Now You See It, Now You Don't: Financial Constraints, Minimum Wage Policies, and Employment

Alona Bilokha   Iftekhar Hasan
Stefano Manfredonia   Ronald W. Masulis

Abstract

Corporate financial constraints play a crucial role in explaining the effect of minimum wage policies on employment levels. Using a border discontinuity design and establishment and firm-level information, we show that minimum wage increases, on average, have no significant impact on employment levels. However, we find a negative and economically significant effect on employment at establishments belonging to financially constrained firms. We provide causal evidence about this relationship using a unique quasi-experimental setting, namely, the federal minimum wage rise during the 2007-2008 financial crisis combined with an exogenous financial constraint due to ex-ante heterogeneity in firms' long-term debt maturity structure.

Keywords: Minimum Wage; Financial Frictions; Employment.
JEL Codes: G30, G32, J30, J38.

* Alona Bilokha is affiliated with the University of North Florida. Iftekhar Hasan is affiliated with Fordham University and the Bank of Finland. Stefano Manfredonia is affiliated with Fordham University. Ronald W. Masulis is affiliated with the University of New South Wales. Corresponding author: Ronald W. Masulis (*E-mail*: ron.masulis@unsw.edu.au; *Address*: UNSW, Sydney, NSW 2052, Australia). Declarations of interest: none. The authors thank Paolo Volpin and seminar participants at Fordham University, the University of Sydney, the AFA Annual Meeting, and the FMA Annual Meeting for their valuable comments and suggestions. The usual caveats apply.

1 Introduction

Minimum wage laws in the United States differ significantly from state to state, with the Federal government establishing a baseline minimum wage that states can choose to exceed. The introduction of The Raise the Wage Act in Congress on July 25, 2023, has intensified the minimum wage discussion by proposing to raise the federal minimum wage from $7.25 an hour, first set in 2009, to $17 an hour by 2029.[1] This congressional proposal is projected to directly raise the salaries of over 27 million U.S. workers, representing nearly 20% of the wage-earning workforce. As a result, minimum wages have risen to the forefront of current U.S. government policy debates (Zipperer, 2023).

One central point of contention in these debates involves the impact of minimum wage law changes on employment levels. An in-depth understanding of this relationship is of great importance to employers, lower-income workers, and policymakers, as it speaks directly to the welfare implications of minimum wage policies. Despite a large body of economic research investigating this relationship, a clear consensus has yet to emerge on whether minimum wages affect employment, with published evidence drawing fundamentally different conclusions.[2]

Our primary objective is to shed new light on this minimum wage-employment relationship by exploring the role of corporate financial characteristics at the individual firm level in determining firm employment responses to labor factor price shocks. Indeed, the causal chain between minimum wages and employment studied in the prior literature ignores the potential role that firms and their balance sheets can play in employment decisions. Yet, there are compelling reasons to believe that firm-level factors can play a crucial role in determining the impacts of minimum wage increases on aggregate labor demand. Firms decide whether to hire, fire, or retain employees and experience wide-ranging heterogeneity in their available resources, exposure to state regulations, and financial flexibility. From

[1] For details, see https://www.cbo.gov/publication/55681 (this reference was last accessed on September 18, 2024).

[2] For example, in a widely cited study, Card and Krueger (1994) analyze the effect of an increase in New Jersey's minimum wage and show that fast-food restaurants in this area increased employment by 13% relative to nearby Pennsylvania stores. Cengiz et al. (2019) reports no significant change in the total number of low-wage jobs during the five years after a minimum wage adjustment. Using survey information at the individual level, Clemens and Wither (2019) identify a negative effect on employment following the federal minimum wage increase during the 2007-2008 financial crisis. For reviews of the extensive literature on the effect of minimum wage policies on employment in the United States, see Neumark and Wascher (2008), Belman and Wolfson (2014), and Schmitt (2015).

this perspective, recent empirical evidence emphasizes the importance of firms' financial constraints in understanding aggregate employment dynamics (Giroud and Mueller, 2017, 2019). A more precise understanding of the role of firm characteristics in this context can aid policymakers in designing more effective minimum wage policies and helping to reconcile previously mixed empirical findings in the literature. Our paper aims to help fill this research gap by analyzing whether corporate financial constraints are essential in explaining employment dynamics following minimum wage increases and uncovering the underlying mechanisms that link the two. For this purpose, we use a unique combination of establishment- and firm-level data.

While previous research in finance has predominantly examined how constraints on accessing external finance affect firms' capital investment decisions (e.g., Hubbard, 1998), financial constraints can distort other major firm decisions, especially those that have cash flow implications (Almeida et al., 2024). For example, financially constrained firms can prefer projects that yield more significant immediate cash realizations or larger cost savings (Eisfeldt and Rampini, 2007; Caggese et al., 2019). For this reason, an increase in labor costs resulting from a minimum wage increase may compel financially constrained firms to reduce their employment levels. This decline in employment at the firm level may also occur because firms cannot cover additional labor expenses from their internal resources. This phenomenon arises when firms face financial constraints, as they can not borrow against their future cash flows. Several factors further exacerbate this situation, including an inability to use labor as collateral and various fixed costs of adjusting employment levels (e.g., search, hiring, and training costs) (Oi, 1962; Ghaly et al., 2017), which presents specific challenges to financing labor costs (Almeida et al., 2012; Benmelech et al., 2019, 2021). Furthermore, since constrained firms are expected to place a relatively high premium on preserving liquidity (Almeida et al., 2004; Faulkender and Wang, 2006; Denis and Sibilkov, 2010; Dasgupta et al., 2019), they are more likely to immediately react to an increase in labor costs by reducing employment.

To advance our research agenda, we begin by analyzing the effect of changes in minimum wages on employment at the establishment level. This approach is more precise than solely examining firms and their headquarters location, as minimum wage laws are based on the locations of their employees, and many firms have employees spread over multiple states and internationally. We begin by extracting information on all establishments that belong to publicly traded firms in the United

States, as well as data on firm balance sheet characteristics.[3] In advancing the previous literature, our combined firm and establishment-level database enables us to compare the same establishments, located in different states, but belonging to the same firm, before and after a minimum wage change, while controlling for aggregate firm-level trends. Our final database comprises more than two million establishment-year observations. As a result, we provide an in-depth perspective on the underlying employment patterns and a more complete picture of how minimum wage policies affect both firm and establishment employment levels.

Our empirical analysis recognizes that minimum wage policies are not fully exogenous; the timing of minimum wage raises can be closely tied to the health of the local/state economy (Neumark et al., 2014; Allegretto et al., 2017). Thus, an increase in the minimum wage could be correlated with changes in other economic characteristics that can also affect firm employment decisions. To address this problem, we follow the previous empirical literature by combining staggered state-wide changes in minimum wage levels with a border discontinuity design to establish a more controlled experimental setting where local economic conditions can be netted out (Card and Krueger, 1994; Mukherjee et al., 2017). For this purpose, we again exploit our firm and establishment database by selecting establishments located across county lines within similar local economies, but in adjacent states. Thus, these establishments have differing minimum wage policies and are exposed to different quasi-random minimum wage changes.

Using the above empirical strategy, we estimate the dynamic effects of changes in minimum wages on the employment levels of establishments exposed to these differential regulations. Importantly, we show that the parallel trend assumption underlying our identification condition has empirical support; specifically, establishments located along opposite sides of state borders have similar employment patterns before a minimum wage increase. In addition, we show that a rise in the minimum wage does not affect establishments' employment in subsequent years. This finding is consistent with other studies that do not find that increases in the minimum wage have an effect on aggregate employment

[3] While several existing studies use establishment-level databases to examine the effects of minimum wage policies on employment and wages (e.g., Chava et al., 2023; Bossler and Gerner, 2020; Gopalan et al., 2021; Dustmann et al., 2022), our paper is the first to merge establishment-level data with key details from a firm's balance sheet. This integration of firm- and establishment-level data enables us to obtain new insights into the interplay between minimum wage policies and corporate employment dynamics, explicitly shedding light on the pivotal role of corporate financial constraints in this context.

during normal economic times (e.g., Cengiz et al., 2019; Dustmann et al., 2022).

Given our research hypothesis, we focus on the role of corporate financial characteristics in this setting to show that the average effect of minimum wage increases conceals significant heterogeneity at the individual firm level. By examining the interaction between minimum wage levels and measures of firm-level financial constraints, we document a significant adverse effect on the employment levels of constrained firms following minimum wage rises. More specifically, our findings indicate that a one standard deviation rise in a firm's financial constraints, represented by alternative conventional financial constraint metrics, leads to an average 0.2% reduction in employment at the affected establishments following a one-dollar increase in the minimum wage. In addition, while examining the marginal effects of minimum wage rates on employment across the entire distribution of the financial constraint variables, we detect some positive, but mostly insignificant effects on employment at unconstrained firms, suggesting that these firms may be better positioned to absorb increased labor costs and benefit from the positive aspects of minimum wage increases, such as reduced turnover, lower training costs, and a larger pool of workers willing to work at higher salaries (Portugal and Cardoso, 2006; Brochu and Green, 2013; Dube et al., 2016, 2019; Coviello et al., 2022; Ku, 2022).

While some studies in this literature concentrate their empirical analysis on corporations operating within specific industries perceived as being particularly susceptible to minimum wage hikes (e.g., Card and Krueger, 1994; Cengiz et al., 2019; Agarwal et al., 2024), our paper considers the broader impact of minimum wage policies across a full range of industries (Neumark et al., 2004; Meer and West, 2016). Consequently, we estimate the average effect of minimum wage changes on aggregate employment, accounting for sector-level variations by utilizing establishment and firm-year fixed effects. This method is particularly appealing given the significance policymakers place on understanding the overall employment responses to minimum wage policies (Cengiz et al., 2019).[4]

To delve deeper into this employment question, we examine the influence of minimum wage changes and financial constraints on employment at heavily minimum wage exposed and non-exposed industries

[4] For example, the Congressional Budget Office (CBO), in attempting to estimate the effect of an increase in the federal minimum wage on overall employment, acknowledged the scarcity of studies at this level of aggregation (Cengiz et al., 2019). Consequently, the CBO proceeded to use estimates of minimum wage elasticities for teenagers and specific sectors particularly affected by minimum wage policies to project the broad effects on employment. Details are available at https://www.cbo.gov/system/files/2019-07/CBO-55410-MinimumWage2019.pdf (this reference was last accessed on September 18, 2024).

(Gustafson and Kotter, 2023). Our findings reveal economically and statistically significant results for financially constrained establishments operating in industries most reliant on minimum wage labor. When we analyze the results for non-minimum wage-reliant industries, the coefficients are still consistent with the negative ones reported in our baseline specification. However, their precision is considerably reduced, rendering them statistically insignificant at conventional levels.

Our analysis also explores whether employees move from financially constrained to unconstrained establishments following minimum wage shocks (Chodorow-Reich, 2014; Mian and Sufi, 2014; Gilchrist et al., 2017). In this setting, the degree of labor force redistribution at the local level relies on various considerations, including employee-firm search and matching frictions within labor markets, along with the costs associated with these labor market adjustments. Therefore, from a theoretical perspective, it is unclear whether this redistribution process impacts local employment levels following minimum wage changes. Thus, to account for general equilibrium effects in our empirical analysis, we adopt the methodology outlined in Dube et al. (2010), which estimates county-level regressions. Using this approach, we consistently find that counties with greater exposure to establishments facing more binding financial constraints exhibit greater employee dismissals following a rise in the state's minimum wage.

Unlike previous research analyzing the relationship between minimum wage and employment at the establishment level, we also consider the possibility that managers may relocate employees from establishments subject to minimum wage regulations to those that are not. This could result in total employment at financially constrained firms remaining largely unaffected following minimum wage changes, while employment at treated establishments is significantly reduced.[5] Nevertheless, even when we examine our data at the firm level, where labor transfers would face lower frictions, we consistently find a significant negative effect of minimum wage changes on total employment for financially constrained firms. This confirms the results obtained from our establishment-level analysis and fails to support the hypothesis that *within* firm reallocation of labor could mitigate or eliminate

[5] On this point, Dustmann et al. (2022) provide evidence that minimum wage policies lead to the reallocation of low-wage workers *across* establishments: from smaller to larger, from lower-paying to higher-paying, and from less productive to more productive ones. Their results underscore the importance of considering labor reallocation to obtain a more comprehensive understanding of how the labor market absorbs wage increases induced by the minimum wage regulation, which our database's unique structure enables us to investigate from a corporate perspective.

our establishment-level findings.

Our firm-level investigation provides further insights into the mechanisms through which minimum wage policies affect the employment dynamics of financially constrained firms. We observe a decline in corporate cash holdings after minimum wage increases. Because exposure to minimum wage policies affects a firm's available internal resources, companies subject to these wage policies may need to acquire additional external financial resources to meet their short-term liquidity needs. Consistent with this hypothesis, we document an increase in the use of trade credit and bank debt (Biais and Gollier, 1997; Giannetti et al., 2011; Custódio et al., 2013). Furthermore, we observe a decrease in investments by those firms. These findings suggest that firms more exposed to minimum wage increases reduce various expenditures to address their liquidity needs (Gustafson and Kotter, 2023).

Our primary specification leverages quasi-random exposure of establishments to minimum wage policies and incorporates a comprehensive set of fixed effects. This approach enables us to compare the same establishment before and after a minimum wage change, while effectively controlling for firm-level aggregate trends. While this approach addresses several endogeneity issues, it does not definitively establish causality due to potential correlations between financial constraints and other corporate characteristics that may influence employment decisions.

In the paper's last section, we adopt a unique quasi-experimental setting to provide causal evidence on the relationship between minimum wage changes, financial constraints, and employment levels to address this endogeneity concern. More specifically, we examine the rise in the federal minimum wage during the 2007-2008 financial crisis and compare employment dynamics of establishments located in states affected by the federal minimum wage change to establishments located in states unaffected by the federal wage increase (i.e., states where a state's minimum wage was equal to or exceeded the new minimum wage level mandated by the federal government) (Gustafson and Kotter, 2023). Indeed, during the financial crisis, establishments in affected states experienced significantly more growth in their *effective* average wage rates compared to establishments in unaffected states (Clemens and Wither, 2019). This crisis period is also particularly relevant for our analysis since it is characterized by severe information asymmetries, high economic uncertainty, and a tight credit market, making access to external financing more difficult (Brunnermeier, 2009; Bernanke, 2023).

Utilizing this identification strategy, we discover that increasing the minimum wage during this critical period significantly reduces employment at treated establishments. More specifically, we observe a 0.45% decrease in employment at establishments located in states affected by the new federal minimum wage. Furthermore, we show that this effect is immediate and persists for several years.

To ensure the reliability of our empirical findings regarding the impact of financial constraints on employment after minimum wage changes, we introduce an exogenous shock to a firm's access to external finance. To do so, we follow the previous literature (Almeida et al., 2012; Benmelech et al., 2019; Duval et al., 2020), and exploit the heterogeneity across firms in the relative portion of their long-term debt that matures at the onset of the financial crisis. The rationale behind this approach is that firms with large amounts of debt maturing during this financial crisis period are generally unable to roll over their maturing debt, given the serious disruption to capital markets, causing these firms to experience more binding financial constraints. Thus, these firms are compelled to modify their behavior to a greater extent than similar firms without the need to refinance their long-term obligations during the crisis period.

In line with our previous results, we find that establishments belonging to firms with a large amount of debt maturing at the onset of the financial crisis and located in states affected by the new federal minimum wage requirements experienced greater declines in their establishment employment levels. Specifically, our analysis shows that a one standard deviation rise in a firm's financial constraint measure leads to a 0.4 % decline in its treated establishments' employment levels. A counterfactual exercise provides further evidence that minimum wage policies could substantially negatively affect *aggregate* employment in the presence of widespread financial constraints.

