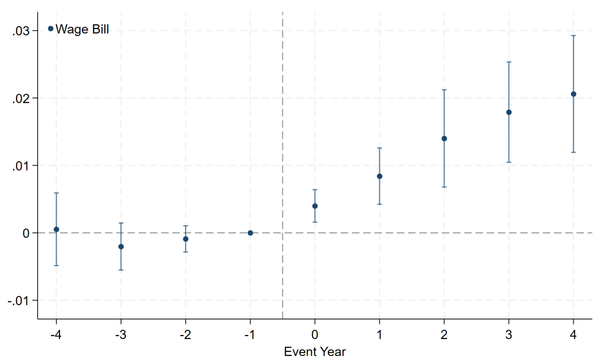
Note: This table shows pre-reform base year averages for independent businesses in highly exposed industries in treatment states, separately for restaurants and other, non-restaurant, highly exposed industries. Share of low earning workers is the share of employees at baseline that earn $< \$20,000$ across all jobs in the base year *s-1* and $< \$25,000$ (including not working) in year *s-2*. Variable costs are defined as COGS+wage bill, and "low earning labor" is the wages paid to low-earning workers.

Figure G.2: Firm Outcomes Scaled by Baseline Revenues - Restaurants

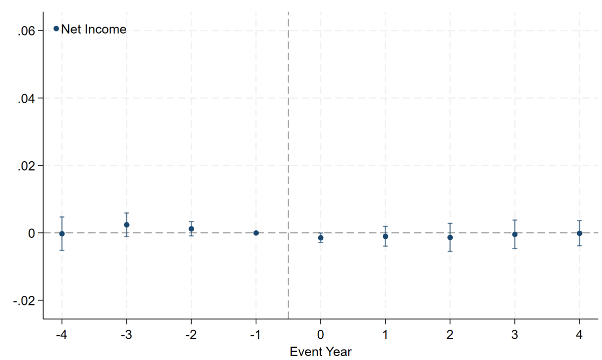
(a) Revenue



(b) Wage Bill



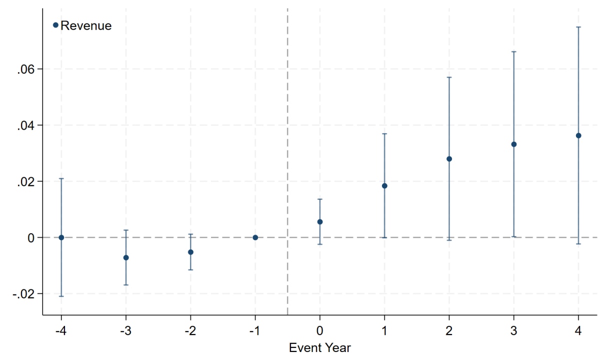
(c) Profits



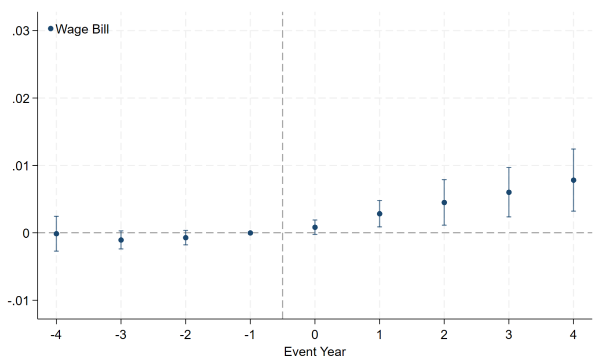
Note: This figure plots the estimated effects of the minimum wage increases on independent restaurants, an industry highly exposed to the minimum wage. Plotted coefficients are estimates of β_s from (1) where the outcomes are firm revenues, wage bills and profits all scaled by revenue in base year $s-1$. The coefficients trace the difference over time in scaled revenue relative to the base year $s-1$ between firms in treated and untreated states. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and outcomes are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure G.3: Firm Outcomes Scaled by Baseline Revenues - Other Exposed Industries

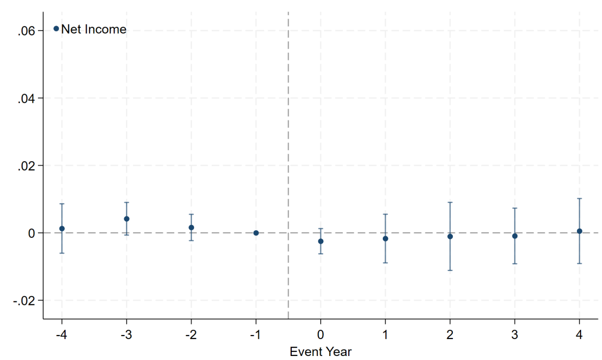
(a) Revenue



(b) Wage Bill



(c) Profits



Note: This figure plots the estimated effects of the minimum wage increases on independent business in highly exposed industries, other than restaurants. Plotted coefficients are estimates of β_s from (1) where the outcomes are firm revenues, wage bills and profits all scaled by revenue in base year $s-1$. The coefficients trace the difference over time in scaled revenue relative to the base year $s-1$ between firms in treated and untreated states. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and outcomes are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Table G.14: Estimated Effects Across Detailed Industry Groups

	All exposed	Restaurants	Retail	Arts, entertainment and recreation	Accommodations
Outcomes scaled by baseline revenues					
Revenue	0.0337**	0.0295**	0.0475***	0.0093	0.0374
	(0.0162)	(0.0136)	(0.0159)	(0.0187)	(0.0254)
Wage bill	0.0146***	0.0206***	0.0057***	0.0041	0.0143***
	(0.0034)	(0.0044)	(0.0012)	(0.0031)	(0.0039)
COGS	0.0132***	0.0053	0.0403***	-0.0024	0.0097***
	(0.0040)	(0.0037)	(0.0108)	(0.0034)	(0.0029)
Owner Profits	0.0001	-0.0001	0.0014	0.0002	0.0005
	(0.0030)	(0.0019)	(0.0026)	(0.0040)	(0.0084)
Percent changes					
Wage bill	0.0603***	0.0663***	0.0587***	0.0056	0.0558***
	(0.0154)	(0.0158)	(0.0141)	(0.0207)	(0.0195)
COGS	0.0351***	0.0067	0.0639***	-0.0035	0.0314
	(0.0106)	(0.0094)	(0.0130)	(0.0269)	(0.0215)
Levels					
Employment	-1.474**	-2.687***	-0.316	-1.278**	-0.478
	(0.598)	(1.054)	(0.411)	(0.501)	(0.632)

Note: This table presents estimated effects of the minimum wage increases on independent businesses by major industry group within the highly exposed industries. Each column is for a 2-digit NAICS industry. In the first four rows the outcome is the variable listed in each row scaled by baseline ($s-1$) revenue. In the next two rows the outcomes are scaled by the firm's baseline value of the listed variable. The employment outcome is the number of distinct employment relationships at the firm. Each estimate is from a firm-level regression of Equation (1); the reported coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. Regressions where outcomes are scaled by baseline revenue are weighted by log baseline revenue; when estimating percent changes regressions are scaled by the baseline value of the outcome. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. Standard errors are clustered at the state-by-cohort level. All raw reported firm values are winsorized from above and below at the 1% level. Regressions are weighted by log baseline revenue.

Table G.15: Detailed Industry Summary Statistics

	Means (base year)			
	Restaurants	Retail	Arts, entertainment and recreation	Accommodations
Revenue ($)	1,664,379	2,650,410	1,393,029	1,612,130
Wage Bill ($)	421,200	247,184	271,843	261,116
Owner Income ($)	126,350	120,532	108,068	134,373
Employees	51	41	42	27
Cost structure:				
Share low earning workers	0.276	0.266	0.292	0.222
Wage bill / revenues	0.26	0.09	0.20	0.16
COGS / revenues	0.41	0.67	0.27	0.18
Variable costs / revenues	0.83	0.86	0.71	0.63
Low earning labor / variable costs	0.042	0.011	0.028	0.022

Note: This table shows pre-reform base year averages for independent businesses in treatment states, across the various major (2-digit NAICS) highly exposed industries included in the main firm analysis sample. Share of low earning workers is the share of employees at baseline that earn $< \$20,000$ across all jobs in the base year *s-1* and $< \$25,000$ (including not working) in year *s-2*. Variable costs are defined as COGS + wage bill, and "low earning labor" is the wages paid to low-earning workers.

H. Robustness and Alternative Specifications: Firm-Level Results

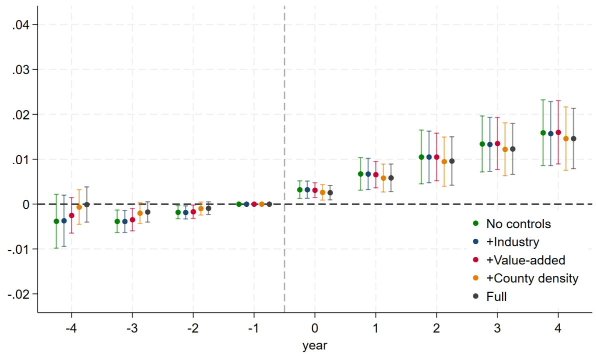
The sensitivity of the main firm analysis results to alternative controls and measures is assessed below.

H.1. Controls

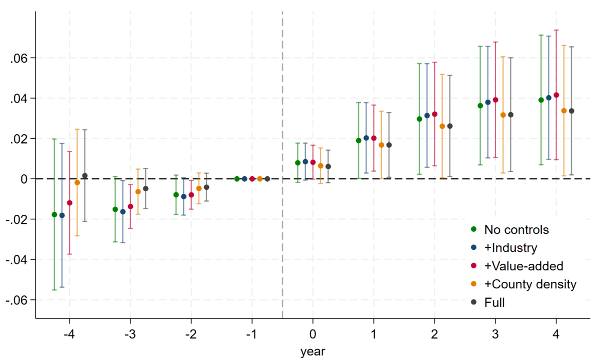
Appendix Figure H.4 presents pre-trend and impact estimates from regression models that do not include controls, control only for value-added deciles, then add county density controls as well, and finally include all controls.

Figure H.4: Robustness to Controls - Firm Outcomes Scaled by Baseline Revenues

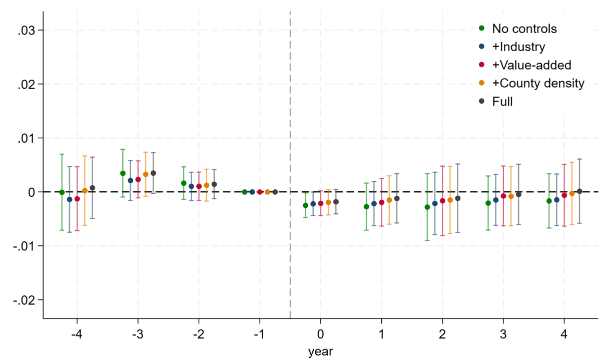
(a) Wage Bill



(b) Revenue



(c) Profits



Note: This figure plots the estimated effects the minimum wage increases on firm outcomes, using various sets of controls. Plotted coefficients are estimates of β_s from (1) where the outcomes are firm wage bills, revenues and profits, all scaled by revenue in base year $s-1$. The different series show estimates layering on control variables. The first estimates "no controls" include only firm-cohort and cohort-by-year fixed effects, "+ industry" adds indicators for two-digit industry interacted with year, "+ Value-added" adds deciles of baseline firm value-added interacted with year, "+ county density" adds quintiles of county density interacted with year, and "Full" includes all of the controls in the main specification (i.e. adds controls for categories of baseline firm size (# of workers) and quintiles of county employment rates each interacted with year). Regressions are weighted by log base year revenue and scaled revenue ratios are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

H.2. Percent Changes

Appendix Table H.16 presents estimates of the impact of state minimum wage increases on key firm outcomes measured as a percentage change relative to baseline; for example, among all firms in highly exposed industries, revenues increase by 3.59% following the analyzed state minimum wage increases. Appendix Figures H.5 and H.6 show the evolution of average firm wage bills and COGS as percentage changes relative to the base year. Estimates are provided for all pass-throughs in highly exposed industries as well as for restaurants and other exposed firms separately. All estimates are from the balanced panel.