Our paper contributes to several distinct strands of literature. In finance, a growing body of literature has analyzed the effect of minimum wage laws on corporate policies. Gustafson and Kotter (2023) find that increases in minimum wages in the United States lead public firms to cut capital expenditures. Geng et al. (2022) find opposite results when they analyze the effect of minimum wage increases in China and show in a sample of manufacturing firms that a rise in the minimum wage is associated with increased capital investment and innovation. Using a similar setting, Hau et al. (2020) find that minimum wage increases in China accelerate input substitution from labor to capital, reduce employment growth, and accelerate total factor productivity growth. They also show that this effect is particularly strong among less productive firms under private Chinese and foreign ownership, but this does not occur among state-owned enterprises. They argue that differences in management practices can explain this heterogeneous effect. Also, examining a large and persistent minimum wage increase in Hungary, Harasztosi and Lindner (2019) show that firms responded to a minimum wage rise by substituting more capital in place of labor. Agarwal et al. (2024) exploit staggered state-level changes in minimum wages in the United States from 2000 to 2008 and, using a comprehensive data set from the hotel industry, find that doubling the minimum wage reduces average hotel revenues by 6% per year and occupancy rates by 3.1%. Chava et al. (2023) show that federal minimum wage increases negatively affect the financial health of small, predominately private businesses in the affected states. Additionally, their findings indicate a causal relationship between minimum wage hikes and increased small business closure rates.

We contribute to this strand of the literature by highlighting the role of corporate balance sheet characteristics in explaining employment dynamics after a minimum wage rise. Furthermore, our analysis at the firm level highlights the effect that exposure to minimum wage policies has on a firm's operating and financial performance and investment policies. Among our many findings, we show that minimum wage policies exert pressure on firm internal resources and compel firms to seek access to additional external capital and/or to cut essential expenditures.

We also contribute to the literature that analyzes the impact of financial frictions on real outcomes. Duval et al. (2020) show that firm financial constraints during the 2008 global financial crisis have a persistent negative effect on firm productivity and innovation outcomes. Benmelech et al. (2019) provide evidence that the lack of access to credit combined with financial frictions harmed firm employment during the Great Depression. Caggese et al. (2019) use matched employer-employee data from Sweden and find that financing constraints push firms to sub-optimally fire short-tenured workers who can offer high expected future productivity. Chodorow-Reich (2014) shows that firms with weaker lender relationships had more difficulty obtaining loans and experienced higher interest rates after the Lehman bankruptcy. They also exhibited greater reductions in employment compared to companies with stronger lending relationships. Giroud and Mueller (2017) discovered that businesses belonging to highly leveraged firms suffered significantly greater reductions in employment when faced with a decrease in local consumer demand. Gilchrist et al. (2017) document that firms with liquidity constraints raised prices during the 2008 financial crisis, while their unconstrained counterparts lowered prices.

In a recent related paper, Almeida et al. (2024) find that funding frictions can limit firms' short-term investments in receivables and inventories, reducing their production capacity. Furthermore, financial constraints are shown to have a significant impact on various firm decisions, including investment and capital structure choices, as well as stock returns (Hennessy and Whited, 2007; Lamont et al., 2001; Cao et al., 2019).

Our analysis reveals a previously unexplored setting where financial frictions can yield real impacts. More specifically, we analyze a factor price shock to corporate internal resources that forces financially constrained firms to reduce their employee headcount.[6] By doing so, we provide evidence that borrower financial weaknesses and the inability to access financial markets provide a plausible explanation for the rise in unemployment after a minimum wage hike (Clemens and Wither, 2019). This result is particularly relevant, considering the challenges that a large body of literature faces when explaining the mixed results obtained from analyzing minimum wage changes in different countries and periods (Belman and Wolfson, 2014).

To the best of our knowledge, there are no other studies that analyze the relationship between minimum wage changes, establishment- and firm-level employment, and corporate balance sheet characteristics. The only study that takes financial constraints into account in this literature is Arabzadeh et al. (2024), where the authors consider a change in the minimum wage in Germany and use employee-employer information and a structural model to investigate the relationship between minimum wage changes, financial frictions, and within-firm wage dispersion. More specifically, they find that within-firm wage dispersion declines more with higher minimum wages when firms are financially constrained.

More broadly, our paper is inspired by the recent strand of literature arguing that optimal government policies should take into account financial frictions (Itskhoki and Moll, 2019; Caballero and Lorenzoni, 2014). According to this literature, when financial frictions constrain firms, governments

[6] Other related studies report that cash holdings are more valuable to financially constrained firms than to unconstrained firms (Denis and Sibilkov, 2010; Faulkender and Wang, 2006; Almeida et al., 2004). Beuselinck et al. (2021) examine the relationship between employee protection laws and cash holdings, demonstrating that legislation providing greater employee protection against termination leads to an increase in firm cash holdings. Ghaly et al. (2017) show that firms with a higher share of skilled workers, which reduces their ability to adjust their employee demand in response to negative cash flow shocks, hold more precautionary cash. They also show that this effect is much stronger for financially constrained firms. Dasgupta et al. (2019) demonstrate both theoretically and in empirical analysis that financially constrained companies, when faced with favorable product market conditions, are restricted in their ability to expand their inventory levels at the same pace as financially unconstrained firms.

must prioritize policy interventions that involve the temporary reduction of wages to elicit increases in labor demand. By doing so, they can accelerate the accumulation of entrepreneurial wealth, promote higher labor productivity, and, ultimately, increase wages and labor supply over time. Our results empirically confirm this hypothesis by focusing on the effect of minimum wage policies on employment, one of the most discussed government interventions in the economy. In line with this strand of literature, our results show that governments should avoid raising minimum wages when many firms are in fragile financial conditions and when it is more difficult for them to access external capital.

2 Data

To advance our research, we collect information on minimum wage policies across individual US states, listed firms' establishment characteristics by state, and the balance sheet characteristics of the listed firms to which the establishments belong. Our variables and their data sources are described in more detail below.

Minimum wage changes and counties on state borders. We source information on minimum wage policies from Vaghul and Zipperer (2021), who provide details about the variations in the state-level minimum wage spanning from 1974 to 2020. However, since we only possess information regarding the establishments from 1990 to 2020, our analysis primarily centers on this sample period. Moreover, the results of our analysis are unlikely to be significantly affected by the inclusion of data from the 1980s since there were minimal changes to minimum wage rates before 1990 (Neumark et al., 2014; Allegretto et al., 2017).

Figure 1(a) illustrates minimum wage dynamics for each state. Our empirical analysis takes advantage of this significant variability in minimum wage changes across states and periods. We also consider that minimum wage policies can be endogenous since they are strongly tied to changes in local economic patterns. To deal with this problem, we follow the previous literature and focus our attention on geographically adjacent treated and control counties located along states' borders to ensure that omitted local economic variables do not affect our results (Card and Krueger, 1994; Mukherjee et al., 2017). In this way, we compare counties that are heterogeneously exposed to increases in the minimum wage because they are located in geographically adjacent states and, therefore, are expected to be similar in both observable and unobservable local economic conditions.

To further ensure the validity of our analysis, we take into account the concern raised in Dube et al. (2010) that counties on a state's borders in the western US are much larger and irregular in shape. As such, we further investigate county pairs that share a state border and whose centroids are within 75 km of each other. This distance cutoff has been determined through a data-driven randomization inference procedure, which minimizes the mean squared error of the estimator (Dube et al., 2010). In Figure 1(b), we highlight these counties by shading them in dark green.

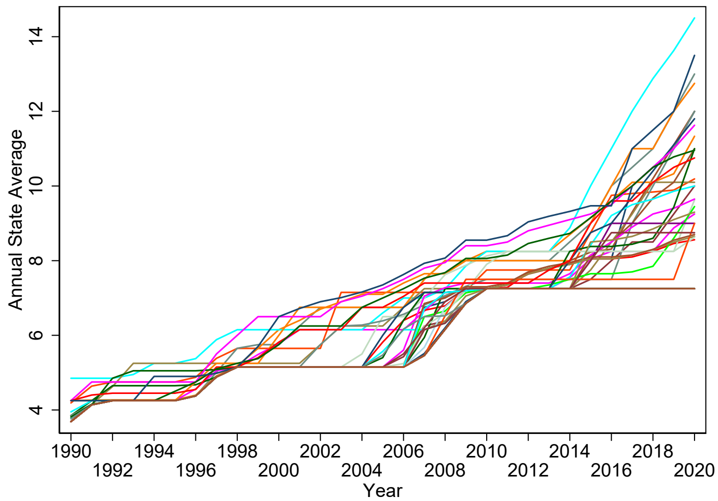
Establishment level information. We gather data on establishments of public corporations in individual states through the National Establishment Time Series (NETS) database (Addoum et al., 2020, 2023). It contains comprehensive information on the characteristics of publicly and privately owned establishments, including their locations from 1990 to 2020. Using this data, we can accurately analyze the impact of minimum wage changes on employment levels. One advantage of this database is that it is not subject to survivorship bias, which is a major consideration in our analysis of the effect of minimum wage policies on the number of operating establishments.

We provide a graphical representation of the geographical distribution of establishments in our sample in Figure 2. We find at least one establishment belonging to a public corporation in almost all the counties across the United States. This broad geographic coverage of establishments enables us to effectively study the impact of changes in the minimum wage by analyzing the counties on opposite sides of state borders across much of the country.

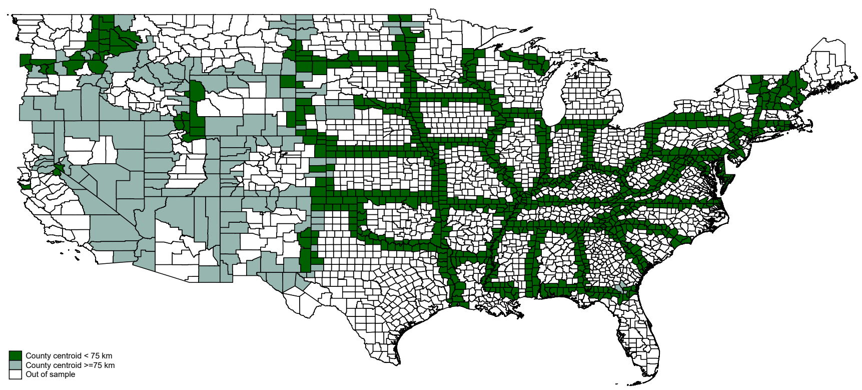
Corporate balance sheet characteristics. From Compustat, we obtain comprehensive information regarding the balance sheet characteristics of publicly traded U.S. companies. Using this database, we develop alternative metrics for evaluating financial frictions at the corporate level.

Our first approach uses size as an indicator of financial frictions, as previous research reports that smaller firms are more financially constrained (Gertler and Gilchrist, 1994; Siemer, 2019). We next consider the Whited and Wu (WW) financial constraint index (Whited and Wu, 2006). The authors propose estimating an Euler Equation derived from a structural investment model to create this measure. The index is constructed from six components: cash flows, assets, dividends, debt, and

Figure 1: Minimum wage dynamics and counties on the states' border



(a) Minimum wage policies



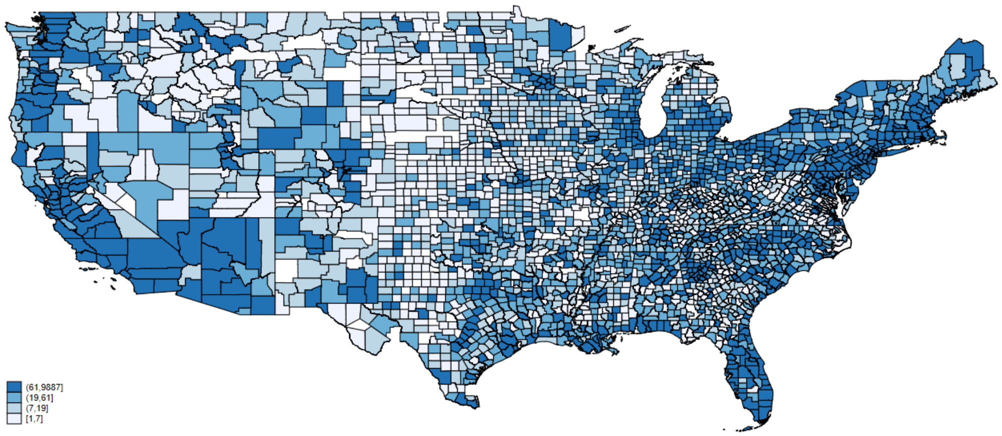
(b) Counties on state borders

Notes: Figure 1(a) shows changes in minimum wage policies for each state during our period of analysis. Figure 1(b) shows counties located on the states' border. The areas shaded in dark green represent pairs of counties that share a border between them and have their centroids within a 75 km distance of each other.

sales growth at the industry and firm levels.

To enhance the robustness of our findings, we also include the size-age (SA) index as an additional measure of corporate financial constraints (Hadlock and Pierce, 2010). The SA index is constructed by sorting firms according to characteristics closely linked to financial constraints. In particular, Hadlock and Pierce (2010) have identified corporate size and age as the factors most strongly associated with financial constraints. These two features are considered to be much less endogenous than other variables that are commonly used to estimate financial constraints, such as cash levels and leverage,

Figure 2: Geographical distribution of the establishments



Notes: Figure 2 displays the number of establishments across the counties in the United States in our sample.

which can be subject to discretionary decisions made by a firm's management. The SA index suggests that financial constraints decrease substantially as young and small firms mature and grow.[7]

Next, we construct an exogenous measure of financial frictions based on the ex-ante variation across firms regarding their long-term debt maturing during the financial crisis. The rationale behind this approach is that the financial crisis's impact on credit conditions was unexpected, and firms could not deliberately schedule their debt to mature just before the crisis to mitigate rollover risk. Therefore, the debt structure of firms before the occurrence of this unexpected event is unlikely to be correlated with any other unobserved firm characteristics or to the exposure of each establishment to changes in the minimum wage (Duval et al., 2020; Almeida et al., 2012; Benmelech et al., 2019).

Figure OA1 shows our sample firms' long-term debt maturity distribution. As expected, there is substantial heterogeneity in debt maturity across the years. We exploit this heterogeneity in our empirical analysis. Specifically, we consider as distinct variables the amount of debt maturing over the financial crisis and the amount of debt maturing in the other years. We scale the two values with total corporate sales before the start of the crisis period.

Finally, we use Compustat to collect variables to better understand how exposure to minimum wage

[7] We also consider, as an additional robustness check, the Kaplan-Zingales (KZ) index (Kaplan and Zingales, 1997), a composite index of various financial constraint indexes, and a measure of financial constraints based on a textual analysis of the 10-K filings (Bodnaruk et al., 2015).

changes affects overall corporate policies and performance, such as cash holding, corporate leverage, trade credit, and capital and research and development expenditures.

Summary statistics. We use these databases to understand how exposure to minimum wage regulation affects corporate employment decisions at different levels. To do so, we merge the NETS database with minimum wage levels and information on corporate balance sheet characteristics from Compustat.[8] We also remove establishments that belong to firms that operate in the utility and financial sectors (*SIC* code equal to 49 and 60). Our final database is comprised of 2,340,503 establishment-year observations, 231,552 establishments and 5,615 firms. We analyze these data for the period spanning 1990 to 2020. We report summary statistics for this sample in Panel A of Table 1. A description of the variables is reported in Panel A of Table OA1.[9]

We use the establishment-level database to analyze whether establishments exposed to minimum wage rises change their employment level and whether corporate balance sheet characteristics explain employment dynamics. Also, we aggregate the data at the firm level to further support our findings and gain insight into how exposure to minimum wage adjustments impacts firm performance and policies. This database comprises 54,829 firm-year observations spanning the period 1990-2020. We report the summary statistics in Panel B of Table 1. A detailed description of the variables included in our sample is reported in Panel B of Table OA1.

3 Minimum wage and establishment-level employment

We evaluate how increases in the minimum wage affect establishments' employment. To do so, we acknowledge in our empirical analysis that minimum wage policies are not exogenous; the timing of minimum wage increases tends indeed to be closely tied to the health of the local economy (Neumark

[8] We use the legal business names of the establishments for cross-referencing with the Compustat database. A potential limitation of this approach is our inability to match subsidiaries with names different from those of their parent companies. However, this may be advantageous as subsidiaries often face different financial constraints than their parent companies, and many have significant operational autonomy. Additionally, we cannot merge companies that have changed their names over time. Nevertheless, our database remains substantially larger than the establishment coverage in the U.S. Census Bureau's Longitudinal Business Database (LBD), widely used in the literature (e.g., Giroud and Mueller, 2017).

[9] Additional details about the construction of our final database are found in Online Appendix OA3.

Table 1: Descriptive statistics

	(1) Count	(2) Mean	(3) SD	(4) p25	(5) p50	(6) p75
			Panel A: Establishment level			
Employment	2,340,503	51.9045	99.3581	6.0000	15.0000	46.0000
Log(Employment)	2,340,503	2.9544	1.3555	1.9459	2.7726	3.8501
Log(Sales)	2,340,491	14.6562	1.6451	13.4794	14.3502	15.8046
MW	2,340,503	6.3455	1.7173	5.1500	6.1579	7.2500
Log(Firm Size)	2,314,911	7.6621	2.1169	6.1942	7.6285	9.0865
WW Index	2,254,576	-0.3699	0.1118	-0.4472	-0.3716	-0.2941
SA index	2,314,911	-0.9798	1.4520	-2.2112	-1.3329	-0.0687
KZ Index	1,861,150	-0.5211	2.7904	-0.8213	0.1468	0.8616
Constraining Words (log)	1,692,852	5.7435	0.7433	5.3181	5.7589	6.2045
Document Length (log)	1,692,852	10.6774	0.5689	10.3631	10.6582	11.0278
Tangibility	2,311,521	0.3587	0.2088	0.1833	0.3506	0.5011
ROA	2,312,564	0.0345	0.1129	0.0134	0.0519	0.0866
ROI	2,252,552	0.0647	0.2354	0.0239	0.0838	0.1445
			Panel B: Firm level			
Employees	54,829	7.8234	19.4842	0.2750	1.3120	5.5700
Log(Employment)	54,829	1.2178	1.1803	0.2429	0.8381	1.8825
Log(Firm Size)	54,829	5.5427	2.1745	3.9859	5.5431	7.0551
SA Index	54,829	0.7236	2.0432	-0.8510	0.5255	2.0423
WW Index	53,452	-0.2604	0.1232	-0.3431	-0.2616	-0.1817
Exposure MW	54,829	6.0921	1.6927	4.8904	5.4465	7.3073
R&D	29,301	0.0942	0.1579	0.0000	0.0313	0.1196
Capital Expenditures	48,583	0.0626	0.0772	0.0175	0.0374	0.0759
Cash	48,952	0.2016	0.2874	0.0257	0.0925	0.2595
Leverage	48,828	0.7434	2.8305	0.0097	0.3539	1.0113
Leverage (Short)	48,984	0.1196	0.5050	0.0000	0.0177	0.1117
Leverage (Long)	48,860	0.6213	2.3147	0.0000	0.2219	0.8127
Trade Credit	49,009	0.2526	0.6383	0.0513	0.1323	0.3046

Notes: This table shows descriptive statistics of the variables used in our analysis. See Table OA1 for a detailed description of our variables and their sources.

et al., 2014; Allegretto et al., 2017). A consequence of this pattern is that changes in our minimum wage exposure variable could be associated with changes in other local economic characteristics that could also affect corporate employment decisions, thereby affecting our results. To address this problem, we follow the previous empirical literature (Card and Krueger, 1994; Mukherjee et al., 2017) and combine the staggered state-wide changes in minimum wage policies with a border discontinuity design.

Dynamic effects. We first estimate the dynamic effects of minimum wage changes on employment levels. For this purpose, we consider the following distributed-lag model:[10]

$$Log(Employment)_{i,t} = \sum_{w=-3}^{5} \beta_w MW_{s,t} + \eta_i + \theta_t + \epsilon_{i,t} \quad (1)$$

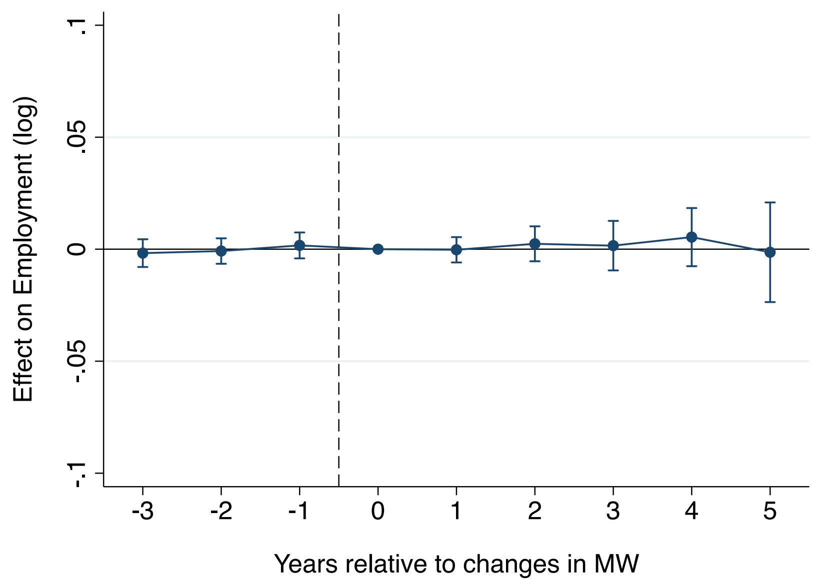
In Equation (1), *Log(Employment)* is the natural logarithm of the number of employees at establishment i at time t. MW is the effective minimum wage of state s at time t. η_i and θ_t are establishment- and year-fixed effects. Establishment-fixed effects enable us to account for time-invariant characteristics at the establishment level, such as its industry and location. These factors are key determinants of employment levels within a given establishment. Year-fixed effects capture changes in macroeconomic conditions and common shocks across the country. In this setting, β gives us the dynamic treatment effect for period w after ($w > 0$) or before ($w < 0$) the event. We focus our sample on the counties located along state borders. Finally, we cluster the standard errors at the state level, that is the level at which the minimum wage treatment takes place (Bertrand et al., 2004; Abadie et al., 2023).

Figure 3 reports estimates of Equation (1) graphically and shows the dynamic effect of minimum wage changes on employment. The plot provides several critical results. First, we take the absence of significant coefficients before the event as an indication that contiguous counties in our sample follow parallel trends in employment dynamics before minimum wage changes, a crucial assumption for the validity of our difference-in-differences estimator. Second, we do not find evidence that changes in

[10] Note that the distributed lag model is equivalent to an event study model with binned endpoints (Schmidheiny and Siegloch, 2023).

minimum wage policies affect employment in the subsequent years.[11]

Figure 3: Dynamics - Average effects



Notes: Figure 3 shows the yearly treatment effects captured by Equation (1). The outcome variable is the natural logarithm of employment in an establishment. The treatment variable is the effective minimum wage rate in the state where the establishment is located. The regression includes establishment- and year-fixed effects. The plot exhibits yearly point estimates and 95% confidence intervals based on standard errors clustered by state.

Difference-in-differences. After analyzing the dynamic effects of minimum wage changes on employment, we estimate a more efficient difference-in-differences model:

$$Log(Employment)_{i,t} = \beta MW_{s,t} + \delta_{f,t} + \eta_i + \epsilon_{i,t} \quad (2)$$

In Equation (2), *Log(Employment)* is the natural logarithm of the number of employees at establishment i at time t. MW is the effective minimum wage in state s at time t. η_i and $\delta_{f,t}$ are respectively establishment and firm times year fixed effects.

[11] Recent studies have highlighted the drawbacks of pre-event trend testing and expressed concerns regarding its limited ability to detect significant deviations from parallel trends (Roth, 2022; Kahn-Lang and Lang, 2020). To evaluate the robustness of our test, we perform a sensitivity analysis based on the methodology suggested by Rambachan and Roth (2023). Importantly, Figure OA2 demonstrates that the fixed length confidence intervals are also not statistically different from zero when allowing for violations of parallel trends that are approximately linear and for larger degrees of possible non-linearity in the parallel trends violation.

This approach allows us to estimate how minimum wage policies affect the employment decisions of similar establishments, located in different states, but belonging to the same firm, before and after a minimum wage change, while controlling for other major time-varying firm characteristics. We estimate this equation for three alternative samples: (i) all counties in the United States, (ii) all counties on the states' border, and (iii) county pairs in separate states that share a state border and whose centroids are within 75 km of each other.

We report the results in Table 2. We do not find any evidence that minimum wage policies affect average establishment employment levels. Indeed, none of the three coefficients of interest are statistically significant. These findings are consistent with several studies in the prior literature that analyze the effect of minimum wage changes in normal economic times (e.g., Cengiz et al., 2019; Dustmann et al., 2022).

While an increase in the minimum wage is directly associated with higher labor costs, which in competitive labor markets should result in reduced labor demand and employment (Stigler, 1946), it is important to recognize that the limited impact of minimum wage policies on establishment employment could be attributed to several factors. These factors include a potential reduction in employee turnover due to higher wages (Portugal and Cardoso, 2006; Brochu and Green, 2013; Dube et al., 2016, 2019), an increase in worker productivity (Coviello et al., 2022; Ku, 2022), or the strategic response of firms to offset higher labor costs by raising prices (Fougère et al., 2010; Leung, 2021) or reducing employee benefits (Clemens and Wither, 2019).

The role of corporate financial constraints. We hypothesize that corporate balance sheet characteristics affect corporate employment dynamics following minimum wage changes. In particular, financial constraints may lead firms to reduce employment levels in response to increased labor costs. This labor reduction occurs when companies cannot solely rely on internal resources to offset these additional expenses, particularly in cases where they face limitations in borrowing against future cash flows. Also, constrained firms are more likely to place a higher marginal value on preserving their liquidity compared to unconstrained firms and, for this reason, to more rapidly adopt cost-saving policies (Eisfeldt and Rampini, 2007; Caggese et al., 2019). We further recognize that, unlike physical capital, labor cannot be used as collateral, and any impediments to securing external funding can

Table 2: Difference-in-differences - Average effect

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
MW	-0.002	0.002	-0.002
	(0.002)	(0.002)	(0.003)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,340,503	707,713	625,679
Adjusted R-squared	0.931	0.932	0.932

Notes: This table shows regression estimates for Equation (2). We use *Employment (log)* as our outcome variable. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

significantly affect a firm's employment choices (Almeida et al., 2012; Benmelech et al., 2019, 2021).[12]

To investigate this hypothesis more carefully, we estimate Equation (3) shown below:

$$Log(Employment)_{i,t} = \beta_0 MW_{s,t} + \beta_1 MW_{s,t} \times Financial\ Constraints_{f,t-1} + \delta_{f,t} + \eta_i + \epsilon_{i,t} \quad (3)$$

Financial Constraints are three alternative proxies for the financial constraints experienced by firm f at time t-1 associated with a firm's establishments. As explained in the data section, we use firm size, the SA index, and the WW index as our main measures of a firm's financial constraints. In this setting, the main coefficient of interest is β_1, which captures the average effect of financial constraints on the relationship between minimum wages and employment dynamics. On the other hand, β_0 reports the effect of the minimum wage on employment when the financial constraint variable is hypothetically equal to zero.

We report model estimates for the three alternative financial constraint measures in Table 3. These results suggest that a firm's financial frictions play a crucial role in explaining employment dynamics following changes in a state's minimum wage. Indeed, the coefficient of interest β_1 is positive and statistically significant when a firm's financial constraint is measured by total assets, as smaller firms

[12] We report a simple model to conceptualize our research hypothesis in the Online Appendix OA4.

are more financially constrained. Alternatively, when we use the WW and SA indexes to capture a firm's financial constraint, the interaction coefficients are negative and statistically significant, where higher values of these two indexes are constructed to capture a tighter financial constraint.

In terms of magnitude, the effect is consistent across the three financial constraint measures. According to the results reported in the last column, a one standard deviation change in one of the three financial constraint variables representing tighter financial constraints decreases establishment employment by 0.2 % concerning the average value of this variable following a $1 increase in the minimum wage.

The marginal effects of minimum wage policies across the distribution of financial constraints. To better understand the relationship between minimum wage, employment, and financial constraints, we plot in Figure OA3 the marginal effects of minimum wage rate changes on employment for different values of the financial constraints.

These plots reveal several important insights. In line with our hypothesis, the impact of the minimum wage on employment varies significantly depending on the level of the financial constraints faced by firms. For firms with median levels of financial constraints, indicated by vertical dashed lines, the effect of the minimum wage on employment is very close to zero. This aligns with our initial findings of no effect in our baseline models. However, above-median financially constrained firms show statistically significant negative employment effects in response to minimum wage increases. The estimated effects are more precise the greater the degree of financial constraints. Conversely, below-median financially constrained firms tend to show slightly positive employment effects, although these effects have wider confidence intervals and, in most of our specifications, are not statistically significant.

While employment in financially constrained firms is significantly negatively affected, these findings suggest that unconstrained firms may be better positioned to absorb labor cost increases and benefit from the positive aspects of higher minimum wages, such as reduced turnover, lower training costs, and a larger pool of workers available to work at higher salaries (Portugal and Cardoso, 2006; Brochu and Green, 2013; Dube et al., 2016, 2019; Coviello et al., 2022; Ku, 2022). Overall, the negative effects on constrained firms and the positive, insignificant effects on unconstrained firms seem to balance out,

Table 3: The role of corporate financial constraints

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.015***	-0.023***	-0.031***
	(0.005)	(0.008)	(0.010)
MW × Log(Firm Size)	0.002**	0.003***	0.003**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933
		Panel B: Interaction with the WW index	
MW	-0.017***	-0.020***	-0.030***
	(0.006)	(0.008)	(0.009)
MW × WW Index	-0.036**	-0.053***	-0.066***
	(0.016)	(0.018)	(0.023)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,186,212	661,777	584,829
Adjusted R-squared	0.932	0.934	0.934
		Panel C: Interaction with the SA index	
MW	-0.005***	-0.006**	-0.010***
	(0.001)	(0.003)	(0.003)
MW × SA Index	-0.002	-0.005***	-0.005**
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, clustered at the state level, are reported in parentheses below the coefficient estimates. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

resulting in a negative, but statistically insignificant overall labor market effect, or a near-zero average effect across all establishments.

Heterogeneous effects across sectors. Corporations face heterogeneous exposure to minimum wage policies due to their varying proportions of minimum wage workers. However, information on the number of minimum wage employees is unavailable in our databases at the establishment and firm levels. To deal with this issue, some existing studies focus their empirical analysis on corporations operating in specific industries that employ larger fractions of minimum wage workers and are therefore deemed to be particularly exposed to minimum wage hikes (e.g., Card and Krueger, 1994; Agarwal et al., 2024).

In our study, we take into account that minimum wage policies can have an impact in other industries as well (Neumark et al., 2004; Meer and West, 2016), and we estimate the average effect of minimum wage changes on overall employment while accounting for sector-level differences using establishment and firm times year fixed effects. This approach is particularly attractive considering the importance policymakers place on understanding overall employment responses to minimum wage policies (Cengiz et al., 2019).