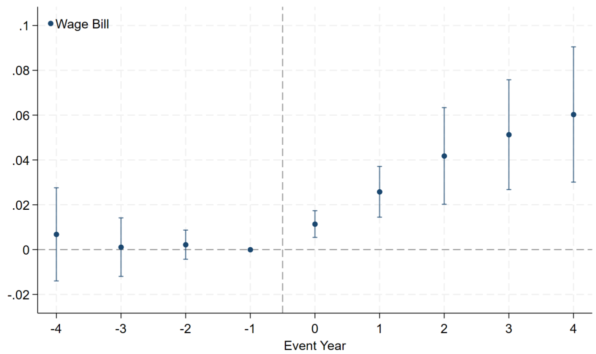
Table H.16: Firm Outcomes - Percent Changes

	Percent Changes *s-1* to *s+4*		
	All exposed	Restaurants	Other exposed/retail
Revenues	0.0359**	0.0266*	0.0408**
	(0.0156)	(0.0142)	(0.0163)
Wage bill	0.0603***	0.0663***	0.0481***
	(0.0154)	(0.0158)	(0.0138)
COGS	0.0351***	0.0067	0.0544***
	(0.0106)	(0.0094)	(0.0122)
Other deductions	0.0204	0.0123	0.0272
	(0.0208)	(0.0201)	(0.0215)
Variable costs	0.0413***	0.0311**	0.0467***
	(0.0149)	(0.0139)	(0.0147)
Employment	-0.0205*	-0.0349***	-0.0047
	(0.0104)	(0.0120)	(0.0111)
Avg. wages	0.0835***	0.104***	0.0640***
	(0.0132)	(0.0154)	(0.0131)
Own wage elasticity	-0.245*	-0.335**	-0.073
	(0.130)	(0.125)	(0.172)

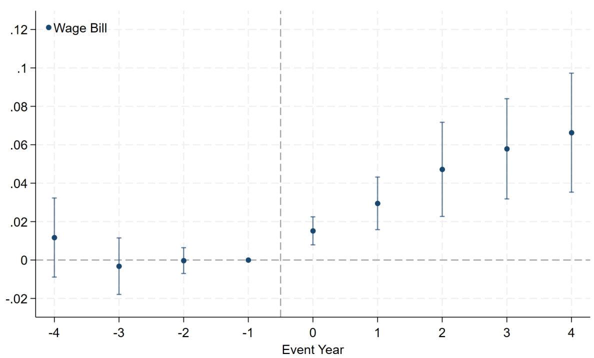
Note: This table presents estimated effects of the minimum wage increases on independent businesses in all highly exposed industries (main sample), then separately for restaurants and other, non-restaurant, highly exposed industries. Each estimate is from a firm-level regression of Equation (1); the coefficients are estimated effects on the outcomes four years after the minimum wage increase, $s+4$, relative to the base year, $s-1$. To estimate percent changes, the outcomes in year t are scaled by the baseline value of the outcome. Regressions are weighted by the baseline value of the outcome. The own-wage elasticity (OWE) is calculated by dividing the estimated percent change in employment by the percent change in average wages, with standard errors calculated using the Delta method. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with with year to allow for differential time trends. Standard errors are clustered at the state-by-cohort level.

Figure H.5: Effect of the Minimum Wage on Percent Changes in Wage Bills

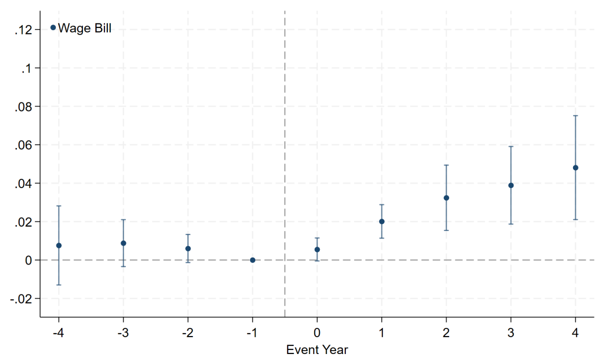
(a) All exposed industries



(b) Restaurants



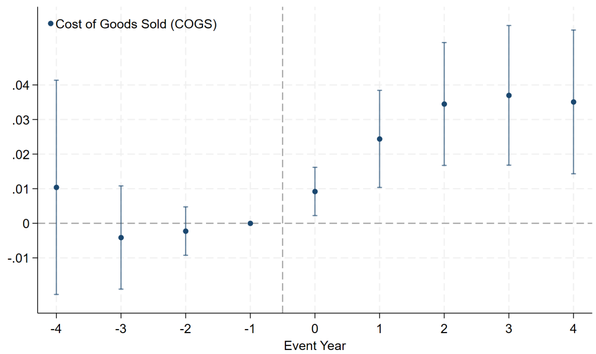
(c) Other highly exposed



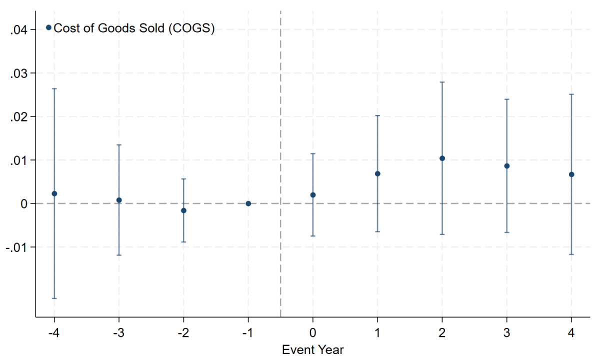
Note: This figure plots the estimated effects of the state minimum wage increases on firm wage bills. Plotted coefficients are estimates of β_s from (1) where the outcome is firm wage bill in year t scaled by base year wage bill. The coefficients trace the differential percent change in wage bills between firms in treated and untreated states, relative to the base year $s-1$. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by base year wage bill and outcomes are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level. Panel (a) presents the estimated effects for independent businesses in all highly exposed industries; Panel (b) for restaurants specifically; and Panel (c) for the other, non-restauant, highly exposed industries.

Figure H.6: Effect of the Minimum Wage on Percent Changes in Cost of Goods Sold

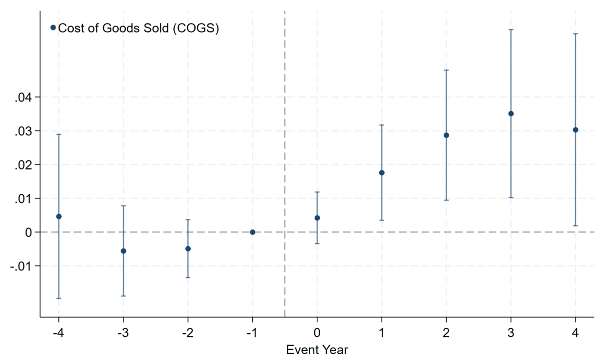
(a) All exposed industries



(b) Restaurants



(c) other highly exposed



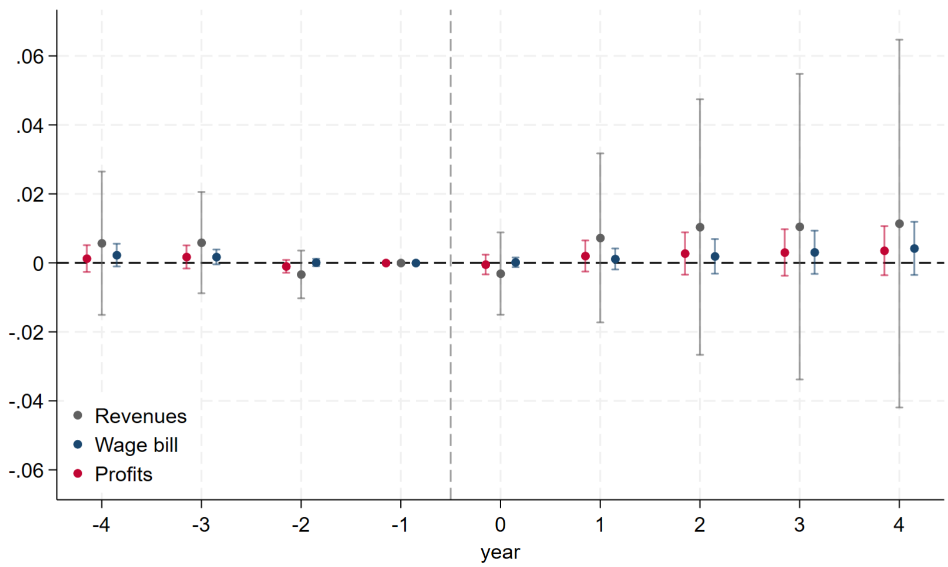
Note: This figure plots the estimated effects of the state minimum wage increases on firm COGS. Plotted coefficients are estimates of β_s from (1) where the outcome is firm COGS in year t scaled by base year COGS. The coefficients trace the differential percent change in COGS between firms in treated and untreated states, relative to the base year $s-1$. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by base year COGS and outcomes are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level. Panel (a) presents the estimated effects for independent businesses in all highly exposed industries; Panel (b) for restaurants specifically; and Panel (c) for the other, non-restaruant, highly exposed industries.

H.3. Placebo Tests

We may worry that the estimated impacts reported in Section 4 reflect not only the impact of the minimum wage but concomitant changes in the economic trajectories of treatment states relative to control states. Though the similarity of pre-trends between firms in states that raised their minimum wages and states that left their wage floors unchanged bolsters our confidence in the comparability of firms in treatment and control states, we also conduct placebo tests to better understand the threat of concurrent macro events.

Appendix Figure H.7 presents regression estimates of Equation (1) but for only firms in industries with low shares of minimum wage workers based on CPS data. Specifically, we include industries employing less than 1% of minimum wage workers. We conduct placebo tests for the impact of the minimum wage in these industries in treatment states (relative to control states) and find no statistically discernible impacts or point estimates of economically meaningful magnitudes for revenues, wage bills or profits. This concert of null estimates suggests that the estimated impacts in industries highly exposed to minimum wage policy are not attributable to macro events affecting the overall economies of states that raised their minimum wages.