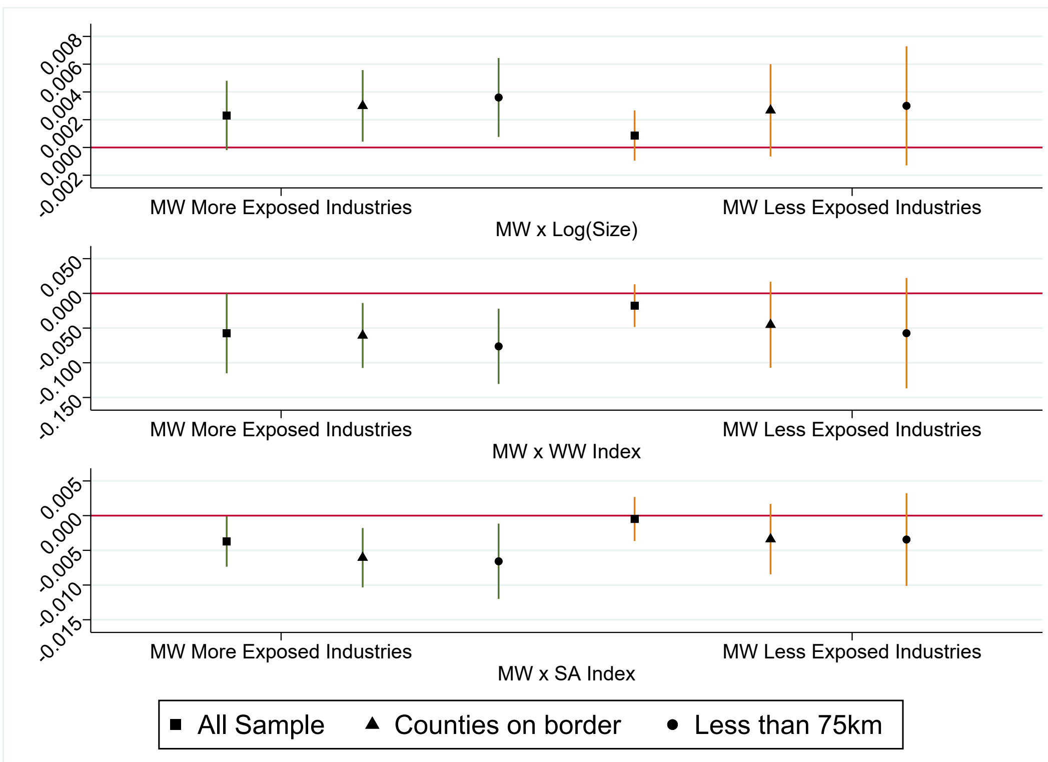
We investigate this point in greater depth by examining the impact of minimum wage changes and firm financial constraints on employment in different sectors. More specifically, we separately analyze establishments of firms that operate in more minimum wage-exposed industries and those that do not. For this purpose, we define minimum wage exposed industries following the work of Gustafson and Kotter (2023). This definition includes restaurants, retail stores, and the entertainment industry, which employ more than 70% of all minimum wage labor (i.e., Fama-French 49 industry categories 7, 43, and 44).

We plot in Figure 4 the interaction coefficients between the minimum wage rate and the corporate financial constraint variable as reported in Equation (3) for alternative measures of firm financial constraints and the alternative industry sub-samples.

We find economically and statistically significant coefficients when we look at the sample of establishments that operate in the industries most exposed to minimum wage changes. When we look at the results for the sample of less exposed industries, we continue to find consistent coefficients with our baseline results. However, their precision is comparatively lower, rendering them statistically insignificant at conventional levels.[13]

These results bolster the hypothesis that our findings are not attributable to other confounding time-varying changes in local economic conditions. To attribute our findings to this alternative explanation would require the variations in economic conditions among states following a minimum wage rise to exclusively affect firms more exposed to changes in minimum wage rates (Gustafson and Kotter, 2023).

Figure 4: Heterogeneity across sectors



Notes: Figure 4 shows the interaction coefficients between the minimum wage rate and the corporate financial constraint indexes, as reported in Equation (3), for alternative specifications and various subsamples. The green coefficients refer to estimates obtained using the sample of industries heavily exposed to minimum wage policies. The yellow coefficients refer to estimates obtained using the sample of industries less exposed to minimum wage policies. We define minimum wage-sensitive industries following the work of Gustafson and Kotter (2023). This definition includes restaurants, retail stores, and the entertainment industry, which employ more than 70% of all minimum wage labor (i.e., Fama-French 49 industry categories 7, 43, and 44). We also show the coefficients when we further restrict the sample to (i) all counties in the United States (indicated with the square black marker), (ii) all counties on the state borders (indicated with the triangle black marker), and (iii) all counties on state borders whose centroids are less than 75 km apart (indicated with the circle black marker).

[13] We report more detailed estimation results for these alternative samples in Tables OA2 and OA3.

4 Robustness analysis and additional results

In this section, we establish the robustness of our primary findings by employing various alternative estimation methods, measures of firm financial constraints, and sub-sample analysis. We also offer further insights into the effects of minimum wages on the cost of labor and the number of establishments (extensive margin) and their performance, supplemented by comprehensive county employment dynamics that reinforce our earlier evidence regarding the role of firm financial constraints. Furthermore, we investigate the influence of local banking conditions within this framework. All tables in this section are found in the Online Appendix to conserve space.

4.1 Robustness analysis

Alternative difference-in-differences estimators. Recent research in econometrics has found that even when treatment is assigned randomly, the accuracy of difference-in-differences regression estimates can be compromised. In fact, using a standard two-way fixed effects (TWFE) model to estimate staggered treatment effects can often result in biased estimators. It occurs when earlier-treated units are mistakenly used as a comparison group for later-treated units (Baker et al., 2022; Goodman-Bacon, 2021).

To examine the potential bias of the staggered difference-in-differences approach in our setting, we estimate the effects of minimum wage policies on establishment employment levels using alternative estimation methods explicitly designed to address this concern. More specifically, we focus our analysis on adjacent counties in separate states located along state borders and report in Figure OA4 annual point estimates of the effects of minimum wage changes on establishment employment levels based on the recommended methodologies in Sun and Abraham (2021) and De Chaisemartin and d'Haultfoeuille (2024). We also compare these estimators to the results obtained from the earlier TWFE model.

Figure OA4 highlights the important results we uncover. First, it shows that the parallel trend assumption also holds when we use these alternative methods. Second, in line with our baseline results, we do not find evidence that changes in minimum wage policies substantially affect establishment employment levels. Finally, it shows that the effects are consistent across alternative estimators.

Alternative measures of financial constraints. We also investigate whether our results hold when considering alternative measures of corporate financial constraints.

We start by considering the first index of financial constraints frequently used in the literature, namely the Kaplan-Zingales (KZ) index (Kaplan and Zingales, 1997). This index has been demonstrated to be not correlated with the other financial constraint indexes (Farre-Mensa and Ljungqvist, 2016), and it has been observed to under-perform them (Whited and Wu, 2006; Hadlock and Pierce, 2010).

We estimate Equation (3) using the alternative KZ measure and report the results in Panel A of Table OA4. Not surprisingly, the triple interaction coefficients are close to zero and are not statistically significant in the alternative proposed specifications. To further assess the robustness of our findings, we build a composite financial constraint index by adopting a methodology similar to that presented by Bartram et al. (2022). To do so, we consider a method that relies on the rankings of firms based on the KZ, SA, and WW indices, as well as firm size. Specifically, we consider whether the firm has above-median annual values for the three alternative indices and below-median annual values for firm size. If most of these indicators suggest that the firm is financially constrained, we assign a value of one to the composite indicator; otherwise, we assign a zero value.

We present the results in Panel B of Table OA4; according to the estimates reported in the last column, firms categorized as financially constrained on average decrease their employment by 1 percentage point after a one-dollar increase in the minimum wage. Our results continue to hold when we consider a composite index based on the first principal component of these four alternative indexes of corporate financial frictions. We report the results in Panel C of Table OA4. According to the results reported in the last column, one standard deviation increase in this financial constraint index decreases employment by 0.8 percentage points after a one-dollar increase in the minimum wage.

Finally, we demonstrate the robustness of our results when considering an alternative financial constraint measure based on textual analysis of firm annual 10-K filings. To do so, we use the dictionary of 'constraining' words proposed in Bodnaruk et al. (2015). They show that the frequency of these words exhibits a very low correlation with traditional measures of financial constraints, yet it predicts subsequent liquidity events better than widely used financial constraint indexes.

We take the natural logarithm of the word frequency of this dictionary and then control for differences in the documents' length.[14] Subsequently, we estimate Equation (3) and present the results in panel D of Table OA4. In line with our baseline results, we find that, after a minimum wage rise, establishments belonging to financially constrained firms reduce their number of employees. More specifically, according to the results reported in the last column, after a one-dollar rise in the minimum wage rate, a 1% change in the textual analysis financial constraint index leads to a 0.02% decrease in employment.

Measurement errors in the NETS database. A limitation of the NETS database is the high imputation rate for employment information, particularly severe in small establishments. According to Barnatchez et al. (2017), employment information for establishments with fewer than five employees is imputed for approximately 45% of these cases. For the rest of the sample, the imputation rate is much lower, around 15%.

To test the reliability of our results, we conduct a robustness check by removing establishments with fewer than five employees. Additionally, we perform another test by excluding establishments with round numbers of employees (5, 10, 100, 200, etc.), as these are more likely to represent imputed data. The results remain consistent and are reported in Tables OA5 and OA6.

Alternative fixed effects specification. In our main specification, we use establishment and firm times year fixed effects. This specification is particularly attractive because it allows us to compare establishments that belong to the same corporations, but are heterogeneously affected by minimum wage law changes because they are located in different states. Also, it allows us to control for time-varying firm characteristics.

In this section, we show that our results hold under the following alternative specifications: using (i) establishment and year fixed effects, (ii) state and year fixed effects, and (iii) county and year fixed effects. We report the three estimation results in Tables OA7, OA8, and OA9 respectively. In terms of magnitude, the financial friction coefficients are consistently higher than our baseline results, suggesting that our main specification, which includes firm times year fixed effects, captures most of

[14] This information is available starting from the year 1993 and has been made available by W. McDonald on this website: https://sraf.nd.edu/sec-edgar-data/lm$_$10x$_$summaries/.

the endogeneity associated with the financial constraint variables. Also, it highlights the importance of firm characteristics in shaping employment patterns following minimum wage law changes.

Matching of bordering counties. We follow Dube et al. (2010) and present an alternative identification strategy. Instead of adopting a more conventional approach, we opt for a method where each county in the border region is carefully matched with all the other possible county pairs in other states. This strategy necessitates a change in samples, as establishments may be represented multiple times for each county pair they belong to. Furthermore, to ensure a meaningful comparison of contiguous counties in terms of their employment trends, we incorporate control variables for pair- or pair-by-period fixed effects. By introducing these fixed effects, we expect to capture the shared economic trends within the local labor market. Subsequently, we proceed to estimate Equation (3). The results of our analysis are presented in Table OA10. Notably, all findings in the table exhibit signs and significance levels consistent with our baseline results.

First difference model. An alternative approach to investigating the relationship between minimum wage and employment involves first differencing the prior model, resulting in the specification reported in the Equation (4) below:

$$\Delta Log(Emp.)_{i,t} = \beta_0 \Delta MW_{s,t} + \beta_1 \Delta MW_{s,t} \times Financial\ Constraints_{f,t-1} + \delta_f + \eta_t + \Delta\epsilon_{i,t} \quad (4)$$

$\Delta Log(Employment)_{i,t}$ is the first difference of the natural logarithm of employment in establishment i at times t and $\Delta MW_{s,t}$ is the yearly changes in the minimum wage. We fully interact this treatment variable with our three alternative measures of corporate financial constraints. We report the results in Table OA11, and observe that they are consistent with our baseline specification.

Clustering the standard errors along alternative dimensions. Our main specification employs standard errors clustered at the state level since this is where the treatment is assigned (Abadie et al., 2023; Bertrand et al., 2004). In this section, we show that our results hold when we cluster standard errors at the establishment level, at the firm level, and also at the county level. We report these three estimation results in Tables OA12, OA13, and OA14. All the coefficients of interest maintain the same level of statistical significance.

Other firm characteristics. One potential concern with our analysis is that financially constrained firms could exhibit a more pronounced response to minimum wage changes, not solely due to their differing financial constraints, but also due to the other firm attributes correlated with their financial constraints.

To mitigate this concern, we incorporate firm times year fixed effects into our main specification. This revised specification enables us to account for any time-varying firm attributes and compare establishments belonging to the same firm, but with varying levels of exposure to state minimum wage policies. We also address this concern in the paper's final section, where we employ an exogenous measure of financially constrained firms based on variation in firms' long-term debt structure around the 2007-2008 global financial crisis.

We demonstrate in this section that our findings remain robust even when we fully interact changes in minimum wages with other time-varying firm attributes. Specifically, we present evidence that our primary results hold when incorporating alternative measures of firm performance, efficiency, and asset tangibility in our regression analysis. We report our results in Table OA15. Again, these findings are consistent with our baseline results.

Geographic characteristics. Another possible concern with our analysis is that local economic characteristics could be positively related to changes in the minimum wage laws, thereby biasing our results. We follow the existing literature to address this issue and employ a border discontinuity design approach.

As an additional test, we demonstrate that our results remain robust even when we explicitly control for several local economic characteristics. More specifically, we separately control for the log of county population, county income per capita, and the county unemployment rate. We report these results in Table OA16.

Considering other types of labor protection. To further assess the robustness of our findings, we examine whether our main results hold when controlling for another significant labor protection policy, unemployment insurance (UI). To do so, we gather data on the UI benefit schedules of each state from the U.S. Department of Labor's 'Significant Provisions of State UI Laws". More specifically, we follow Guo et al. (2024) and consider the overall UI benefit level in a given state and year by multiplying the maximum weekly benefit amount by the maximum duration of benefits provided under each state's regular UI program.[15] Additionally, we collect information on the rates of union membership (UMR) by state from the Current Population Survey (CPS).[16]

We include controls for UI and UMR in Equation (3). Also, as with minimum wage levels, we introduce an interaction term between these variables and the alternative measures of firm financial constraints. The outcomes are reported in Table OA17, and this evidence shows that our primary findings are robust to controlling for state UI and unionization rates.

Weighting. We demonstrate the robustness of our findings by incorporating weights based on establishment size into our regressions. Specifically, we use establishment sales, represented by the natural logarithm, as a proxy for establishment size. Including weights enables us to identify average partial effects even in the presence of unmodeled heterogeneity in effects. Furthermore, it allows for a better understanding of the aggregate effect on employment (Solon et al., 2015). The results of our weighted regressions are presented in Table OA18. Notably, these findings remain consistent with the baseline results, and the newly reported coefficients are within one standard deviation change of the original results.

4.2 Additional results

Minimum wage and the cost of labor. Understanding whether and to what extent minimum wage policies impact wages is crucial for our analysis. Regrettably, this information is not available in our establishment-level database. To address this gap, we gather wage data from the Current Business Patterns (CBP). This database offers total salary information at the county, year, and industry levels.[17]

[15] To validate this measure, Agrawal and Matsa (2013) and Hsu et al. (2018) show that changes in this variable are correlated with higher aggregate state UI expenditures.

[16] For details, see https://www.unionstats.com/ (this reference was last accessed on September 18, 2024).

[17] After 1998, NAICS codes are used to report data at the industry level. Before 1998, industry information was reported using SIC codes. Therefore, we convert the four-digit SIC code to a six-digit NAICS code using the 1997 bridge file.

With this data, we estimate the following Equation:

$$Wage_{j,k,t} = \alpha_j + \theta_{k,t} + \beta MW_{s,t} + \epsilon_{j,k,t} \tag{5}$$

The dependent variable of interest is the total payroll divided by the number of employees in the county. We also control for county (α_j) and industry-year fixed effects ($\theta_{k,t}$). We report the results for the alternative samples in Table OA19.

Our findings reveal that, on average, minimum wage increases lead to a rise in the cost of labor per employee in a county and year. Specifically, we observe an increase of about $900 per employee per year. This result aligns with prior research indicating a positive impact of minimum wage policies on average wages. More specifically, minimum wage policies affect not only minimum wage workers but also have spillover effects on other relatively low-wage incumbent workers and new hires (e.g., Gopalan et al., 2021, 2024).

Extensive margin. A crucial characteristic of our establishment-level database is that it is not affected by survivorship bias. In this section, we report estimates of the effect of changes in minimum wage policies and financial constraints on the extensive margin. Specifically, we analyze whether there is an effect on the number of establishments located in areas exposed to a minimum wage rate increase.

To investigate this question, we estimate the following Equation (6):

$$Log(Establishments)_{f,c,t} = \beta_0 MW_{st} + \beta_1 MW_{st} \times Financial\ Frictions_{f,t-1} + \delta_{f,t} + \eta_c + \epsilon_{i,t} \tag{6}$$

Establishments are the number of establishments that belong to firm f in county c at time t. Since our analysis is not at the establishment level, we can not include establishment fixed effects. However, we do employ county fixed effects (η_c).

We report the results in Table OA20 for our three alternative samples (all the counties in the United States, all adjacent counties on opposite sides of the state's border, and all adjacent counties on the opposite sides of a state's border whose centroids are less than 75 km apart). Higher minimum wage policies decrease the number of establishments in exposed areas. In our preferred specification, all coefficients reported in Column (3) demonstrate negative and statistically significant associations, except for the coefficient linked to the SA index. Nonetheless, it's noteworthy that this coefficient remains negative. In terms of magnitude, based on the estimates reported in the last column, we find that a one standard deviation change in the financial constraint index reduces the number of a firm's establishments by 0.8 % relative to the average number of establishments at the beginning of our sample period.

Establishments' performance. We consider that an increase in the minimum wage may positively impact local demand if minimum wage workers exhibit a higher propensity to consume compared to the owners of businesses. If this holds, increasing the minimum wage could potentially affect the performance of establishments in the states where the change is implemented.

Previous research has empirically analyzed this question in various contexts. For instance, when examining a substantial and consistent increase in Hungary's minimum wage, Harasztosi and Lindner (2019) document a positive effect on the total revenue of firms affected by the minimum wage hike. However, their findings also reveal that this effect was driven by increased prices, concurrently highlighting a reduction in output and profitability. By studying the changes brought about by introducing a national minimum wage in 1999 in the United Kingdom, Draca et al. (2011) observed a direct translation of rising labor costs due to minimum wages into reductions in profits. Studying staggered adjustments in minimum wages across individual states and using census data from over 29,000 hotel properties, Agarwal et al. (2024) find that doubling the minimum wage leads to an annual average reduction of 6% in hotel revenues.

We investigate this question in our setting using as our outcome variable of interest the natural logarithm of establishments' total sales in Equations (2) and (3). We next report the estimates in Tables OA21 and OA22, respectively.

We find no average effect of minimum wages on sales, as reported in Table OA21. However, the results in Table OA22 show evidence of a negative effect on financially constrained firms. More specifically, a one standard deviation change in the three financial friction measures decreases establishment sales by 0.4 % of the average value of this variable after a $1 increase in the minimum wage. This result suggests that an employee reduction is not optimal for financially constrained corporations and leads to a reduction in production output.

The role of corporations' market power. In a similar vein, we explore the hypothesis that corporations might adjust their product prices in response to minimum wage policies (Fougère et al., 2010; Leung, 2021). Assuming an inelastic demand, a potential price increase could positively impact corporations' cash flows, potentially reducing their reliance on external financial resources.

Due to the absence of product prices in our database, we alternatively investigate whether there is a differential impact on corporations that possess a greater ability to transfer additional labor costs to consumers. To test this condition, we assess market power within an industry using the Lerner index.[18] Subsequently, we estimate Equation (3) for a sample of firms with relatively greater market power, which we define as establishments whose Lerner index value is above the median in our sample.

The results of this analysis are presented in Table OA23. The estimated coefficients are consistent with our baseline findings and do not support the alternative hypothesis that establishments in industries where it is easier to pass on cost increases to consumers can better deal with the adverse effects of financial constraints on employment following a minimum wage increase.

The role of local banking markets. The prior literature has underscored the influence of local banking markets on employment (Ziebarth, 2013; Boustanifar, 2014; Benmelech et al., 2019). This leads us to investigate the potential significance of local banking market conditions in our context. Specifically, we anticipate that financially constrained establishments in communities where firms find it easier to secure additional credit will be less inclined to reduce the number of employees following a minimum wage increase.

To assess the role of local banking markets, we introduce an exogenous shock to local banking conditions. Specifically, we use the banking deregulation wave following the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994. This banking deregulation statute enabled states to gradually eliminate interstate bank branching restrictions, thereby augmenting local banking market competition, efficiency, and ultimately local credit supply (e.g., Favara and Imbs, 2015; Krishnan et al., 2015; Berger et al., 2021).

To gauge the intensity of deregulation on the local banking market, we adopt the index proposed by

[18] The Lerner index is computed using Compustat data, measuring the average ratio of operating profits to sales (Peress, 2010). Operating profits are calculated by subtracting the cost of goods sold and general administrative expenses from sales revenue.

Rice and Strahan (2010). This index ranges from zero to four, with four denoting the most deregulated states. Consistent with Favara and Imbs (2015) and Rice and Strahan (2010), we presume that all states had full interstate banking restrictions before deregulation events. Further details about the timing of banking deregulation across states, the regulations adopted, and the associated index scores used in our analysis are presented in Table OA24.

We limit our analysis to the sub-sample period 1994-2005 (the deregulation period). We next introduce and fully interact the deregulation index with the minimum wage level and the corporate financial constraint index in Equation (3). The results are documented in Table OA25.

Consistent with our hypothesis, we observe that the triple interaction coefficient is positive and statistically significant when using the SA and WW indices and negative and statistically significant when considering a firm's total asset size as the financial constraint measure. Thus, we conclude that an expansion of credit supply alleviates the adverse effect of corporate financial frictions on employment after minimum wage increases.

This finding is particularly significant as it offers additional evidence of the importance of firms' financial constraints underpinning our findings. Moreover, it underscores the role of local banking market conditions in this context. Consistent with Benmelech et al. (2019), our findings suggest that local banking market conditions can alleviate the impact of corporate financial constraints on employment. In terms of magnitude, a unit increase in the deregulation index mitigates almost half of the employment decline observed in financially constrained establishments.

County-level analysis. In a general equilibrium context, employment can potentially shift from establishments encountering significant financial constraints to establishments affiliated with corporations with strong balance sheets. The magnitude of this labor reallocation hinges on various factors, including local labor market conditions, the capacity of non-financially constrained firms to offer higher product prices, and the substitutability of goods among these establishments (Chodorow-Reich, 2014; Gilchrist et al., 2017; Mian and Sufi, 2014).

To account for possible general equilibrium effects in our empirical analysis, we adopt the methodology outlined by Dube et al. (2010), which employs county-level regressions, as reported in Equation (7).

$$Log(County\ Emp.)_{j,t} = \beta_0 MW_{s,t} + \beta_2 Constraints_{j,t-1} + \beta_1 MW_{s,t} \times Constraints_{j,t-1} + \delta_j + \eta_c + \epsilon_{i,t} \tag{7}$$

More specifically, we measure county-level (j) employment as the total employment of all the establishments within a county in our sample. The county-level financial constraint measures are the employment-weighted average values derived from the three alternative measures of financial constraints across all establishments in our sample within each county.

The findings in Table OA26 echo our baseline results, revealing that counties with greater exposure to establishments with a high level of financial constraints face greater aggregate employment declines after a rise in the minimum wage. From the results reported in the last column of this table, we find that a one standard deviation rise in one of the alternative financial constraint indexes reduces total employment by approximately 1% of the average employment outcome following a one-unit minimum wage hike. Interestingly, the coefficients that capture the effect of financial constraints on the local economy when the minimum wage is hypothetically close to zero are positive and statistically significant. This result suggests that in the absence of minimum wage pressures, constrained firms might rely more heavily on labor, which is cheaper, rather than on more costly capital investments.

5 A firm-level analysis

In this section, we explore how corporate exposure to shifts in the minimum wage influences overall employment decisions, financial performance, and corporate policies. This investigation allows us to gain a deeper understanding of how minimum wage policies impact the employment choices of financially constrained firms. Also, our original approach, which uses establishment-level information to track the minimum wage effect on aggregate corporate performance, is likely to be more precise compared to solely examining corporate headquarters, as has been done in previous studies (e.g., Gustafson and Kotter, 2023).

Corporate employment. It might be supposed that firms would find it optimal to shift employment from establishments exposed to rising minimum wage levels to non-exposed establishments. This adjustment could also imply that a firm's aggregate employment changes little, if at all. However, the optimal employment level of each establishment is likely independent of employment levels in other establishments, as each has its unique characteristics and optimal equilibrium. In our empirical analysis, we account for these establishment differences by using establishment-fixed effects. In addition, transferring employees between establishments faces several frictions that could more than offset any wage saving. These frictions include training costs, skill mismatches, and geographic labor constraints.

To empirically investigate whether redistribution of employees across establishments of the same firm plays any particular role, we turn to firm-level regressions. For this purpose, we measure corporate dollar exposure to changes in minimum wage policies for firm f at time t using a weighted average approach as reported in the following Equation:

$$Exp.\ MW_{f,t} = \sum_{n=1}^{51} Share\ Employees_{c,s,t} \times MW_{s,t} \tag{8}$$

Share Employees is the share of employees of company f in state s at time t and MW is the effective minimum wage in state s at time t.

We next estimate the following Equation:

$$\begin{aligned} Log(Emp.)_{f,t} = \beta_0 Exp.\ MW_{f,t} + \beta_1 Exp.\ MW_{f,t} \times Financial\ Constraints_{f,t-1} + \\ \beta_3 Financial\ Constraints_{f,t-1} + \eta_{s,t} + \theta_f + \epsilon_{i,t} \end{aligned} \tag{9}$$

$\eta_{s,t}$ and θ_f are respectively sector-year and firm fixed effects. One limitation of this specification is that we are unable to control for firm-year fixed effects, which are co-linear with our other dependent variables of interest. Additionally, we cannot use a border discontinuity approach since our treatment variable has been constructed by considering the entire distribution of establishments across U.S. states rather than focusing solely on corporate headquarters.

We report the model estimates in Table 4. These results are consistent with our previous findings at the establishment level. We continue to find that financially constrained firms decrease their employment after increases in the minimum wage. In terms of magnitude, this effect is even greater than the results that we documented earlier at the establishment level. More specifically, we find that a one standard deviation increase in financial frictions decreases overall corporate employment by 2.2 % of the average value of the outcome variable after a $1 increase in the minimum wage exposure variable.[19]

Table 4: Firm level analysis - Employment

	(1) Employment (log)	(2) Employment (log)	(3) Employment (log)
MW Exposure	-0.043***	-0.017	0.015
	(0.012)	(0.012)	(0.010)
Log(Firm Size)	0.251***		
	(0.019)		
MW Exposure × Log(Firm Size)	0.008***		
	(0.002)		
WW Index		-0.965***	
		(0.301)	
MW Exposure × WW Index		-0.151***	
		(0.037)	
SA Index			-0.121***
			(0.017)
MW Exposure × SA Index			-0.018***
			(0.002)
Firm FE	✓	✓	✓
Year × Industry FE	✓	✓	✓
Observations	54,829	50,158	54,829
Adjusted R-squared	0.955	0.941	0.945

Notes: This table shows regression estimates for Equation (9). We use *Employment (log)* as our outcome variable. All the regressions include firm and year × industry fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their data sources.

Corporate policies and performance. We use this setting to investigate how exposure to minimum wage policies affects corporate financial policies and performance. According to our theoretical

[19] One issue with employment information reported in Compustat is its tendency to aggregate the total number of employees at the global level. To examine whether this impacts our findings, we exclude multinational firms from the sample and re-estimate Equation (9). Specifically, a firm is classified as multinational if it reports non-zero foreign income in the previous three years. Alternatively, we define a firm as multinational if at least 5% of its sales were generated from outside its home country, based on the Compustat Geographic Segment database (Nimier-David et al., 2023). We find consistent results in Tables OA27 and OA28.

framework, we expect that minimum wage policies exert pressure on firms' internal resources. This pressure can force firms to reduce additional expenditures and seek external resources to address their short-term liquidity needs and other financial requirements.

To investigate whether these dynamics hold, we estimate the following Equation:

$$Outcome_{f,t} = \beta Exposure\ MW_{f,t} + \eta_{s,t} + \theta_f + \epsilon_{i,t} \tag{10}$$

$\eta_{s,t}$ and θ_f are respectively sector-year and firm fixed effects. *Outcome* alternatively represents one of the following variables: Cash, Leverage, Leverage (Short Term), Leverage (Long Term), Trade Credit, R&D, and Capital Expenditures. These variables are defined in detail in Table OA1.

We report the results in Table 5. In the first column, we examine how corporate exposure to minimum wage policies affects cash holdings (Cash). We find that a one-dollar increase in the minimum wage variable leads to a decrease in cash holdings of 0.8 basis points. This corresponds to a 4% decrease relative to the average cash holding level. Therefore, the effect is also economically significant.

One plausible explanation for the dynamics of this outcome variable is that minimum wage policies cause firms to experience short-term liquidity needs. Because exposure to minimum wage policies impacts the availability of a firm's internal resources, companies subject to these policies may seek to acquire additional external resources to address their short-term liquidity needs and other financial requirements. This, in turn, can account for the increase in corporate leverage reported in Column (2) (Custódio et al., 2013). Indeed, a firm's leverage coefficient is statistically significant, with a one-unit increase in the minimum wage measure associated with an 11 basis point increase in its debt-to-equity ratio.

When we analyze current and long-term debt as separate outcome variables in Column (3) and Column (4), we observe that both coefficients are positive and statistically significant. Specifically, we document an increase of 1 basis point in current debt and 9 basis points in long-term debt after a one-dollar increase in exposure to the minimum wage variable.

We then examine in Column (5) trade credit as another source of financing that can address short-term liquidity needs. This source of financing is particularly attractive for constrained firms (Biais and

Gollier, 1997; Giannetti et al., 2011), even if it typically comes with relatively high implicit interest rates (Petersen and Rajan, 1995, 1997; Cunat, 2007). In line with the previous results, trade credit increases by 2.7 basis points. In summary, our findings suggest that changes in minimum wage policies expose firms to additional resource needs to meet their short-term liquidity requirements, potentially leading financially constrained firms to reduce employment.

In Columns (6) and (7), we investigate the impact of exposure to minimum wage policies on corporate investment decisions. We find that a one-unit increase in the minimum wage exposure measure is associated with a half-basis point decrease in R&D expenditures, corresponding to an average 5.5% decline in R&D expenditures. Additionally, we also observe that exposure to minimum wage policies reduces capital expenditures. However, this coefficient is not statistically significant at conventional levels.

These results suggest that firms exposed to increases in the minimum wage also reduce their expenditures to address short-term liquidity needs (Gustafson and Kotter, 2023). Furthermore, they suggest another mechanism through which financial frictions can impact employment, namely, a decrease in firm investments. In fact, given the interdependence between labor and capital, adjustments in employment levels can be triggered as a response to a reduction in capital investment (Benmelech et al., 2021).