Figure H.7: Placebo Test - Effect of Minimum Wage on Unexposed Industries



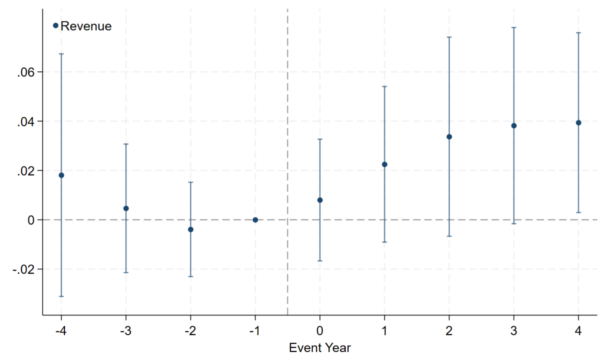
Note: This figure plots the estimated effects of the minimum wage increases on independent businesses operating in industries not highly exposed to the minimum wage. Plotted coefficients are estimates of β_s from Equation (1) where the outcomes are firm revenues, wage bills and profits all scaled by revenue in base year $s-1$. The coefficients trace the difference over time in scaled revenue relative to the base year $s-1$ between firms in treated and untreated states. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and outcomes are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

H.4. Unbalanced Panel of Independent Businesses

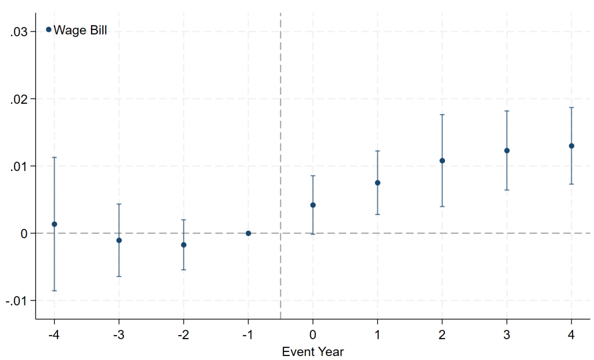
The main firm results presented in Tables 1 and 2 and Figures 2 through 5 use a balanced panel of independent businesses. Appendix Figure H.8 uses an unbalanced panel of firms to estimate the impacts of the minimum wage increases on firm revenues, wage bills and owner profits. The results are statistically indistinguishable from the main results using the balanced panel presented in the text. Estimates of the impact on firm outcomes in $s+4$ are reported in Appendix Table E.7.

Figure H.8: Robustness - Unbalanced Panel, Firm Outcomes Scaled by Baseline Revenues

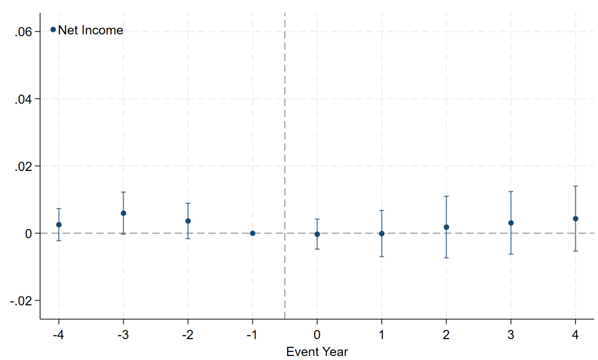
(a) Revenue



(b) Wage Bill



(c) Profits

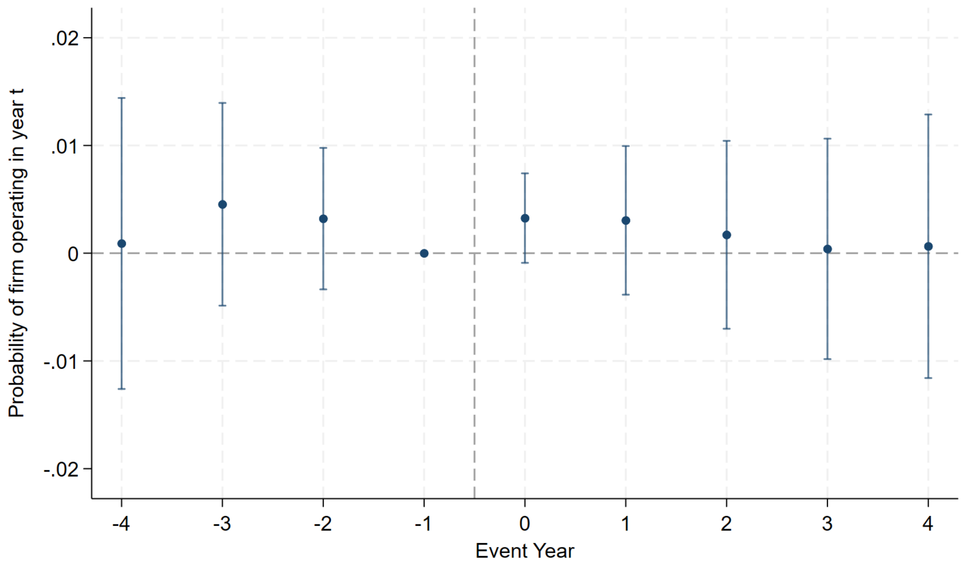


Note: This figure plots the estimated effects of the minimum wage increases on independent business in highly exposed industries, using an unbalanced panel. The unbalanced panel consists of firms that were active in pre-reform year *s-1*, but may have exited before year *s+4* or entered after year *s-4*. Plotted coefficients are estimates of β_s from (1) where the outcomes are firm revenues, wage bills and profits all scaled by revenue in base year $s-1$. The coefficients trace the difference over time in scaled revenue relative to the base year $s-1$ between firms in treated and untreated states. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and outcomes are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

H.5. Alternative Firm Exit Specification

The entry and exit results plotted in Figure 6 are from regressions of count data. The construction of these data are detailed in Appendix L.5. Below, we offer estimates from an alternative specification. The estimates plotted in Appendix Figure H.9 are the coefficients from linear probability model regression of Equation (1) where the outcome is a binary indicator equal to one if a firm that filed a tax return in time $s-1$ does not file a return in time s.

Figure H.9: Effects on Firm Exit - Estimates from Firm-Level Regressions



Note: The figure above plots the estimated effects of the minimum wage increases on the exit rates of independent businesses operating in highly exposed industries. Plotted coefficients are estimates of β_s from linear probability model regressions of Equation (1) where the dependent variable is a binary indicator equal to one when a firm that filed a tax return in year $s-1$ does not file a return in year s.In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Regressions are weighted by log base year revenue and outcomes are winsorized at the 99th percentile. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

H.6. Excluding Firms with High Shares of Workers in Multiple States

Some independent businesses in our linked panel employ workers in more than one state. These may be enterprises that operate near a state border or they may be multi-establishment enterprises that operate in multiple states. In the latter case, we may worry that some workers may be employed in non-treated states and thus not subject to the wage floor increases, leading to downward biased estimates of the minimum wage's impact.

Appendix Table H.17 reports estimates of the four-year impact of the minimum wage increases on independent businesses excluding firms that report high shares of workers in multiple states. The upper row drops the 7% of firms that have more than 10% of their workers in control states, while the second row examines the 78% of firms with more than 70% of their workers in the same state. All estimates closely align with the main results presented in the paper.

Table H.17: Robustness - Excluding firms with higher shares of workers in multiple states

	Outcomes scaled by baseline revenues					
	Revenues	Wage bill	COGS	Profits	Employees	OWE
Excluding firms w/ >10%	0.0347***	0.0152***	0.0129***	0.0002	-1.407**	-0.240*
workers in control states	(0.0161)	(0.0034)	(0.0041)	(0.0031)	(0.622)	(0.136)
Excluding firms w/ <70%	0.0356**	0.0149***	0.0154***	0.0001	-0.802	-0.115
workers in reporting state	(0.0160)	(0.0031)	(0.0040)	(0.0033)	(0.651)	(0.194)

Note: This table shows estimates of the effects of the minimum wage on firm outcomes for subsamples of firms, defined by the share of the firms' workers that live in a different state than the reported firm address. The first row excludes the 7% of treatment firms that have more than 10% of their workers in control states. The second row includes only firms with more than 70% of their workers in the same state (78% of firms). All outcomes are estimated differences between firms in treatment and control states four years after the minimum wage increases (year *s+4*), relative to base year *s-1*, estimated using Equation (1). The outcomes for revenues, wage bill, COGS and profits are scaled by base year firm revenues and weighted by log baseline revenue. The employee estimates are in levels. The own-wage elasticity (OWE) is estimated by dividing the the estimated effects on log employment by the estimated effects on log average wages and standard errors are calculated using the Delta method. In addition to firm-cohort and cohort-by-year fixed effects, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. Standard errors are clustered at the state-by-cohort level.

I. Robustness and Alternative Specifications: Individual Results

The tables and figures below examined the sensitivity of the main individual analysis results to alternative controls and measurement choices, and investigate heterogeneity in responses.

I.1. Controls

Appendix Table I.18 and Appendix Table I.19 present four-year estimates of the impact of minimum wages on individual earnings and employment with different sets of controls, and using a "regression adjusted" estimator (see Appendix J for details of the regression adjustment). Estimates are very similar across controls.

Table I.18: Robustness - Individual Panels: Worker Earnings Effects

	Across regression controls				Regression
	(1)	(2)	(3)	(4)	adjusted
All low-earning	2,350***	2,177***	2,163***	1,472***	1,271***
	(407)	(372)	(347)	(304)	(330)
Low-earning exposed industries	2,018***	1,898***	2,005***	1,532***	1,387***
	(321)	(312)	(307)	(287)	(351)
All young	3,117***	2,884***	2,894***	1,995***	1,747***
	(609)	(547)	(539)	(488)	(609)
All teen	2,247***	2,161***	2,121***	1,845***	1,571***
	(345)	(322)	(352)	(1.803)	(393)
Controls:					
Baseline industry		X	X	X	X
Age and age^2			X	X	X
local market characteristics				X	X

Note: This table presents the estimated effects of the minimum wage increases on earnings for baseline low-earning workers and young individuals, using various sets of controls and an alternative estimation strategy. The first four columns show estimates of β_s from Equation (2). Reported coefficients are estimates of the average change in annual earnings for each group between the base year prior to the policy change $s-1$ and four years hence $s+4$. Col. (1) shows results including only controls for individual-cohort and cohort-by-year fixed effects, Col. (2) adds controls for baseline industry, Col (3) adds baseline age and age-squared, Col. (4) adds baseline quintiles of county density and quintiles of county employment rates. All controls are flexibly interacted with year. Standard errors clustered at the state-by-cohort level. The last column shows results using a "regression adjusted" (RA) estimator, described in detail in Appendix J. The RA estimation uses the full set of controls and standard errors estimated using clustered bootstrapping with resampling at the state-cohort level.

Table I.19: Robustness - Individual Panels: Employment Effects

	Across regression controls				Regression
	(1)	(2)	(3)	(4)	adjusted
All low-earning	0.0008	0.0006	0.0003	-0.0023	0.0006
	(0.0081)	(0.0076)	(0.0070)	(0.0068)	(0.0087)
Low-earning exposed industries	0.0025	0.0021	0.0053	0.0039	0.0051
	(0.0069)	(0.0070)	(0.0068)	(0.0066)	(0.0080)
All young	0.0123***	0.0134***	0.0186***	0.0139***	0.0106*
	(0.0030)	(0.0037)	(0.0042)	(0.0038)	(0.0055)
All teen	0.0146***	0.0141***	0.0170***	0.0139***	0.0099**
	(0.0032)	(0.0035)	(0.0035)	(0.0045)	(0.0050)
Controls:					
Baseline industry		X	X	X	X
Age and age^2			X	X	X
local market characteristics				X	X

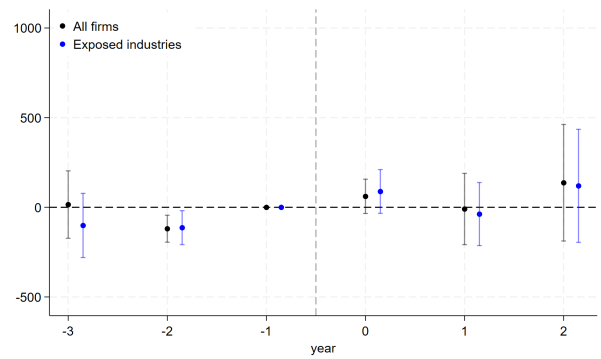
Note: This table presents the estimated effects of the minimum wage increases on employment for baseline low-earning workers and young individuals, using various sets of controls and an alternative estimation strategy. In the first four columns, estimates are from linear probability model regressions of Equation (2) where the dependent variable is an indicator if the individual receives a W-2 in year s. Reported coefficients are estimates of the average change in employment probability for each group between the base year prior to the policy change $s-1$ and four years hence $s+4$. Col. (1) shows results including only controls for individual-cohort and cohort-by-year fixed effects, Col. (2) adds controls for baseline industry, col (3) adds baseline age and age-squared, Col. (4) adds baseline quintiles of county density and quintiles of county employment rates. All controls are flexibly interacted with year. Standard errors clustered at the state-by-cohort level. The last column shows results using a "regression adjusted" (RA) estimator, described in detail in Appendix J. The RA estimation uses the full set of controls and standard errors estimated using clustered bootstrapping with resampling at the state-cohort level.

I.2. Placebo Tests

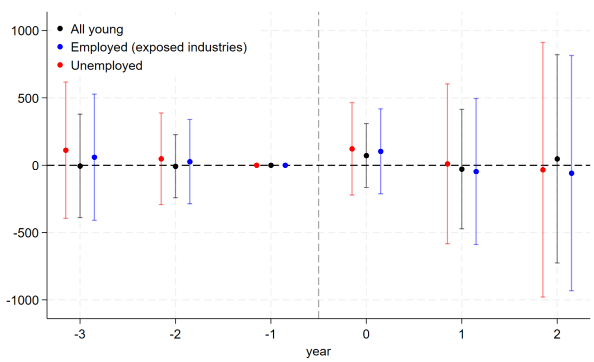
Although visual inspection of pre-trends in each of the individual analyses presented in the main results suggests that individuals in treatment and control states were on similar trajectories prior to the minimum wage increases, we also conduct a formal placebo test to examine the comparability of treatment and control groups. Appendix Figure I.10 and Appendix Figure I.11 show the results of placebo tests where we estimate differences in individual earnings and employment in treatment and control states around a placebo minimum wage increase in 2010, which is three years before any of our true policy changes. All estimates show no impact of the placebo event.

Figure I.10: Placebo Test - Individual Earnings "Effects" for Placebo Treatment Year

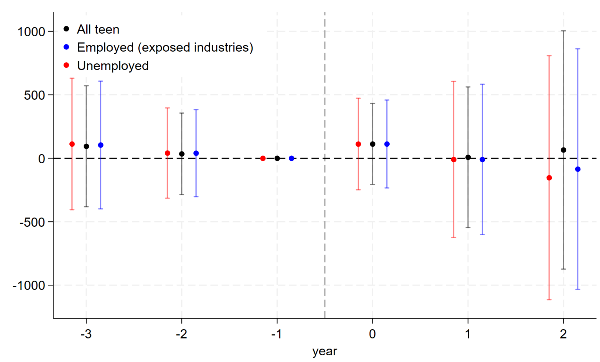
(a) Low Earning Workers



(b) Young Individuals

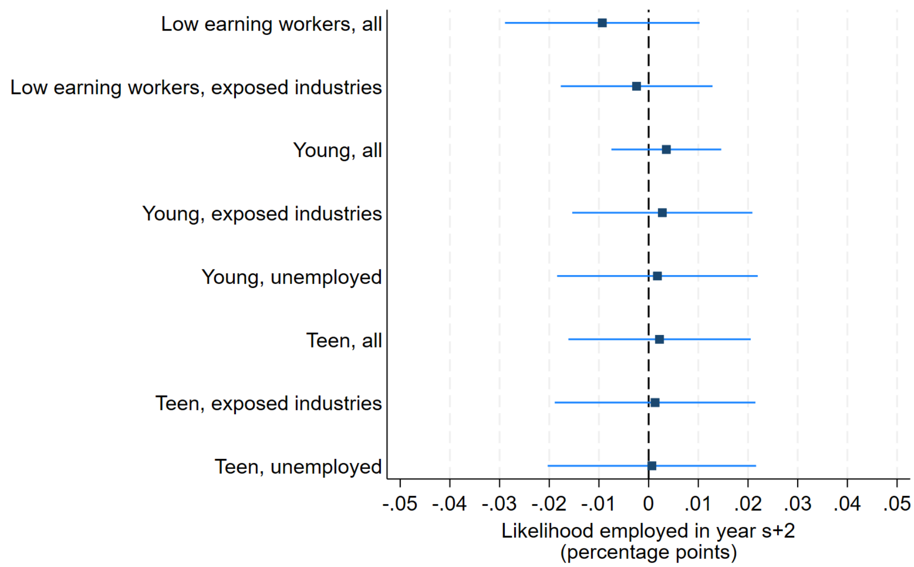


(c) Teen Individuals



Note: This figure shows results of a placebo test where we estimate differences in individual earnings between treatment and control states around a placebo minimum wage increase in year 2010, (i.e. at least three years prior to the first minimum wage increases). This mirrors the main estimates and estimation strategy discussed in Section 5 presented in Figure 7, but the base year in Equation (2) is 2009 for all states, and the low-earning worker and young individual panels are sampled using base year 2009 characteristics. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure I.11: Placebo Test - Individual Employment “Effects” for Placebo Treatment Year



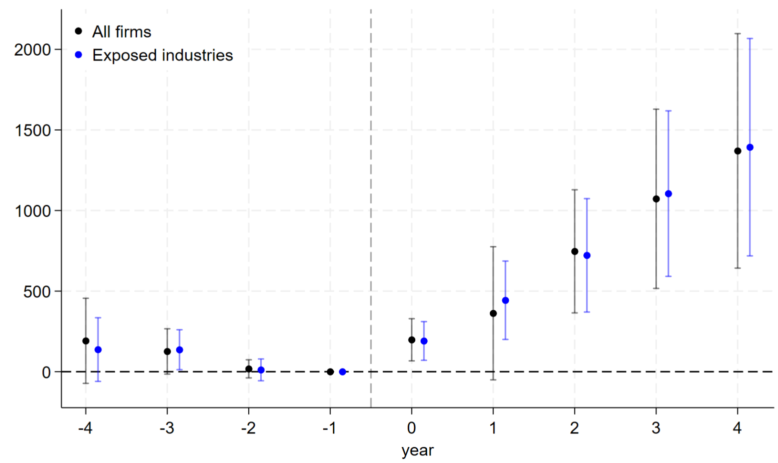
Note: This figure shows results of a placebo test where we estimate differences in individual employment probabilities between treatment and control states around a placebo minimum wage increase in year 2010, (i.e. at least three years prior to the first minimum wage increases). This mirrors the main estimates and estimation strategy discussed in Section 5 presented in Figure 8, but the base year in Equation (2) is 2009 for all states, and the low-earning worker and young individual panels are sampled using base year 2009 characteristics. Plotted coefficients are estimates of the average change in employment probability for each group between the placebo base year, 2009, and three years after the placebo minimum wage increase, 2012. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

I.3. Excluding California

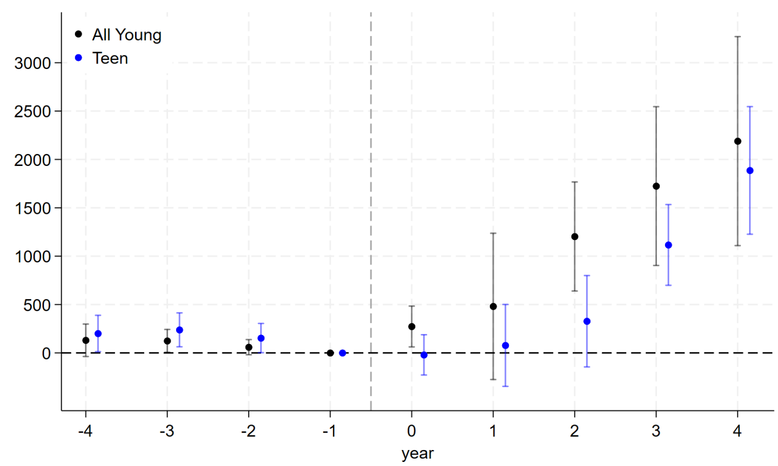
Appendix Figure I.12 and Figure I.13 show the individual earnings and employment impacts of minimum wage changes when excluding California as a treatment state in the analysis.

Figure I.12: Individual Earnings - Robustness, Excluding California

(a) Low Earning Worker Panel

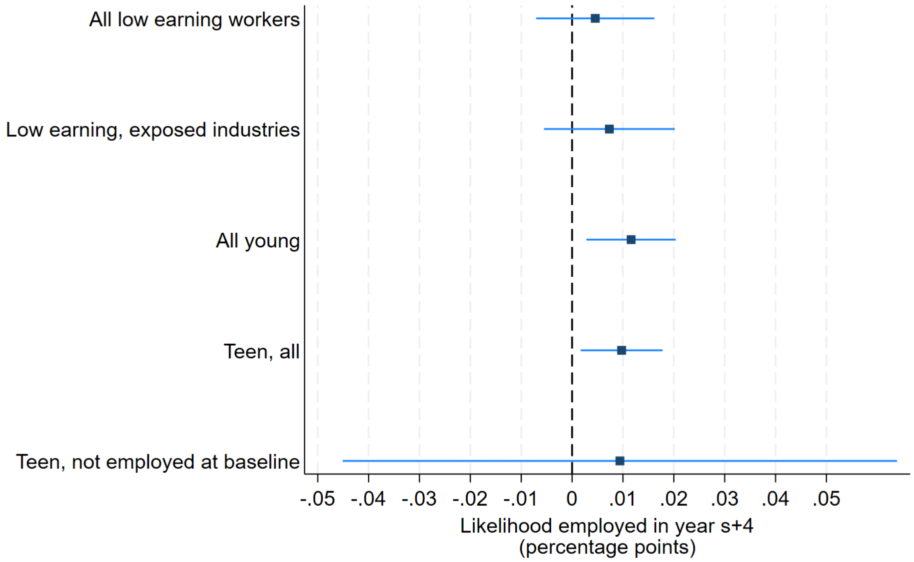


(b) Young Individual Panel



Note: This figure demonstrates the impact of minimum wage increases on the evolution of average earnings for low-earning and young workers when excluding California (CA) as a treatment state in the analysis. This tests robustness to dropping the largest state in the sample. CA accounts for almost 32% of low-earning workers in the main treatment group sample, ≈30% of low-earning workers in highly exposed industries, ≈31% of young individuals and ≈24% of teens. Plotted coefficients are estimates of β_s from Equation (2) where the dependent variable is annual earnings. Panel (a) plots the difference in the earnings trajectories for all low-earning individuals and those employed in highly exposed industries in base year $s-1$. Panel (b) plots the impact on earnings trajectories for young individuals ages 15-26 in base year $s-1$ separately by base year employment status. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure I.13: Individual Employment - Robustness, Excluding California