Finally, in the last column, we also consider how exposure to minimum wage policies affects firm performance, using return to assets as the outcome variable of interest. Consistent with our establishment-level analysis, we do not find any evidence that exposure to minimum wage policies affects corporate performance.

6 The financial crisis, the federal minimum wage, and corporate employment

We present empirical evidence suggesting that increases in the minimum wage have no average effect on employment levels. However, we also uncovered significant variations in firm responses to such changes, particularly among firms facing financing challenges. These findings provide valuable insights into the

Table 5: Firm level analysis - Corporate policies and operating performance

	(1) Cash	(2) Leverage	(3) Leverage (Short)	(4) Leverage (Long)	(5) Trade Credit	(6) R&D	(7) Capital Exp.	(8) ROA
MW Exposure	-0.008**	0.111***	0.012*	0.093***	0.027***	-0.004**	-0.001	0.001
	(0.004)	(0.035)	(0.007)	(0.033)	(0.008)	(0.002)	(0.001)	(0.004)
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year × Industry FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	48,422	48,297	48,456	48,329	48,481	28,616	48,055	54,696
Adjusted R-squared	0.532	0.186	0.239	0.207	0.284	0.719	0.543	0.554

Notes: This table shows regression results for Equation (10). We use alternative outcome variables as reported in the second row. All the regressions include firm and year × industry fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their data sources.

dynamics of minimum wage policies, but they do not necessarily establish causal relationships. This is because financial constraints can be associated with other corporate characteristics that also influence employment decisions.

In this section, we focus on a unique experiment that allows us to understand whether financial constraints *causally* affect establishment employment. For this purpose, we exploit two sources of exogenous variation in our sample period. One comes from an increase in the federal minimum wage during the global financial crisis, which heterogeneously affected establishments across individual states depending on whether state minimum wage rates are below the new federal minimum wage requirement. This crisis period is particularly relevant for our analysis since it is characterized by severe information asymmetries, uncertainty, and tight credit market conditions (Brunnermeier, 2009; Bernanke, 2023). The second source of exogenous variation comes from individual corporate debt structures and the ex-ante variation in a firm's long-term debt maturing in the crisis period. Indeed, the financial crisis hit the US economy unexpectedly, which meant that managers could not preemptively adjust their corporate debt structure to reduce their rollover risk during the financial crisis period.

For this exercise, we limit our sample period to center around the 2007-2008 financial crisis period, covering the 2003-2011 period. We report the summary statistics for this sample period in Table OA29.

The US federal minimum wage change implemented during the global financial crisis. Before the financial crisis, the federal minimum wage was set at \$5.15 per hour, a rate established in 1997. This rate remained unchanged until 2007. Figure 5(a) illustrates the evolution of the federal minimum wage over the years of our sample period.

At the time of the federal minimum wage change, occurring at the onset of the financial crisis, some states had minimum wage rates higher than the new federal minimum wage. As a result, the effective minimum wage in these states remained unaffected by the federal minimum wage increase. Figure 5(b) illustrates the states impacted by this federal minimum wage rise.

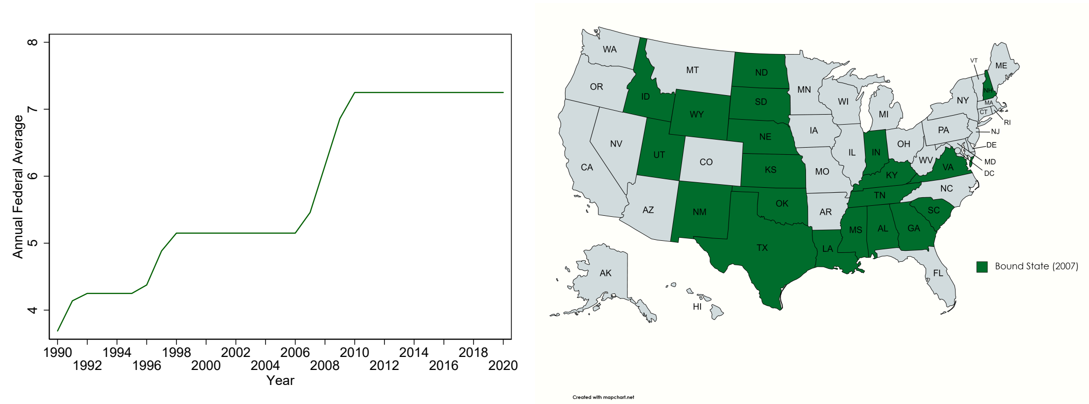
The rationale behind our identification strategy is straightforward. After the minimum wage increase, firms may want to adjust their establishment employment levels due to the relative changes in local labor costs. To identify the impact of the federal minimum wage increase, we compare similar establishments in states affected by the wage increase before and after its implementation to establishments in states unaffected by the federal regulatory change (Clemens and Wither, 2019; Gustafson and Kotter, 2023).

As we see in Figure OA5(a), following the onset of the financial crisis, establishments in affected states experience greater growth in their *effective* minimum wage rates compared to those in unaffected states, consistent with the regulatory shock having a tangible effect. More specifically, in the month before the federal minimum wage change, the average effective minimum wage rate was \$5.16 in the affected states versus \$6.81 in the unaffected states (a difference of \$1.65). At the end of the financial crisis, the effective minimum wage in affected states was \$6.82 compared to \$7.37 in unaffected states (a difference of \$0.55). Importantly, this convergence of more than \$1 was not due to any minimum wage change initiated by individual states, but is simply the result of the change in the federal minimum wage. We plot these divergent patterns in Figure OA5(b), where we demean each time series using the pre-crisis average effective minimum wage rates for the treatment and control groups.

Employment dynamics after the federal minimum wage rise. To determine whether employment levels of establishments in affected states decrease, we estimate the following Equation:

$$Log(Employment)_{i,t} = \sum_{t=2003,t\neq 2006}^{t=2011} \beta_t Treated_s \times Year_t + \eta_i + \theta_t + \epsilon_{i,t} \tag{11}$$

Figure 5: The federal minimum wage change



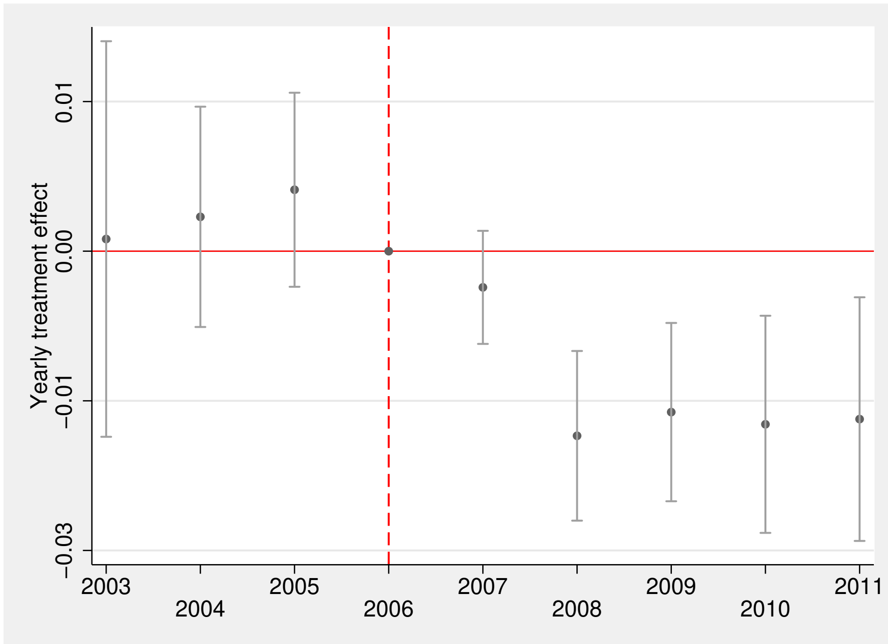
(a) Changes in the federal MW (b) Treated vs untreated states

Notes: Figure 5(a) shows changes in the federal minimum wage over our sample period. Figure 5(b) illustrates the states that fall into the categories of affected (treated) and unaffected (untreated) in our empirical setting.

In Equation (11), *Treated* is an indicator variable that equals to one if the establishment is located within a state treated by the rise in the federal minimum wage in 2007 and is zero otherwise. η_i and θ_t are respectively establishment and year fixed effects. In this model, we interact *Treated* with a complete set of year-fixed effects using the year before the change in the federal minimum wage as a reference. We consider three lags and four leads around the change. The coefficients β_t report the differential effects of belonging to a state affected by a rise in the federal minimum wage on establishment employment for each particular year compared to the year before the change in the federal minimum wage.

We present the β coefficients estimated from Equation (11) and the 95% confidence intervals in Figure 6. The graphs provide several striking findings. First, the yearly point estimates show significant negative effects every year after the rise in the minimum wage. In terms of economic magnitude, we find that establishments in a state affected by a rise in the federal minimum wage decrease their employment level by 0.3 % of their average employment level. Also, we find that the yearly treatment effects are not significant before the changes in the minimum wage, suggesting that the parallel trend assumption holds and that we are comparing economically similar establishments.

Figure 6: Yearly treatment coefficients - Financial crisis



Notes: Figure 6 shows the yearly treatment effects from equation (11). The outcome variable is the natural logarithm of employment in establishments. The treatment variable is an indicator variable that equals one if the establishment is located within a state affected by the rise in the federal minimum wage in 2007 and is zero otherwise. The regression includes establishment and year-fixed effects. The plot exhibits yearly point estimates and 95% confidence intervals based on standard errors clustered by state.

Difference-in-differences. Event study estimates from Equation (11) identify treatment effects over time and provide evidence consistent with the parallel trend assumption, supporting the validity of our approach. Since we show the effect is long-lasting and persistent, we consider a more aggregated difference-in-differences approach and estimate Equation (12) reported below:

$$Log(Employment)_{i,t} = \beta Treated_s \times Post_t + \eta_i + \theta_{f,s} + \epsilon_{i,t} \tag{12}$$

In this setting, the variable *Post* is an indicator variable equal to 1 for years after 2007 and 0 otherwise. *Treated* is an indicator variable that equals to one if the establishment is located within a state treated by the rise in the federal minimum wage in 2007 and is zero otherwise.

We report estimation results in Table 6. In line with the earlier event study results, we find that establishments in states affected by a rise in the federal minimum wage decreased their employment levels. More specifically, according to the coefficient estimate reported in the last column, employment decreases by 0.45 % of its average value.

Table 6: Difference-in-differences - Financial crisis

	(1)	(2)	(3)
	All counties	Counties on borders	Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
Post $\times$ Treated	-0.005	-0.014***	-0.013**
	(0.004)	(0.005)	(0.005)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	714,091	217,077	192,622
Adjusted R-squared	0.955	0.957	0.957

Notes: This table shows regression results for Equation (12). We use *Employment (log)* as our outcome variable. All the regressions include establishment and year $\times$ firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, clustered at the state level, are reported in parentheses below the coefficient estimates. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Financial constraints after the federal minimum wage rise. Based on our theoretical framework and previous findings, we expect the negative effect we identify to be more pronounced in establishments belonging to financially constrained firms. We test the role of this firm level factor in this setting by exploiting an exogenous measure of a firm's ability to access external capital, namely the ex-ante variation in long-term debt levels maturing at the onset of the financial crisis. This measure is expected to be unrelated to corporate investment prospects and other corporate characteristics, while also affecting a firm's need for credit intermediation. In fact, firms with large amounts of debt maturing during this financial crisis period are generally unable to roll over their maturing debt, given the serious disruption to capital markets, causing these firms to experience more binding financial constraints. Thus, these firms are compelled to modify their behavior to a greater extent than similar firms without the need to refinance their long-term obligations during the crisis period (Almeida et al., 2012; Benmelech et al., 2019; Duval et al., 2020).

We use this exogenous measure of corporate financial frictions and estimate the following Equation:

$$Log(Employment)_{i,t} = \beta_0 Treated_i \times Post_t + \beta_1 Treated_i \times Post_t \times Constraints_f + \eta_i + \theta_{f,s} + \epsilon_{i,t} \tag{13}$$

In this setting, *Constraints (Short)* represents our exogenous measure of firms experiencing tightening financial constraints during the financial crisis. It is calculated from the dollar amount of long-term debt due in one year in 2007, adjusted by total sales. We express this value as a percentage by multiplying it by 100.

We present the model estimates in Table 7. Our findings indicate that financial constraints significantly alter the employment dynamics of establishments in states affected by the federal minimum wage change. More specifically, the results in the last column suggest that a one-standard-deviation change in the financial friction measure leads to a 0.4% decrease in employment compared to the average establishment employment level.[20]

As a placebo test, we also investigate the impact of long-term debt that matures after the financial crisis, denoted as *Constraints (long)*. This variable is calculated as the sum of long-term debt due in two, three, four, and five years relative to 2007, adjusted by total sales and expressed as a percentage by multiplying it by 100. Consistent with our hypothesis, our analysis reported in Table OA31 does not yield any evidence suggesting that *Constraints (long)* affect establishment employment levels. In fact, the interaction coefficients are approximately zero and statistically insignificant.

Aggregate implications. With additional assumptions, we can use our estimation results from column (3) of Table 7 to assess the impact of corporate financial constraints economy-wide. For this purpose, we follow Chodorow-Reich (2014) and conduct a counterfactual exercise.

We first estimate the total impact of financial constraints on employment in our sample. In the counterfactual analysis, we assume that every establishment exposed to the federal minimum wage

[20] We further demonstrate the validity of our results by estimating Equation (13) using firm-level information. In this setting, *Treated* is measured as the share of employees in states affected by the 2006 federal minimum wage increase. We present consistent results in Table OA30. This is inconsistent with the hypothesis that constrained firms would adjust their employment levels by simply moving employees inter-state across establishments.

Table 7: Financial constraints during the crisis

	(1)	(2)	(3)
	All counties	Counties on borders	Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
Post × Bound	-0.003	-0.006	-0.006
	(0.004)	(0.006)	(0.006)
	[0.403]	[0.295]	[0.365]
Post × Bound × Constraints (short)	-0.000	-0.005*	-0.005*
	(0.002)	(0.002)	(0.003)
	[0.967]	[0.053]	[0.069]
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	611,520	185,902	164,925
Adjusted R-squared	0.953	0.955	0.955

Notes: This table shows regression results for Equation (13). We use *Employment (log)* as our outcome variable. All the regressions include establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, clustered at the state level, are reported in parentheses below the coefficient estimates. *p <0.10, **p <0.05, ***p <0.01. Detailed p-value information is reported in square brackets. See Table OA1 for a detailed description of these variables and their data sources.

increase faces no financial constraints, as described by the following equation:

$$Log(Emp_{i,t})^{CF} = E[Log(Emp_{i,t})|Constraints(Short)_i = 0] = \widehat{Log(Emp_{i,t})} + \hat{\beta}_1 \times \Delta FC_i \quad (14)$$

$Log(Emp_{i,t})^{CF}$ represents the counterfactual (log) employment for establishment i at time t. $E[Log(Emp_{i,t})|Constraints(Short)_i = 0]$ is the expected (log) employment given the absence of a financial constraint. $\widehat{Log(Emp_{i,t})}$ is the fitted value from the employment regression. The adjustment term $\hat{\beta}_1 \times \Delta CF_i$ includes $\hat{\beta}_1$, the estimated coefficient of the interaction between the event, the federal minimum wage treatment, and the financial constraint variable, and ΔCF_i, which represents the difference between the counterfactual and actual financial constraint levels. Then these counterfactual predictions are averaged over establishments in states affected by the federal minimum wage change.

Figure 7 displays the results of this counterfactual analysis. The red line shows the predicted average (log) employment levels assuming no financial constraints, while the blue line depicts the predicted average (log) employment based on actual values observed in the database.