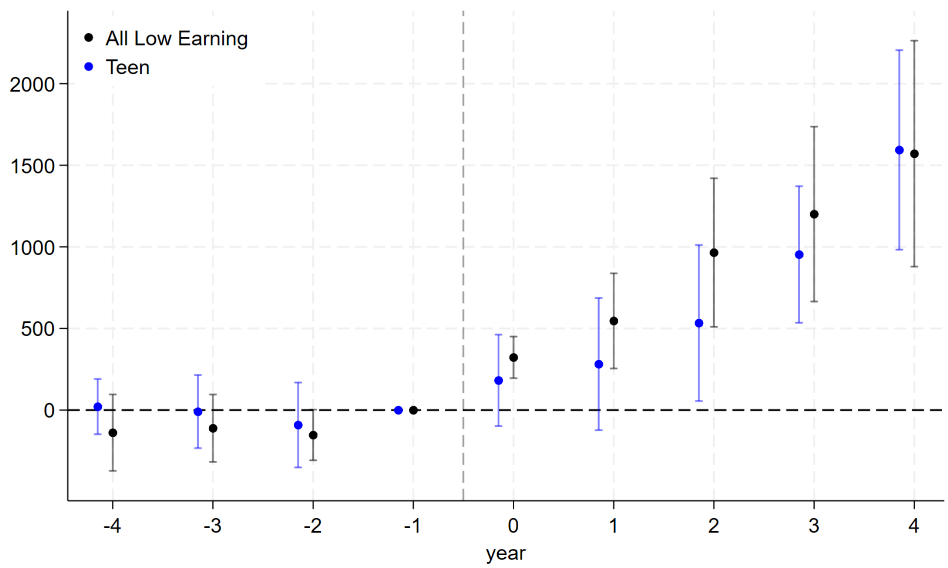


Note: This figure demonstrates the impact of minimum wage increases on employment for low-earning and young workers when excluding California (CA) as a treatment state in the analysis. This tests robustness to dropping the largest state in the sample. CA accounts for almost 32% of low-earning workers in the main treatment group sample, ≈30% of low-earning workers in highly exposed industries, ≈31% of young individuals and ≈24% of teens. Estimates are from linear probability model regressions of Equation (2) where the dependent variable is an indicator if the individual receives a W-2 in year s. Plotted coefficients are estimates of the average change in employment probability for each group between the base year prior to the policy change $s-1$ and four years hence $s+4$. The top two estimates are from the low-earning worker panel and the lower three are from the panel of young individuals. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

I.4. Individuals Employed by Independent Businesses at Baseline

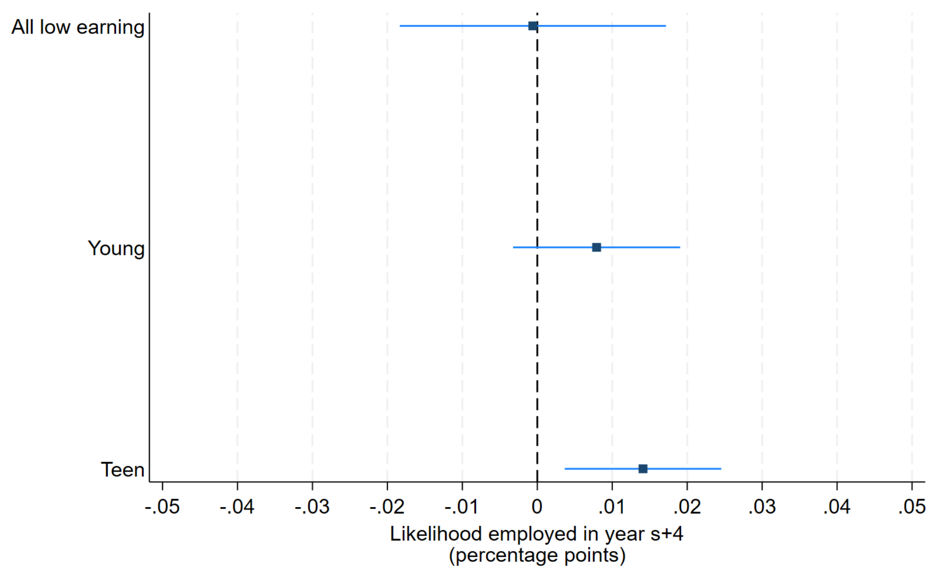
Appendix Figure I.14 and Appendix Figure I.15 examine the earnings trajectories and employment impacts of minimum wage increases for low-earning workers employed by independent businesses operating in highly exposed industries. The figures examine low-earning workers employed by highly exposed independent business overall, as well subgroups by age at baseline.

Figure I.14: Individual Earnings - Workers at Independent Businesses at Baseline



Note: This figure demonstrates the impact of minimum wage increases on the evolution of average earnings for workers at highly exposed independent businesses at baseline. Plotted coefficients are estimates of β_s from Equation (2) where the dependent variable is annual earnings across all jobs. The first series plots the difference in the earnings trajectories for low-earning workers (earning <20,000 but >0 across all jobs in year $s-1$, and <25,000 in year $s-2$) in highly exposed independent businesses at baseline. The second series shows estimates is for teen (15-19yrs old) workers at these firms at baseline. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

Figure I.15: Individual Employment - Workers at Independent Businesses at Baseline



Note: This figure demonstrates the impact of minimum wage increases on employment four years after the minimum wage increases for workers at highly exposed independent businesses at baseline. Estimates are from linear probability model regressions of Equation (2) where the dependent variable is an indicator if the individual receives a W-2 in year s. Plotted coefficients are estimates of the average change in employment probability for each group between the base year prior to the policy change $s-1$ and four years hence $s+4$. The first estimate presents outcomes for low-earning workers (earning $<$20,000 but $>$0 across all jobs in year $s-1$, and $<$25,000 in year $s-2$) in highly exposed independent businesses at baseline. The second estimate is for young (15-26yrs old) workers at these firms at baseline; the third is for teenagers (15-19yrs old) at these firms at baseline. In addition to individual-cohort and cohort-by-year fixed effects, the specifications include baseline age and age squared, quintiles of county density and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For each point estimate the 95% confidence interval is marked with standard errors clustered at the state-by-cohort level.

J. Regression Adjusted Estimators

Our primary regression specifications to estimate treatment effects are linear regressions of the form represented in Equations (1) and (2). Roth et al. (2023) show that even when the identifying parallel trends assumption holds conditional on control variables X_j, linear regressions can lead to inconsistent estimates of the treatment effect in DiD estimators if treatment effects are heterogeneous in X_j. While our use of non-parametric controls mitigates this concern, we also present estimates using "regression adjusted" estimators of the treatment effect which provides consistent estimates under weaker homogeneity assumptions than those required with linear regression (Roth et al. (2023)).

Regression adjustment is a two-step approach to estimating treatment effects. First, separate models are fit regressing the outcome on control variables separately for the treatment and control groups. To estimate the average treatment effect (ATE), the differences in the average predicted values for the treatment and control groups give the ATE. To estimate the average treatment effect on the treated (ATT), the predicted values for the treatment group from both the control and treatment regressions are used, (i.e. the predicted control values are evaluated using the observed covariates of the treatment group).

We estimate the treatment effects in a long differences specification, comparing outcomes of treatment and control groups from base year $s-1$ to post year $s+4$. The first-step equation is:

$$y_{jct} = \alpha + \Gamma_s X_{jc} + I(post_{s=t}) + I(post_{s=t}) \times \Gamma_s X_{jc} + \delta_{ct} + \nu_{jct} \tag{J.2}$$

where $I(post_{s=t})$ is an indicator for event year $s+4$. This model is run separately for the treatment and control groups. The ATE is estimated by taking the mean difference-in-differences in the predicted outcomes for the treatment and control group. Define $\hat{Y}_{T,t}(T)$ as the average predicted outcomes where: subscript $T \in 0, 1$ indexes that values are predicted from the first step regression run on the control group (0) or treatment group (1), $t \in pre, post$ indexes that the event year of the predicted values, and the T in parentheses indicates that the predicted values are evaluated at the observed covariates of group T. Then, the ATE and ATT are estimated as:

$$ATE = (\hat{Y}_{1,post}(1) - \hat{Y}_{1,pre}(1)) - (\hat{Y}_{0,post}(0) - \hat{Y}_{0,pre}(0))$$

$$ATT = (\hat{Y}_{1,post}(1) - \hat{Y}_{1,pre}(1)) - (\hat{Y}_{0,post}(1) - \hat{Y}_{0,pre}(1)).$$

We estimate these treatment effects separately for each outcome of interest. The ATT estimates for the firm results are reported in Table J.20 and the ATT estimates for the individual results are reported in Tables I.18 and I.19. Standard errors are obtained using clustered bootstrapping, where the first and second steps are jointly boostrapped and resampling is done at the state-cohort level.

Table J.20: Robustness - Regression Adjusted Estimators

	Outcomes scaled by baseline revenues, *s-1* to *s+4*				
	Revenue	Wage bill	COGS	Profits	Employment
All exposed	0.0351*	0.0143***	0.0132**	0.0005	-1.44**
	(0.0210)	(0.0036)	(0.0062)	(0.0041)	(0.684)

Note: This table presents estimates of the effect of the effect of minimum wage increases on firm outcomes using a "regression adjusted" (RA) estimator. The details of the two-step RA estimation are discussed in Section J. The estimation uses the same set of controls as in the main specification, cohort-by-year fixed effects, categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all interacted with year. The estimates are for differences between in outcomes for firms in treatment and control states four years after the minimum wages increases (year *s+4*), relative to pre-reform year *s-1*. Revenue, wage bill, COGS and profits are scaled by baseline revenue and weighted by log baseline revenue. Employment is estimated in levels. Standard errors are obtained using clustered bootstrapping, where the first and second steps are jointly boostrapped and resampling is done at the state-cohort level.

K. Additional Individual Transition Analysis

To augment the transition analysis by firm type reported in the main results in Table 5, Appendix Table K.21 reports employment patterns four years after the minimum wage increases by size of the destination firm, and Appendix Table K.22 examines employment patterns over a shorter, two-year, horizon.

Panel A of Appendix Table K.21 reports differences in employment rates across the firm size distribution between low-earning workers in treatment and control states. Employment patterns are reported for all workers and just for those who switch employers (Movers). Low-earning workers are more likely to be employed by the largest employers after the minimum wage increase, with worker transitions away from the smallest firms in highly exposed industries and toward the largest employers outside of highly exposed industries driving the pattern. Panel B reports the results from our repeated cross-section analysis of teenagers (working and not). Teens are also less likely to be employed at the smallest firms in highly exposed industries.

Appendix Table K.22 repeats the analysis of Table 5 and Table K.21 using a shorter two-year horizon rather than the five-year horizon used in the other tables. The transition patterns we discuss over the five-year horizon are largely evident just two years after the minimum wage increases.

Table K.21: Employment Patterns of Low-Earning Workers and Teenagers

Panel A: Low Earning Workers Panel

	Firm size quartile (exposed industries)			
	Q1	Q2	Q3	Q4
All workers				
All industries	-0.0013	-0.0005	0.001	0.0096***
	(0.0008)	(0.0009)	(0.0012)	(0.0027)
Exposed industries	-0.008***	-0.0031	-0.0019	0.0202**
	(0.0029)	(0.0035)	(0.0047)	(0.0096)
Movers				
All industries	-0.0018**	-0.0012	0.0007	0.0077**
	(0.0008)	(0.0010)	(0.0011)	(0.0033)
Exposed industries	-0.0107**	-0.006	-0.0054	0.016
	(0.0038)	(0.0041)	(0.0049)	(0.0122)

Panel B: Teens, repeated cross-section

	Firm size quartile (exposed industries)			
	Q1	Q2	Q3	Q4
All Teens	-0.0022**	-0.0031***	0.0005	-0.0004
	(0.0010)	(0.0010)	(0.0015)	(0.0051)
Employed teens	-0.0029*	-0.0049***	0.0026	-0.0003
	(0.0017)	(0.0018)	(0.0021)	(0.0077)

Note: The table above reports employment patterns for individuals from our low-earning panel and repeated cross-sections of teenagers. Employment patterns are tracked by firm size quartile, with size is defined by total labor payments as detailed in Appendix L.3. Panel A describes the impact of minimum wage increases on employment transitions between years $s-1$ and $s+4$ for a panel of low-earning workers. These workers are all employed at baseline in either exposed or unexposed industries and the estimates report the change in their transition patterns relative to similar workers in untreated states. Each row describes transition patterns conditional on the industry in which low-earning workers were employed in the base year. Movers columns focus on transition patterns of workers no longer employed by their baseline ($s-1$) employer in year $s+4$. Estimates are from a linear probability model specification of Equation (2) where the dependent variable is an indicator for a worker being employed at a firm in a given size quartile in year $s+4$. The lower panel describes employment patterns for randomly sampled repeated cross-sections of teenagers. Appendix L.4 details the construction of these data. The reported estimates convey five-year changes in average employment rates at firms in different size quartiles between years $s-1$ and $s+4$. All standard errors are clustered at the state-cohort level.

Table K.22: Employment Patterns of Low-Earning Workers and Teenagers - Short Differences (2 year)

Panel A: Low Earning Workers Panel

			All workers				Movers			
Baseline industry	retain	exposed industry	exposed industry independent business	exposed industry Large C-corp.	unexposed industry independent business	unexposed industry large C-corp.	exposed industry independent business	exposed industry large C-corp.	unexposed industry independent business	unexposed industry large C-corp.
Exposed industries	0.0232***	0.0113**	-0.0056	0.0143**	-0.0017	-0.0063**	-0.0211**	0.0134*	-0.0011	-0.0072**
	(0.0046)	(0.0048)	(0.0051)	(0.0062)	(0.0031)	(0.0027)	(0.0072)	(0.0076)	(0.0038)	(0.0031)
Unexposed industries	0.0115**	-0.0055	-0.0019	-0.0028**	0.0064*	0.0032	-0.0015	-0.0037**	0.0026	0.0035
	(0.0056)	(0.0034)	(0.0024)	(0.0011)	(0.0037)	(0.0037)	(0.0034)	(0.0014)	(0.0055)	(0.0045)
			All workers Firm size quartile (exposed industries)				Movers Firm size quartile (exposed industries)			
			Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
All industries			-0.0009	-0.0001	0.0005	0.0087***	-0.0018***	-0.0007	-0.0005	0.0069**
			(0.0006)	(0.0006)	(0.0009)	(0.0021)	(0.0006)	(0.0007)	(0.0009)	(0.0033)
Exposed industries			-0.0052**	-0.0020	-0.0033	0.0232***	-0.0088**	-0.0052	-0.0090*	0.0187
			(0.0022)	(0.0026)	(0.0040)	(0.0075)	(0.0037)	(0.0033)	(0.0046)	(0.0119)

Panel B: Teens, repeated cross-section

	Earnings ($)	Employed	exposed industry any	unexposed industry any	exposed industry independent business	exposed industry large C-corp.	Firm size quartile (exposed industries) Q1	Q2	Q3	Q4
All Teens	57	-0.0040	-0.0080	0.0040*	-0.0100***	0.0026	-0.0006	-0.0022***	-0.0001	-0.0050
	(71)	(0.0060)	(0.0052)	(0.0021)	(0.0029)	(0.0022)	(0.0009)	(0.0006)	(0.0011)	(0.0040)
Employed teens	191	.	-0.0079	0.0079	-0.0164***	0.0083**	-0.0007	-0.0039***	0.0010	-0.0042
	(132)	(.)	(0.0064)	(0.0064)	(0.0042)	(0.0036)	(0.0014)	(0.0011)	(0.0018)	(0.0053)

Note: The table above reports employment patterns for individuals from our low-earning panel and repeated cross-sections of teenagers. Employment patterns are tracked by industry, firm type, and size quartile (with size defined by total labor payments). Panel A describes the impact of minimum wage increases on employment transitions between years $s-1$ and $s+2$ for a panel of low-earning workers. These workers are all employed at baseline in either exposed or unexposed industries and the estimates report the change in their transition patterns relative to similar workers in untreated states. Each row describes transition patterns conditional on the industry in which low-earning workers were employed in the base year. Movers columns focus on transition patterns of workers no longer employed by their baseline ($s-1$) employer in year $s+2$. Estimates are from a linear probability model specification of Equation (2) where the dependent variable is an indicator for a worker being employed at a given type of firm in year $s+2$. The lower panel describes employment patterns for randomly sampled repeated cross-sections of teenagers. Appendix L.4 details the construction of these data. The reported estimates convey three-year changes in average employment rates in different industries and firm types measured between years $s-1$ and $s+2$. All standard errors are clustered at the state-cohort level.

L. Administrative Data and Empirical Methods

L.1. Linked Firm-Worker Data: Details and Variable Definitions

The spine of our firm-worker dataset is a 100% sample of all active pass-through firms in treatment and control states from 2010-2019.[50] From the firm income tax returns we keep information on firm revenues, cost of goods sold (COGS), value-added (gross profits, or revenues-COGS), net business income (firm profits or losses), all business deductions (deductible expenses), industry (6 digit NAICS codes), state and zipcode. The line item "other deductions" includes "total allowable trade or business deductions that aren't deductible elsewhere" (quoted from the instructions of Form 1120S).[51] We define non-labor "intermediate costs" as the sum of COGS and the "other deductions" line items and firm "variable costs" as the wage bill plus non-labor intermediate costs.

To each active firm we link all workers in all years, defined as those receiving a W-2 with non-zero wage and salary income from that firm. For each worker, we keep annual wage and salary income from the W-2. We separately link information on each worker's age. Using the information from worker W-2s, we define the total wage bill for each firm-year (the sum of all W-2 wage and salary amounts) and the number of employees in the year (total number of distinct workers receiving wages from the firm in a year). We also use these data to estimate the changes in the distributions of worker characteristics by including the number of workers in different age or earnings groups at each firm and the share of the wage bill going to those within earnings groups (see Figures 2 and 3).

Further, we use earnings information to define a subset of persistently low-earning workers that are more likely to be exposed to minimum wage changes. We define low-earning workers in the base year, *s-1*, based on their earnings history from year *s-2* to year *s* to isolate persistently low earning workers as opposed to those with idiosyncratically low earnings in a given year. We define low-earning workers as those with total wage and salary earnings from all jobs that are less than 40 hours per week for 52 weeks at the minimum wage in each year from year *s-2* to year *s*. We use this information to define the number of low-earning workers at each firm at baseline and the share of the firm's wage bill and variable costs associated with earnings paid to low-earning workers. We note that while this group should contain essentially all minimum wage workers, it may also include consistently part-time workers that earn wages above the minimum wage.

Pass-throughs are required to file a Schedule K-1 on behalf of each owner, which reports the owner's share of firm income in each year. To identify owners, we match each firm with all filed Schedule K-1 reports. Owners can be active, shareholder-employees, or passive owners. Firm owners earn annual profits or losses from their businesses and active owners pay themselves wage and salary income as well. We exclude active owners when calculating the number of workers at the firm and the firm's annual wage bill. We define total owner income as the sum of net business income and the wages and salaries paid to active owners.

[50] Pass-through firms are partnerships and S-corporations that file income tax return forms 1065 and 1120S, respectively. Active is defined as having non-zero net income in a given year.

[51] Some of the eligible deductions listed on the instructions of the form include: Certain business start-up and organizational costs, insurance premiums, legal and professional fees, supplies used and consumed in the business, and utilities.

L.2. Analysis Samples

The various samples of firms used in throughout the analysis are summarized in Table L.23. The full sample of firms contains all active pass-throughs in treatment and control states in the 9 years around each cohort year s, i.e., from *s-4* to *s+4*. The full sample includes approximately 2.6 million firms per year. The main firm results presented in Sections 4.1 to 4.3 use a balanced panel of firms in highly exposed industries active in each year *s-4* to *s+4* to assess minimum wage increases impact operating firms. The balanced panel includes 66,246 treatment firms and 68,917 control firms per year. As robustness, we use an unbalanced panel of firms in highly exposed industries that are active in year *s-1* but may enter after year *s-4* or exit before *S+4* (Appendix Table E.7). This unbalanced panel includes 125,430 treatment and 130,687 control firms in year *s-1*. As a placebo test, we examine the effects of the minimum wage change on firms in non-highly exposed industries (Figure H.7). This balanced panel includes 662,741 treatment and 723,232 control firms per year. The aggregate analyses of firm entry and exit in Sections 4.5 to 4.6 use the full set of active firms in highly exposed industries in each year. This sample includes approximately 130,000 treatment firms and 135,000 control firms per year.

Table L.23: Analysis Samples: Number of Firms

	All	Treatment	Control
All firms	2,634,972	1,254,489	1,380,483
Highly exposed industries	271,308	132,900	138,408
Highly exposed industries (Balanced)	135,163	66,246	68,917
Highly exposed industries (Unbalanced)	256,117	125,430	130,687
Non-highly exposed industries (Balanced)	1,385,973	662,741	723,232

Note: The table above reports sample sizes for the different firm analyses reported in the main text and appendix. Treatment and Controls states are defined in Table A.1.

L.3. Individual Datasets

Low-earning Worker panel. We construct a worker-level panel to study the effects of the minimum wage changes on low-earning workers. Our spine is all low-earning workers living in treatment and control states in base year, *s-1*. We define the state by the state of the address reported on the individual tax return (Form 1040) and define persistently low-earing workers as those with earning between $1-20,000 from all employers in year *s-1* and $0-25,000 in year *s-2*. This spine includes low-earning workers in all industries and working for any type of firm including large corporations.

From this spine, we draw a 2% random sample. From this sample, we link on information for all jobs (Form W-2) for all individuals in each year from year *s-4* to year *s+4*. From the W-2 we keep information on the wage and salary earnings and the employer's (deidentified) Employer Identification Number (EIN). For each job, we collect information about the employer by linking the EIN to information on the industry, the business form (partnership, S-corporation, C-corporation, sole proprietorship or not-for-profit). Using these data, we create a balanced panel of workers, defining total earnings as the sum of wage and salary income from all W-2s in a given year and setting earnings as zero for those "not employed" in a given year, defined as those receiving no W-2s with positive earnings. We define the number of jobs as the number of W-2s from distinct employers in a given year. To assign industry and type of employer, we keep the two highest paying job in each year. Also, for each worker we collect information on the year of birth.

To estimate the firm size of each employer, we combine several pieces of information. First, we link on firm income tax returns from S-corporations (Form 1120S), Partnerships (Form 1065) and C-corporations (Form 1120) and keep information on the annual revenues. For many individuals working for large C-corporations, the EIN on the W-2 does not link directly to a corporate income tax return, either because it is a subsidiary with no direct filing requirement or because the EIN on the W-2 is associated with a payroll service provider rather than the corporation itself. To recover a consistent measure of firm size for all firms, including these large corporations, we collect information from Form 941 (Employer's Quarterly Federal Tax Return), which reports information on total quarterly payrolls and number of employees for payroll tax purposes. For each firm, we average the values across the four quarters to get a measure of annual payroll and employment. We then rank all firms according to their position in the distributions of payroll and employment as a measure of relative firm size. We use quartiles of the distribution of total payroll to estimate differential transitions across the firms size distribution, using the individual LPM specification (2) where the outcome variable is an indicator for working at a firm in a given quartile of the size distribution (Appendix Table K.21).