To estimate total employment gains in our sample assuming firms face no financial constraints, we first define Employment Gains for each establishment i at time t as the difference between the counterfactual and actual employment levels:

$$\text{Employment Gains}_{i,t} = \exp(\log(Emp_{i,t})^{CF}) - \exp(\log(\widehat{Emp_{i,t}})) \tag{15}$$

We then calculate the percentage of total employment gains using the following Equation:

$$\text{Total \% Employment Gains} = \frac{\sum_{t=2007}^{2011} \sum_i \text{Employment Gains}_{i,t}}{\text{Total Employment}_{2007}} \times 100 \tag{16}$$

Total Employment_{2007} represents the actual total employment level in our sample in the base year 2007. This measure allows us to calculate the percentage increase in total employment over the 2007-2011 period relative to the initial employment level in 2007, assuming no financial constraints. This exercise indicates a 1.4% increase in total employment relative to the initial aggregate employment level in our sample, abstracting from general equilibrium effects and assuming that the total employment effects represent the sum of the direct effects at each establishment.

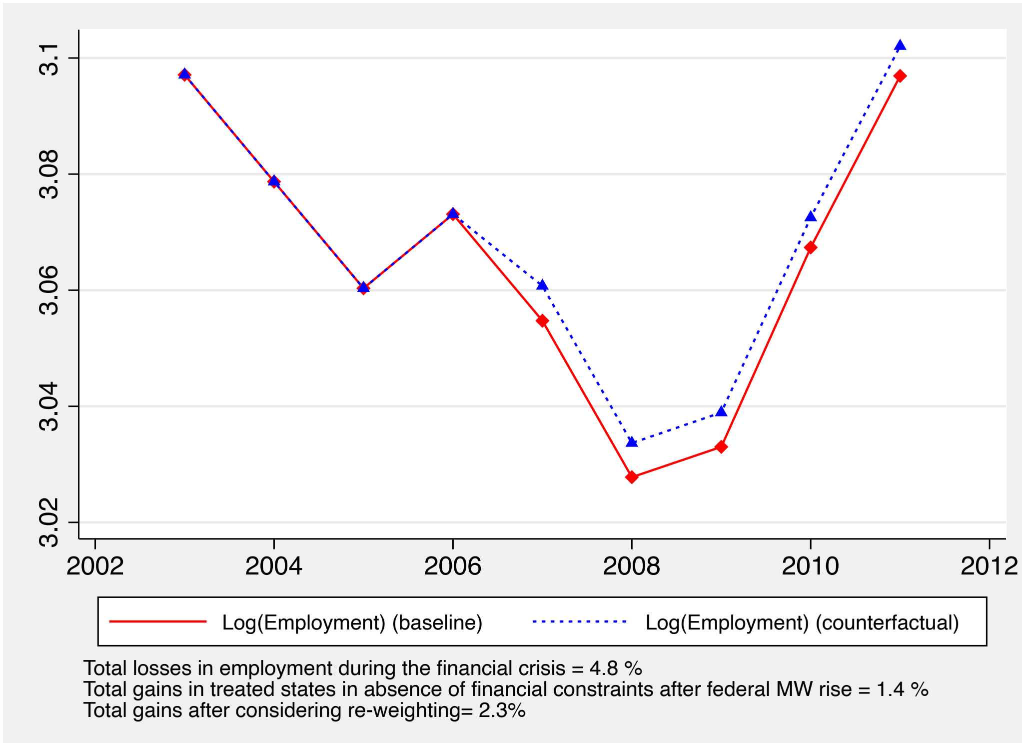
To align our estimate with the broader establishment population, we adjust our predicted values using the weights derived from the NETS dataset's employment distribution across establishment size categories.[21] We apply these weights when aggregating predicted employment levels and calculating total employment effects, scaling our sample-based estimates to better represent the overall economy, which includes both public and private establishments of various sizes. This approach results in a more pronounced employment gain of 2.3%.

Given that the employment gains in our empirical analysis are observed exclusively in states affected by the federal minimum wage change, these estimates have significant economic implications. We conclude that minimum wage policies can significantly impact *aggregate* employment in the presence of corporate financial constraints.

The external validity of our estimated coefficient $\hat{\beta}_1$ limits our prediction of employment gains in

[21] Weights are reported in Table OA32.

Figure 7: Counterfactual exercise



Notes: Figure 7 illustrates our counterfactual exercise results. The red line represents the average Log(Employment) level assuming no financial constraints in states affected by the federal minimum wage change. The blue line depicts the predicted value based on observed values. This graph focuses on establishments in states affected by the federal minimum wage change.

the overall population. Applying this estimate to all US establishments may be compromised as our sample is derived from public corporation establishments. However, private firms are likely to face more serious financial constraints when experiencing comparable debt levels maturing during the crisis period. This suggests that our analysis provides a conservative, lower-bound estimate on the total aggregate employment gains expected in the absence of financial constraints after a minimum wage rise.

7 Conclusions

An extensive literature in economics analyzes the effect of minimum wage policies on employment and finds mixed results (e.g., Card and Krueger, 1994; Cengiz et al., 2019; Clemens and Wither, 2019). Our paper contributes to this literature by analyzing whether corporate balance sheet characteristics can help further explain the employment dynamics at heterogeneous firms following minimum wage changes.

To advance our research, we collect information on the individual establishments that belong to publicly traded corporations in the United States and the corporate balance sheet characteristics of the firms that own them. Using a border discontinuity approach (Card and Krueger, 1994; Cengiz et al., 2019; Dustmann et al., 2022), we do not find any evidence that the minimum wage increases affect employment. However, in line with our research hypothesis, we document that this average effect masks important corporate-level heterogeneity. When we interact a rise in minimum wages with alternative measures of corporate financial frictions, we find a large negative effect for financially constrained firms. In line with the hypothesis that financial constraints are at the core of our findings, we also show, using banking deregulation as an exogenous shock, that an expansion of the local credit supply can mitigate this adverse effect.

Next, we provide causal evidence about the relationship between rises in minimum wage, financial frictions, and employment using a unique quasi-experimental setting. Our analysis exploits the federal minimum wage increase during the 2007-2008 financial crisis and the heterogeneity across firms in long-term debt levels maturing during the same period. Using this clean identification strategy, we find that financially constrained firms in states affected by the federal minimum wage regulations suffer further declines in their employment levels.

Our findings provide new insights into the relationship between minimum wage policies, corporate financial strategies, performance, and subsequent employment decisions. They underscore the crucial role played by financial constraints and access to external capital in explaining the broader dynamics of employment following shifts in minimum wage policies. These insights contribute to a better understanding of the heterogeneous effects of minimum wage changes on employment documented in the previous literature. In doing so, we also provide valuable guidance to policymakers by enhancing their understanding of how firm-specific characteristics influence the efficacy of minimum wage policies. This understanding can aid in crafting more targeted and effective minimum wage policies tailored to specific firms or industries more affected by minimum wage regulation. Our work underscores the need to consider financial frictions – both at the firm and local levels – when formulating optimal government minimum wage policies. Finally, our research enhances our understanding of how minimum wage policies impact corporate financial resources and the potential consequences these laws could have on corporate policies.

References

Abadie, A., S. Athey, G. W. Imbens, and J. M. Wooldridge (2023). When should you adjust standard errors for clustering? *The Quarterly Journal of Economics 138*(1), 1–35.

Addoum, J. M., D. T. Ng, and A. Ortiz-Bobea (2020). Temperature shocks and establishment sales. *The Review of Financial Studies 33*(3), 1331–1366.

Addoum, J. M., D. T. Ng, and A. Ortiz-Bobea (2023). Temperature shocks and industry earnings news. *Journal of Financial Economics 150*(1), 1–45.

Agarwal, S., M. Ayyagari, and R. Kosová (2024). Minimum wage increases and employer performance: Role of employer heterogeneity. *Management Science 70*(1), 225–254.

Agrawal, A. K. and D. A. Matsa (2013). Labor unemployment risk and corporate financing decisions. *Journal of Financial Economics 108*(2), 449–470.

Allegretto, S., A. Dube, M. Reich, and B. Zipperer (2017). Credible research designs for minimum wage studies: A response to Neumark, Salas, and Wascher. *ILR Review 70*(3), 559–592.

Almeida, H., M. Campello, B. Laranjeira, and S. Weisbenner (2012). Corporate debt maturity and the real effects of the 2007 credit crisis. *Critical Finance Review* (1), 3–58.

Almeida, H., M. Campello, and M. S. Weisbach (2004). The cash flow sensitivity of cash. *The Journal of Finance 59*(4), 1777–1804.

Almeida, H., D. Carvalho, and T. Kim (2024). The working capital credit multiplier. *The Journal of Finance*.

Arabzadeh, H., A. Balleer, B. Gehrke, and A. A. Taskin (2024). Minimum wages, wage dispersion and financial constraints in firms. *European Economic Review 163*, 104678.

Baker, A. C., D. F. Larcker, and C. C. Wang (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics 144*(2), 370–395.

Barnatchez, K., L. D. Crane, and R. Decker (2017). An assessment of the national establishment time series (nets) database.

Bartram, S. M., K. Hou, and S. Kim (2022). Real effects of climate policy: Financial constraints and spillovers. *Journal of Financial Economics 143*(2), 668–696.

Belman, D. and P. J. Wolfson (2014). *What does the minimum wage do?* WE Upjohn Institute.

Benmelech, E., N. Bergman, and A. Seru (2021). Financing labor. *Review of Finance 25*(5), 1365–1393.

Benmelech, E., C. Frydman, and D. Papanikolaou (2019). Financial frictions and employment during the Great Depression. *Journal of Financial Economics 133*(3), 541–563.

Berger, E. A., A. W. Butler, E. Hu, and M. Zekhnini (2021). Financial integration and credit democratization: Linking banking deregulation to economic growth. *Journal of Financial Intermediation 45*, 1042–9573.

Bernanke, B. S. (2023). Nobel lecture: Banking, credit, and economic fluctuations. *American Economic Review 113*(5), 1143–1169.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Beuselinck, C., G. Markarian, and A. Verriest (2021). Employee protection shocks and corporate cash holdings. *Journal of Corporate Finance 69*, 102027.

Biais, B. and C. Gollier (1997). Trade credit and credit rationing. *The Review of Financial Studies 10*(4), 903–937.

Bodnaruk, A., T. Loughran, and B. McDonald (2015). Using 10-k text to gauge financial constraints. *Journal of Financial and Quantitative Analysis 50*(4), 623–646.

Bossler, M. and H.-D. Gerner (2020). Employment effects of the new German minimum wage: Evidence from establishment-level microdata. *ILR review 73*(5), 1070–1094.

Boustanifar, H. (2014). Finance and employment: Evidence from U.S. banking reforms. *Journal of Banking & Finance 46*, 343–354.

Brochu, P. and D. A. Green (2013). The impact of minimum wages on labour market transitions. *The Economic Journal 123*(573), 1203–1235.

Brunnermeier, M. K. (2009). Deciphering the liquidity and credit crunch 2007–2008. *Journal of Economic Perspectives 23*(1), 77–100.

Caballero, R. J. and G. Lorenzoni (2014). Persistent appreciations and overshooting: A normative analysis. *IMF Economic Review 62*(1), 1–47.

Caggese, A., V. Cuñat, and D. Metzger (2019). Firing the wrong workers: Financing constraints and labor misallocation. *Journal of Financial Economics 133*(3), 589–607.

Cao, D., G. Lorenzoni, and K. Walentin (2019). Financial frictions, investment, and Tobin's q. *Journal of Monetary Economics 103*, 105–122.

Card, D. and A. B. Krueger (1994). Minimum wages and employment: A case study of the fast food industry in New Jersey and Pennsylvania. *The American Economic Review 84*(4), 772–793.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics 134*(3), 1405–1454.

Chava, S., A. Oettl, and M. Singh (2023). Does a one-size-fits-all minimum wage cause financial stress for small businesses? *Management Science 69*(11), 7095–7117.

Chodorow-Reich, G. (2014). The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *The Quarterly Journal of Economics 129*(1), 1–59.

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

Correia, S. (2016). reghdfe: Estimating linear models with multi-way fixed effects. In *2016 Stata Conference*, Number 24. Stata Users Group.

Coviello, D., E. Deserranno, and N. Persico (2022). Minimum wage and individual worker productivity: Evidence from a large US retailer. *Journal of Political Economy 130*(9), 2315–2360.

Cunat, V. (2007). Trade credit: Suppliers as debt collectors and insurance providers. *The Review of Financial Studies 20*(2), 491–527.

Custódio, C., M. A. Ferreira, and L. Laureano (2013). Why are us firms using more short-term debt? *Journal of Financial Economics 108*(1), 182–212.

Dasgupta, S., E. X. Li, and D. Yan (2019). Inventory behavior and financial constraints: Theory and evidence. *The Review of Financial Studies 32*(3), 1188–1233.

De Chaisemartin, C. and X. d'Haultfoeuille (2024). Difference-in-differences estimators of intertemporal treatment effects. *Review of Economics and Statistics*, 1–45.

Denis, D. J. and V. Sibilkov (2010). Financial constraints, investment, and the value of cash holdings. *The Review of Financial Studies 23*(1), 247–269.

Draca, M., S. Machin, and J. Van Reenen (2011). Minimum wages and firm profitability. *American Economic Journal: Applied Economics 3*(1), 129–151.

Dube, A., L. Giuliano, and J. Leonard (2019). Fairness and frictions: The impact of unequal raises on quit behavior. *American Economic Review 109*(2), 620–663.

Dube, A., T. W. Lester, and M. Reich (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *The Review of Economics and Statistics 92*(4), 945–964.

Dube, A., T. W. Lester, and M. Reich (2016). Minimum wage shocks, employment flows, and labor market frictions. *Journal of Labor Economics 34*(3), 663–704.

Dustmann, C., A. Lindner, U. Schönberg, M. Umkehrer, and P. Vom Berge (2022). Reallocation effects of the minimum wage. *The Quarterly Journal of Economics 137*(1), 267–328.

Duval, R., G. H. Hong, and Y. Timmer (2020). Financial frictions and the great productivity slowdown. *The Review of Financial Studies 33*(2), 475–503.

Eisfeldt, A. L. and A. A. Rampini (2007). New or used? Investment with credit constraints. *Journal of Monetary Economics 54*(8), 2656–2681.

Farre-Mensa, J. and A. Ljungqvist (2016). Do measures of financial constraints measure financial constraints? *The Review of Financial Studies 29*(2), 271–308.

Faulkender, M. and R. Wang (2006). Corporate financial policy and the value of cash. *The Journal of Finance 61*(4), 1957–1990.

Favara, G. and J. Imbs (2015). Credit supply and the price of housing. *American Economic Review 105*(3), 958–992.

Fougère, D., E. Gautier, and H. Le Bihan (2010). Restaurant prices and the minimum wage. *Journal of Money, Credit and Banking 42*(7), 1199–1234.

Geng, H. G., Y. Huang, C. Lin, and S. Liu (2022). Minimum wage and corporate investment: Evidence from manufacturing firms in China. *Journal of Financial and Quantitative Analysis 57*(1), 94–126.

Gertler, M. and S. Gilchrist (1994). Monetary policy, business cycles, and the behavior of small manufacturing firms. *The Quarterly Journal of Economics 109*(2), 309–340.

Ghaly, M., V. Anh Dang, and K. Stathopoulos (2017). Cash holdings and labor heterogeneity: The role of skilled labor. *The Review of Financial Studies 30*(10), 3636–3668.

Giannetti, M., M. Burkart, and T. Ellingsen (2011). What you sell is what you lend? Explaining trade credit contracts. *The Review of Financial Studies 24*(4), 1261–1298.