Young and teen panel. We create a second individual panel focusing on young individuals - those with or without a job in the pre-reform year *s-1*. The sample frame is all individuals ages 15-26 in treatment in control states in year *s-1*. These are individuals who file tax returns on their own, receive a W-2 from an employment relationship, or are listed as dependents on a household return. From this frame, we take a 2% random sample of individuals in the baseline year.

For this spine of individuals, we link on information for all jobs (Form W-2) for all individuals in each year from year *s-4* to year *s+4*. From the W-2 we keep information the wage and salary earnings and the employer's (deidentified) Employer Identification Number (EIN). Using these data, we create a balanced panel of workers, defining total earnings as the sum of wage and salary income from all W-2s in a given year and setting earnings as zero for those "not employed" in a given year, defined as those receiving no W-2s with positive earnings. we define the number of jobs as the number of W-2s from distinct employers in a given year. We perform separate analyses with this panel for those working at baseline and for those not employed (receiving no W-2s) at baseline.

L.4. Repeated Cross-Section of Teens

In Section 5.6, we analyze a repeated cross-section of teenagers in treatment and control states for years *s-4* to *s+4*. To estimate effects on employment rates of concurrent teenagers in each year surrounding the minimum wage increases, we want a representative sample of teens in each year whether employed or not. To do so, we start with a sample frame of all individuals with birth years corresponding to 16-19 year olds in treatment and controls states in a given sample year. The year of birth data is populated for all individuals who filed an income tax return, were claimed as dependents on a parent or guardian's tax return or received a W-2 information report at any point before by year 2023. This allows us to sample teens that may not have been employed in any given year, and may not have been employed yet by the sample year. Yet, administrative tax records are not designed for statistical analysis of employment rates or to make representative subsamples of U.S. populations, so this sample may not be perfectly representative of the population of teens, particularly since it will not contain teens that never appear on a tax return before 2023, which may be disproportionately those that never work or that are from low income families. For this reason, we conduct complementary analysis using the nationally representative American Community Survey (ACS) as described in Section 5.6 and Appendix N.

Starting from this frame of the available population of teenagers in treatment and control states in each year from event years *s-4* to *s+4*, we draw a 2% random sample of teens in each year. For this sample, we link on information for all jobs (Form W-2) in that year. From the W-2 we keep information the wage and salary earnings and the employer's (deidentified) Employer Identification Number (EIN). We define total earnings as the sum of wage and salary income from all W-2s in a given year and set earnings to zero for those "not employed" in a given year, defined as those receiving no W-2s with positive earnings. we define the number of jobs as the number of W-2s from distinct employers in a given year. Using the employer EIN we classify employers by their business form and size in the same way as was done for the low-earning panel as previously described.

L.5. Collapsed Data and Aggregate Effects

Sections 4.5 and 4.6 present aggregate analyses of the effects of the minimum wage on firm entrance and exit, and the effects on sector-level aggregates (where "sector" is defined as independent firms in highly exposed industries), respectively. To conduct these analyses in a well-identified causal framework, we use collapsed datasets to estimate aggregates.

Firm Entry and Exit. To estimate differential firm entry and exit, we create a collapsed dataset that allows us to estimate differential entry and exit rates while controlling for relevant market characteristics to approximate the firm-level regressions. Concretely, we collapse the firm-level dataset by cohort, year, treatment and control states, industry, quintiles of county density, and quintiles of pre-period firm churn at the commuting zone level. The first controls mimic the market level controls from the firm analysis. The last control, for firm churn at the CZ-level is added to control for average pre-period differences in firm entrance and exit across treatment and control states. Pre-period entrance and exit rates are taken from years *s-4* to *s-1*. Data are collapsed by these variables and counts of firms within each cell are retained, differentiating between "incumbent firms", or those that operate in year t and operated in base year $s-1$ and "entrant firms", those that operate in year t but did not operate in year $s-1$. The total number of firms operating in each cell is the sum of the incumbent and entrant firms in that cell.

Regressions are run on the collapsed data using a specification is similar to that described by Eq. 1:

$$y_{hct} = \alpha + \sum_{s=-4, s\neq -1}^{4} (\beta_s treat_{hc} + \Gamma_s Z_{hc}) \times year_{s=t} + \delta_{ct} + \omega_{hc} + \nu_{hct} \tag{L.3}$$

where h indexes treatment or control firms within a given industry-by-county density quintile-by-CZ churn quintile. The outcome variables are scaled by baseline values within cohort and set of market characteristics, h, to estimate relative changes. Concretely, the outcome variables are total firms in hc in year t over the total number of firms in hc in year *s-1*, total number of entrant firms in hc in year t over the total number of firms in hc in year *s-1*, and total number of incumbent firms in hc in year t over the total number of firms in hc in year *s-1*. The regressions are weighted by the baseline number of firms in *hc,s-1*. This methodology allows us to estimate changes in the total number of firms operating, decomposed into entry and exit rates using a DiD framework similar to the main firm-level specifications.

For firm exit rates, which can be estimated using firm-level regressions, we validate the exit results using Specification (1) with the standard set of firm and market controls and the outcome variable being an indicator for the firm operating in year t. Appendix Figure H.9 shows the results of this exercise, which are very similar to the main results using the collapsed data, shown in Figure 6.

Aggregate Effects. We also use collapsed data to estimate the aggregate effects of the minimum wage increases on the sector of independent businesses in highly exposed industries. To do so, we create a simple state-by-industry dataset, collapsing firm-level income statement data by cohort, year, state and industry. We run state-level regressions of the form:

$$y_{rct} = \alpha + \sum_{s=-4, s\neq -1}^{4} (\beta_s treat_{rc} + \Gamma_s \chi_{rc}) \times year_{s=t} + \delta_{ct} + \phi_{rc} + \nu_{rct} \tag{L.4}$$

where r indexes state, χ_{rc} is a state-by-cohort fixed effect and χ_{rc} are a vector of indicators for industry. Outcome variables are state-by-industry totals of a given outcome scaled by baseline (*s-1*) revenue within state-by-industry groups for comparison with the main firm-level results using the balanced panel. Regressions are weighted by baseline revenues.

M. Border Design

We provide additional analyses using a border county design similar to that advanced in Card and Krueger (2000), Dube et al. (2010, 2016) and Allegretto et al. (2017). To implement this design within our context, we use treatment states (T) and the control states (C) in our sample that border these states. Of the 19 treatment locations in our sample, 15 have bordering control states.[52] The state pairs that contain the counties in this analysis are: Arkansas (T) with Mississippi, Oklahoma, Tennessee and Texas (C); California (T) with Nevada (C); Washington, DC (T) with Virginia (C); Delaware (T) with Pennsylvania (C); Chicago (T) with Indiana (C); Massachusets (T) with New Hampshire (C); Maryland with Pennsylvania and Virginia (C); Maine (T) with New Hampshire (C); Michigan (T) with Wisconsin and Indiana (C); Minnesota (T) with Wisconsin and North Dakota (C); Nebraska (T) with Wyoming and Kansas (C); New Jersey (T) with Pennsylvania (C); New York (T) with Pennsylvania (C); South Dakota (T) with North Dakota and Wyoming (C); and West Virginia (T) with Pennsylvania (C). Within these border states, we select firms in counties with centroids within 30 miles of the state border. When a county is within 30 miles of multiple borders, the firms in that county will have multiple observations - one observation per border pair (Dube et al. (2010, 2016)).

This design compares independent firms in highly exposed industries in states that raise the minimum wage to similar firms just across the border in a state that did not have a minimum wage increase in the period. The regression specification is similar to that used in the main analysis (equation (1)), but adding fixed effects for border pairs interacted with year:

$$y_{jct} = \alpha + \sum_{s=-4, s\neq -1}^{4} (\beta_s treat_{jc} + \Gamma_s X_{jc} + \Psi_s I_{jc}) \times year_{s=t} + \delta_{ct} + \psi_{jc} + \nu_{jct} \tag{M.5}$$

where the variables are defined as in equation (1) with the addition of I_{jc} which represents border pair fixed effects.

The results of this analysis are presented in Table M.24. The reported estimates are DiD coefficients for the differential change in the outcome variable for treatment firms relative to control firms from years $s-1$ to $s+4$ (i.e. β_4 from equation (M.5)). Panel A shows results for the main outcomes - revenues, wage bill, COGS and profits scaled by baseline revenues, as well as level employment and the estimated own-wage-elasticity (OWE).

While the magnitudes of the estimated effects on firm outcomes are somewhat attenuated, the qualitative results are very similar when using the border firms as when using the full sample. Revenues raise sufficiently offset the new higher wage and non-labor costs such that owner profits do not decrease. The estimated employment effects and corresponding OWEs are very similar to those estimated using the full sample as well.

[52] Only Connecticut, Rhode Island, Hawaii and Alaska do not.

Table M.24: Effects of the Minimum Wage - Border Design

	Outcomes scaled by baseline revenues, *s-1* to *s+4*					
	Revenue	Wage bill	COGS	Profits	Employment	OWE
All exposed	0.0134*	0.0067***	0.0047	0.0016	-1.011*	-0.238
	(0.0074)	(0.0021)	(0.0029)	(0.0014)	(0.603)	(0.284)

Note: Table M.24 presents results from the border design and regression specification described in Appendix ??. The reported estimates are DiD coefficients for the differential change in the outcome variable for treatment firms relative to control firms from years $s-1$ to $s+4$ (i.e. β_4 from equation (M.5)). The first four columns show the estimated effects on revenues, wage bill, COGS and profits scaled by baseline revenues. The employment effect is estimated in levels and the OWE is estimated by dividing the log employment estimate by an estimate of the log change in average wages, with the standard error estimated using the delta method. The border design uses firms in counties with centroids within 30 miles of the state border. Outcomes are winsorized at the 1^{st} and 99^{th} percentiles. In addition to firm-cohort and cohort-by-year fixed effects, and border-pair fixed effects interacted with year, the specifications include categories of baseline firm size (# of workers), deciles of baseline firm value-added, baseline two-digit industry, quintiles of county density, and quintiles of county employment rates – all flexibly interacted with year to allow for differential time trends. For the first four outcomes, regressions are weighted by log base year revenues. standard errors are clustered at the state-by-cohort level.

N. Earnings and Employment Analysis with Public Data

To complement the individual-level analysis results reported in Section 5, we conducted a similar analysis using publicly-available data from the American Community Survey (ACS). The ACS provides nationally representative cross-sections of the United States population by surveying approximately 295,000 addresses monthly.

As a representative survey of the U.S. population, the ACS is well-suited for providing estimates of the earnings and employment impacts of minimum wage policies. Importantly, these data include individuals who may not have recently filed a tax return — a necessary condition for being included in our linked panel drawn from IRS tax data. This is particularly useful when examining the impact of minimum wage increases on young individuals who are naturally less likely to have filed a return. While our main analysis with the tax data tries to address this issue by including individuals claimed as dependents in prior years, the ACS is designed to capture these and other non-workers. Further, unlike the tax data which report earnings, the ACS captures hours worked, an important dimension of the labor market impacts of minimum wages.

For the ACS analysis, we use IPUMS Amercian Commmunity Survey data for years 2007 to 2020 (Ruggles et al., 2024). Our outcomes of interest are employment, usual hours worked and wage and salary income. To determine whether an individual was employed in a highly exposed industry, we compared the ACS industry code to our list of highly exposed industries based on Current Population Survey data (see Appendix ??). We also use information on individual age, restricting our analysis to individuals ages 16 to 60 years old.

Individuals are classified into treatment and control groups according to their state of residence (see Appendix Table A.1). In the case of Illinois, individuals residing outside of Chicago are classified as control while those residing in Chicago are considered treated. Treated individuals are assigned to one of four cohorts based on the year their state of residence increased its minimum wage. These cohorts are also detailed in Appendix Table A.1.

Our regression specification follows:

$$y_{ict} = \alpha + \sum_{s=-4, s\neq -1}^{4} (\beta_s treat_{ic} + \Gamma_s V_{ic}) \times year_{s=t} + \delta_{ct} + \nu_{ict} \tag{N.6}$$

where, $treat_{ic}$ is a dummy equal to one if the individual resides in a state that raised its minimum wage, V_{ic} is a vector of control variables, δ_{ct} represents cohort-by-year fixed effects, and ν_{ict} is the error term. Note that this specification does not include the individual-cohort fixed effects of our main analysis as the ACS data comprise cross-sections rather than a panel. Like in the main analysis, the construction of the cohorts will mean that individuals in control states may appear in multiple cohorts. We avoid double-counting by clustering at the state-cohort level as described below.

The key parameters of interest are the β_s, which trace the impacts of the minimum wage increases on employment and earnings measures, averaging across the four cohorts. In Appendix Table N.25 below, we present estimates for four age groups: prime-age (16 to 60 years old), teenagers (16 to 19 years old), young individuals (16 to 26 years old), and young adults (20 to 26 years old).

Using the ACS data, we find no evidence of any impact on employment rates or employment in highly exposed industries for any of the age groups, either immediately after the minimum wage increase or four years after. Usual hours worked per week are also unaffected. However, earnings are higher following the minimum wage increase. ACS data show that wage and salary income increases by $1,411 for the all ages group. These gains are concentrated in older workers, as none of the three younger subgroups post statistically significant increases in wages and salary income.

These results help to put our main analysis using tax return data into context.

It is worth noting that for neither of the individual panels of our IRS data presented in Section 5 no longer condition on employment

Appendix Table N.26 reports the sensitivity of the above ACS results to different sets of controls. We focus on the full set of prime-age individuals ages 16 to 60 years. Starting without any controls, each row adds a new set of controls such that the final row corresponds to the prime-age estimates reported in the Appendix Table N.25. Local area characteristic controls are PUMA-level density and unemployment interacted with time fixed effects. Age controls add $age + age^2$ terms. The results are not qualitatively different across the specifications.

Table N.25: Employment and Earnings Effects of Minimum Wage Increase, ACS Data

	Prime age (16-60)	Young workers (16-26)	Teenager (16-19)	Young adults (20-26)
Employed (%)	0.01	-0.57	-1.18	-0.28
	(0.33)	(0.54)	(0.76)	(0.49)
Employed in an exposed industry (%)	-0.01	-0.04	-0.30	0.10
	(0.15)	(0.37)	(0.57)	(0.35)
Usual hours worked per week	0.01	-0.12	-0.21	-0.09
	(0.14)	(0.23)	(0.24)	(0.25)
Wage and salary income	1,410.85	242.76	-76.66	374.76
	(274.99)	(176.84)	(91.43)	(232.42)

Note: The table above reports estimates of Appendix Equation (N.6) using ACS data from 2007 to 2020 where the outcomes are 1) an employment dummy, 2) a dummy indicating a worker is employed in a highly exposed industry, 3) usual hours worked each week, and 4) total wage and salary income. Estimates report impacts four years after the policy change ($s+4$) relative to the year before the policy change ($s-1$). We estimate linear probability models for binary outcomes. An individual is considered employed in an exposed industry if their ACS industry code corresponds with the list of highly exposed industries listed in Appendix Table B.3. Regressions are weighted by ACS person weights and standard errors are clustered at the state-cohort level.

Table N.26: Employment and Earnings Effects of Minimum Wage Increase: Specification Comparison, ACS Data

	No controls	With cohort-year fixed effects	With local area characteristic controls	With linear and nonlinear age controls
Employed (%)	0.45	0.37	0.05	0.01
	(0.39)	(0.35)	(0.34)	(0.33)
Employed in an exposed industry (%)	0.69	0.04	-0.02	-0.01
	(0.43)	(0.16)	(0.15)	(0.15)
Usual hours worked per week	0.22	0.17	0.03	0.01
	(0.16)	(0.15)	(0.15)	(0.14)
Wage and salary income	1,387.57	1,515.15	1,464.15	1,410.85
	(322.49)	(293.63)	(301.15)	(274.99)

Note: The table above reports estimates of Appendix Equation (N.6) for prime-age individuals (ages 16 to 60 years) using ACS data from 2007 to 2020. The outcomes examined are 1) an employment dummy, 2) a dummy indicating a worker is employed in a highly exposed industry, 3) usual hours worked each week, and 4) total wage and salary income. Estimates report impacts four years after the policy change ($s + 4$) relative to the year before the policy change ($s - 1$). We estimate linear probability models for binary outcomes. For each outcome, the table presents estimates from alternative specifications with different, additive controls. Local area characteristic controls are PUMA-level density and unemployment interacted with time fixed effects and age controls include $age + age^2$. Regressions are weighted by ACS person weights and standard errors are clustered at the state-cohort level.

O. Dynamic Impacts: Minimum Wages and Imperfect Competition in Product Markets

We analyze a framework of Cournot competition with fixed costs to highlight the role of extensive margin responses and embed asymmetric production technologies to highlight how selection on productivity can further mediate the observed effects of the minimum wage. Consider a market characterized by Cournot competition among N firms facing market demand $Q_D(P)$ with asymmetric (but constant) marginal costs, c_i, which yields a familiar expression for the price-marginal cost margin.

$$\frac{P - c_i}{P} = \frac{s_i}{\varepsilon_D(Q_D)}$$

where P is the common output price, ε_D is the absolute value of the elasticity of consumer demand and varies with market quantity, and s_i denotes the market share of firm i selling quantity q_i:

$$s_i = \frac{q_i}{Q_D} = \frac{q_i}{\sum_i q_i}$$

Market shares are proportional to margins, with the most efficient firms enjoying both the largest margins and market shares. If costs are symmetric, the N firms simply split the market equally.

Minimum wages will increase marginal costs to the extent firms employ workers at wages below the new floor. As such, the marginal cost increase, Δ_i^c, can vary by firm. The resulting market share for firm i will be:

$$\frac{P^{\bar{w}} - (c_i + \Delta_i^c)}{P^{\bar{w}}} = \frac{(s_i + \Delta_i^s)}{\varepsilon_D(Q_D^{\bar{w}})} \tag{O.7}$$

where the super-script $\bar{w}$ denotes post-minimum wage hike prices and quantities, and Δ_i^s is the resulting change in market share.

Heterogeneous technology implies reallocation in response to a cost shock. When firms have heterogeneous ex-ante productivity, c_i, even when the marginal cost shock is the same for all firms, $\Delta_i^c = \Delta^c$, there will be reallocation of market shares toward the more productive firms as their margins shrink relatively less. Asymmetric cost shocks will lead to reallocation from high shock to low shock firms.[53] In any case, without a decline in the number of firms, average profits and average quantities per firm will decrease along with the total quantity in the market.

We find that minimum wage increases reduce firm entry; that is, for some firms, the added marginal cost of complying with higher wage floors will exceed market price increases, and these narrower margins will leave them unable to cover their fixed costs f, or

$$\pi_i^{\bar{w}} = \left(P^{\bar{w}} - (c_i + \Delta_i^c)\right) q_i^{\bar{w}} - f < 0.$$

Firms choosing not to enter could be those with the lowest *ex ante* productivity (high c_i), the largest cost shock due to a high expected share of low-wage labor (high Δ_i^c) per unit of

[53] Depending on the differentials between the shocks, asymmetric shocks can cause profits and quantities to increase among some firms.

output, or a combination of the two. Our empirical evidence indicates that reduced entry results in a market with approximately 2% fewer firms.

Under Cournot competition, a reduction in the number of players with unchanged costs will lead to higher markups and higher market shares for operating firms, with the most cost-efficient firms gaining the most. When a cost shock reduces the number of firms, the market price will rise due to both the increase in market power and cost pass-through. Even with homogeneous firms that experience symmetric cost increases, average profits can remain flat if the number of operating firms declines sufficiently relative to the cost shock.

When heterogeneity shapes entry, such that the least productive firms or those that intended to rely most heavily on low-wage labor decide not to enter, market quantity is reallocated to more efficient firms, leading to smaller declines in market quantity and making it more likely that average profits remain unchanged. Larger markups for some firms will be matched by higher market shares ($\Delta_i^s > 0$ on the right-hand side of equation (O.7)), offsetting potentially more elastic demand as prices rise. For other firms, the added marginal cost of complying with the minimum wage can narrow margins and reduce market shares.

We find that minimum wage increases block the entry of less productive (lower value-added) and less efficient (higher material costs/revenue) firms. More efficient firms can thus benefit from minimum wage increases through the combination of higher margins and larger market shares as demand is reallocated to incumbent firms and the firms that still enter. Empirically, we estimate that average revenue increases significantly by 4.15% (0.00475) while aggregate revenue increases only 2.61% (0.0202), indicating that revenues are reallocated over a smaller number of firms following the minimum wage increase. Appendix Table F.10 shows that among incumbent firms, those with the highest baseline value-added see the strongest revenue growth. Profits do not decline among operating incumbents and entrants as a result of this added revenue.

While the model suggests that non-entry could be associated with low productivity or high exposure (i.e. a large share of low-wage labor), our findings suggest that productivity and efficiency are the main factors determining who enters. Entering firms feature high value-added and material cost efficiency, suggesting that minimum wage increases deter less productive and efficient firms, leaving the pool of actual entrants highly selected. As reported in the second column of Table 4 wage bills rise similarly among incumbent firms and entrant firms, indicating that while low-productivity firms are deterred from entry, there is no such differentiation by labor use.

The empirical entry dynamics correspond with the implications of this framework, clarifying the nature of selection and helping explain why firms seem able to weather the cost pressures of higher wage floors. By raising production costs, higher minimum wages cull the least productive independent firms and force entrants to be leaner, yielding a more productive distribution of firms after the policy change. This pattern is consistent with Sorkin (2015)'s insight that minimum wage policies shape the choices of entrants, and may hint at larger longer-run employment impacts of higher minimum wages. Through entry, and demand reallocation, the minimum wage shapes the productivity distribution of independent firms in a manner akin to how the emergence of new technologies (Collard-Wexler and De Loecker, 2015), exposure to international trade (Melitz, 2003), and recessions (Osotimehin and Pappadà, 2017) can shift the productivity distribution in an industry.

In addition to the reallocation of revenues among independent businesses, we find evidence that employment relationships are reallocated from the least productive independent firms to C-corporations. Table 5 shows that low-earning workers and teens are both less likely to work at highly exposed independent businesses and more likely to work at C-corporations after minimum wage increases while Appendix Table F.10 shows that lower value-added firms drive the decline in employment relationships among incumbent independent businesses. This pattern may help explain why some corporate firms support minimum wage increases despite the higher labor costs they will face.[54] Yet, we underscore that there may be other welfare-relevant margins not captured by this model. For example, Kroft et al. (2024) develop a model of imperfect competition with consumers who have love-of-variety preferences to analyze the welfare effects of commodity taxes. They show that both the externality associated with firm entry and exit, as highlighted here, and the loss of variety associated with exit are welfare-relevant and the loss of variety may dominate in many cases.

[54]See Amazon in addition to the polls cited in the Introduction.