Gilchrist, S., R. Schoenle, J. Sim, and E. Zakrajšek (2017). Inflation dynamics during the financial crisis. *American Economic Review 107*(3), 785–823.

Giroud, X. and H. M. Mueller (2017). Firm leverage, consumer demand, and employment losses during the Great Recession. *The Quarterly Journal of Economics 132*(1), 271–316.

Giroud, X. and H. M. Mueller (2019). Firms' internal networks and local economic shocks. *American Economic Review 109*(10), 3617–49.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics 225*(2), 254–277.

Gopalan, R., B. H. Hamilton, A. Kalda, and D. Sovich (2021). State minimum wages, employment, and wage spillovers: Evidence from administrative payroll data. *Journal of Labor Economics 39*(3), 673–707.

Gopalan, R., B. H. Hamilton, J. Sabat, and D. Sovich (2024). Aversion to student debt? Evidence from low-wage workers. *Journal of Finance 79*(2), 1249–1295.

Guo, L., J. Kong, and R. W. Masulis (2024). Do employee interests affect target board decisions about acquisition offers? Evidence from changes in unemployment insurance. *Management Science*.

Gustafson, M. and J. Kotter (2023). Higher minimum wages reduce capital expenditures. *Management Science 69*(5), 2933–2953.

Hadlock, C. J. and J. R. Pierce (2010). New evidence on measuring financial constraints: Moving beyond the KZ index. *The Review of Financial Studies 23*(5), 1909–1940.

Harasztosi, P. and A. Lindner (2019). Who pays for the minimum wage? *American Economic Review 109*(8), 2693–2727.

Hau, H., Y. Huang, and G. Wang (2020). Firm response to competitive shocks: Evidence from China's minimum wage policy. *The Review of Economic Studies 87*(6), 2639–2671.

Hennessy, C. A. and T. M. Whited (2007). How costly is external financing? Evidence from a structural estimation. *The Journal of Finance 62*(4), 1705–1745.

Hsu, J. W., D. A. Matsa, and B. T. Melzer (2018). Unemployment insurance as a housing market stabilizer. *American Economic Review 108*(1), 49–81.

Hubbard, R. G. (1998). Capital-market imperfections and investment. *Journal of Economic Literature 36*(1), 193–225.

Itskhoki, O. and B. Moll (2019). Optimal development policies with financial frictions. *Econometrica 87*(1), 139–173.

Kahn-Lang, A. and K. Lang (2020). The promise and pitfalls of differences-in-differences: Reflections on *16 and Pregnant* and other applications. *Journal of Business & Economic Statistics 38*(3), 613–620.

Kaplan, S. N. and L. Zingales (1997). Do investment-cash flow sensitivities provide useful measures of financing constraints? *The Quarterly Journal of Economics 112*(1), 169–215.

Krishnan, K., D. K. Nandy, and M. Puri (2015). Does financing spur small business productivity? Evidence from a natural experiment. *The Review of Financial Studies 28*(6), 1768–1809.

Ku, H. (2022). Does minimum wage increase labor productivity? Evidence from piece rate workers. *Journal of Labor Economics 40*(2), 325–359.

Lamont, O., C. Polk, and J. Saaá-Requejo (2001). Financial constraints and stock returns. *The Review of Financial Studies 14*(2), 529–554.

Leung, J. H. (2021). Minimum wage and real wage inequality: Evidence from pass-through to retail prices. *Review of Economics and Statistics 103*(4), 754–769.

Meer, J. and J. West (2016). Effects of the minimum wage on employment dynamics. *Journal of Human Resources 51*(2), 500–522.

Mian, A. and A. Sufi (2014). What explains the 2007–2009 drop in employment? *Econometrica 82*(6), 2197–2223.

Mukherjee, A., M. Singh, and A. Žaldokas (2017). Do corporate taxes hinder innovation? *Journal of Financial Economics 124*(1), 195–221.

Neumark, D., J. I. Salas, and W. Wascher (2014). Revisiting the minimum wage—employment debate: Throwing out the baby with the bathwater? *ILR Review 67*(3_suppl), 608–648.

Neumark, D., M. Schweitzer, and W. Wascher (2004). Minimum wage effects throughout the wage distribution. *Journal of Human Resources 39*(2), 425–450.

Neumark, D. and W. L. Wascher (2008). *Minimum wages.* MIT press.

Nimier-David, E., D. Sraer, and D. Thesmar (2023). The effects of mandatory profit-sharing on workers and firms: Evidence from France. Technical report, National Bureau of Economic Research.

Oi, W. Y. (1962). Labor as a quasi-fixed factor. *Journal of Political Economy 70*(6), 538–555.

Peress, J. (2010). Product market competition, insider trading, and stock market efficiency. *The Journal of Finance 65*(1), 1–43.

Petersen, M. A. and R. G. Rajan (1995). The effect of credit market competition on lending relationships. *The Quarterly Journal of Economics 110*(2), 407–443.

Petersen, M. A. and R. G. Rajan (1997). Trade credit: Theories and evidence. *The Review of Financial Studies 10*(3), 661–691.

Portugal, P. and A. R. Cardoso (2006). Disentangling the minimum wage puzzle: An analysis of worker accessions and separations. *Journal of the European Economic Association 4*(5), 988–1013.

Rambachan, A. and J. Roth (2023). A more credible approach to parallel trends. *Review of Economic Studies 90*(5), 2555–2591.

Rice, T. and P. E. Strahan (2010). Does credit competition affect small-firm finance? *The Journal of Finance 65*(3), 861–889.

Roth, J. (2022). Pretest with caution: Event-study estimates after testing for parallel trends. *American Economic Review: Insights 4*(3), 305–22.

Schmidheiny, K. and S. Siegloch (2023). On event studies and distributed-lags in two-way fixed effects models: Identification, equivalence, and generalization. *Journal of Applied Econometrics 38*(5), 695–713.

Schmitt, J. (2015). Explaining the small employment effects of the minimum wage in the United States. *Industrial Relations: A Journal of Economy and Society 54*(4), 547–581.

Siemer, M. (2019). Employment effects of financial constraints during the Great Recession. *Review of Economics and Statistics 101*(1), 16–29.

Solon, G., S. J. Haider, and J. M. Wooldridge (2015). What are we weighting for? *Journal of Human Resources 50*(2), 301–316.

Stigler, G. J. (1946). The economics of minimum wage legislation. *The American Economic Review 36*(3), 358–365.

Sun, L. and S. Abraham (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics 225*(2), 175–199.

Vaghul, K. and B. Zipperer (2021). Historical state and sub-state minimum wages. Version 1.3.0, https://github.com/benzipperer/historicalminwage/releases/tag/v1.3.0.

Whited, T. M. and G. Wu (2006). Financial constraints risk. *The Review of Financial Studies 19*(2), 531–559.

Ziebarth, N. L. (2013). Identifying the effects of bank failures from a natural experiment in Mississippi during the Great Depression. *American Economic Journal: Macroeconomics 5*(1), 81–101.

Zipperer, B. (2023). The impact of the Raise the Wage Act of 2023. *Economic Policy Institute*, Retrieved from https://www.epi.org/publication/rtwa–2023–impact–fact–sheet/.

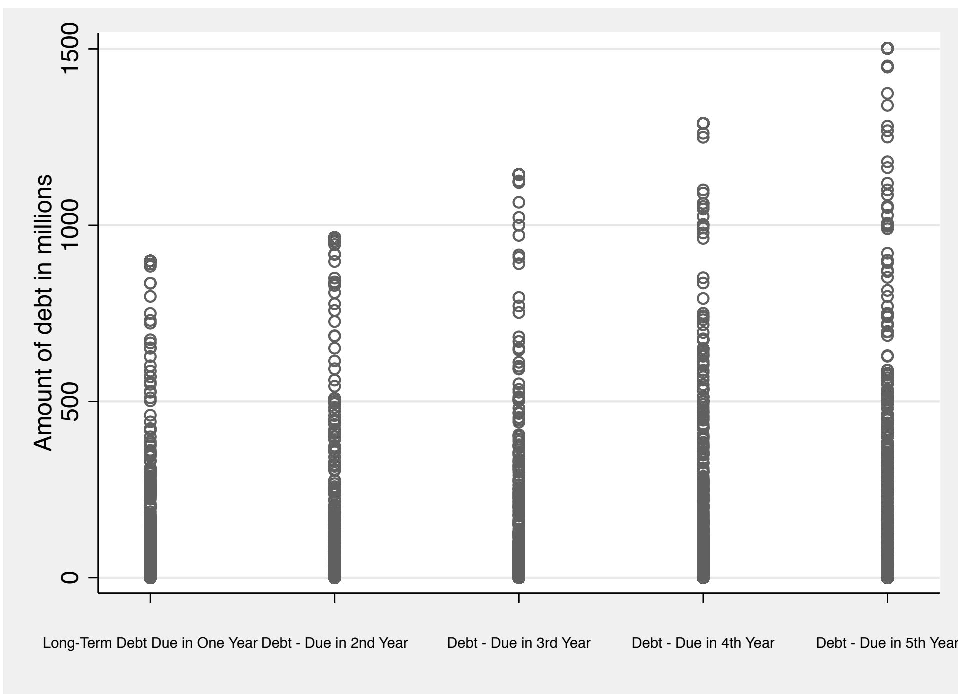
Online Appendix

This Appendix is for Online Publication (OA) and provides further details on the data and the results of the paper "Now You See It, Now You Don't: Financial Constraints, Minimum Wage Policies, and Employment". In the last section, we also report a simple model to conceptualize our research hypothesis.

OA1 Figures

Corporate debt structure during the financial crisis. Figure OA1 illustrates the wide variation in long-term debt maturity across different years at the beginning of the financial crisis.

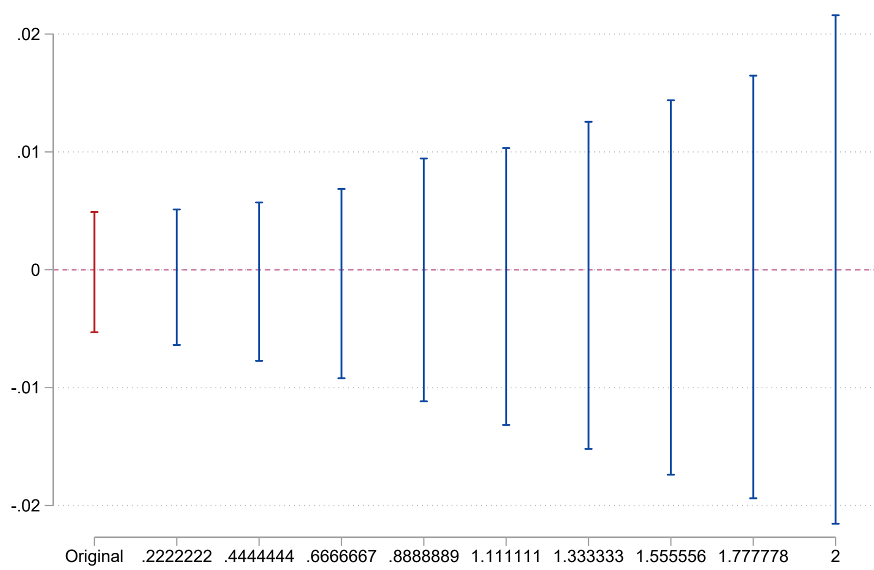
Figure OA1: Corporate debt structure



Notes: Figure OA1 illustrates the wide variation in debt maturity across different years at the beginning of the financial crisis.

Sensitivity test. Figure OA2 shows the results of the sensitivity test proposed by Rambachan and Roth (2023). We calculate 95 % confidence intervals for our main estimators under varying assumptions on the value M, the upper limit for the change between two consecutive periods in the slope of the underlying linear trend. We show that the fixed length confidence intervals are similar to those from our baseline estimator when allowing for violations of parallel trends that are approximately linear and for larger degrees of possible non-linearity in the violation of parallel trends.

Figure OA2: Sensitivity test

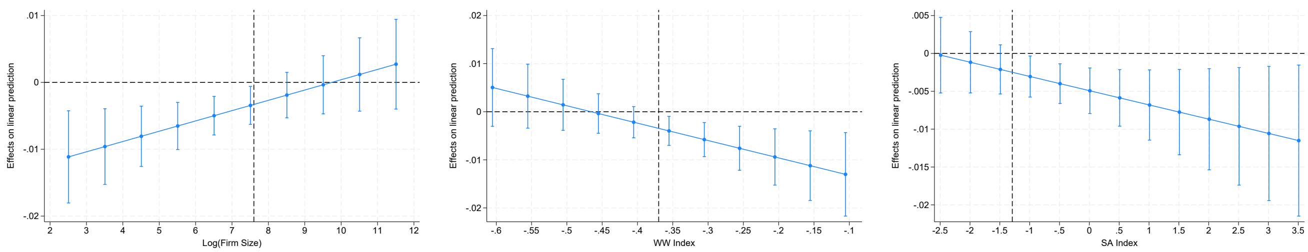


Notes: Figure OA2 provides the results of a formal sensitivity analysis that relates the magnitude of violations of parallel trends to the robustness of treatment estimates in post-treatment periods (Rambachan and Roth, 2023). It shows 95 % confidence intervals for our main estimators under varying assumptions of the value M on the x-axis.

Marginal effects. Figure OA3 shows the marginal effects of minimum wage on employment for different values of the financial constraint variables.

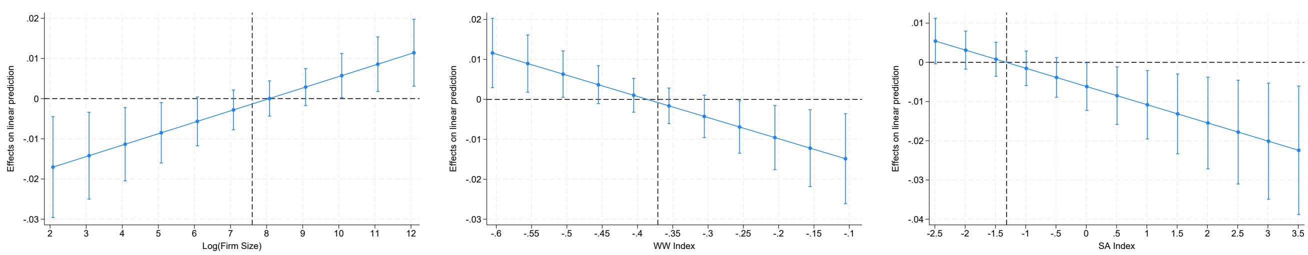
Figure OA3: Marginal effects

All Counties



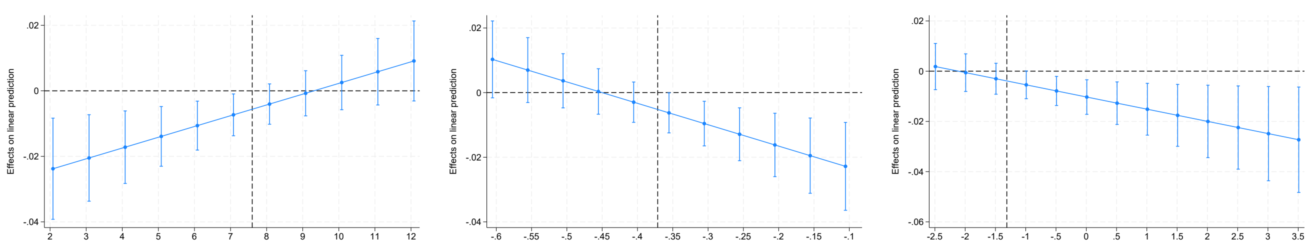
(a) Size (b) WW Index (c) SA Index

Counties on borders



(a) Size (b) WW Index (c) SA Index

Counties on borders (,â§ 75 km)

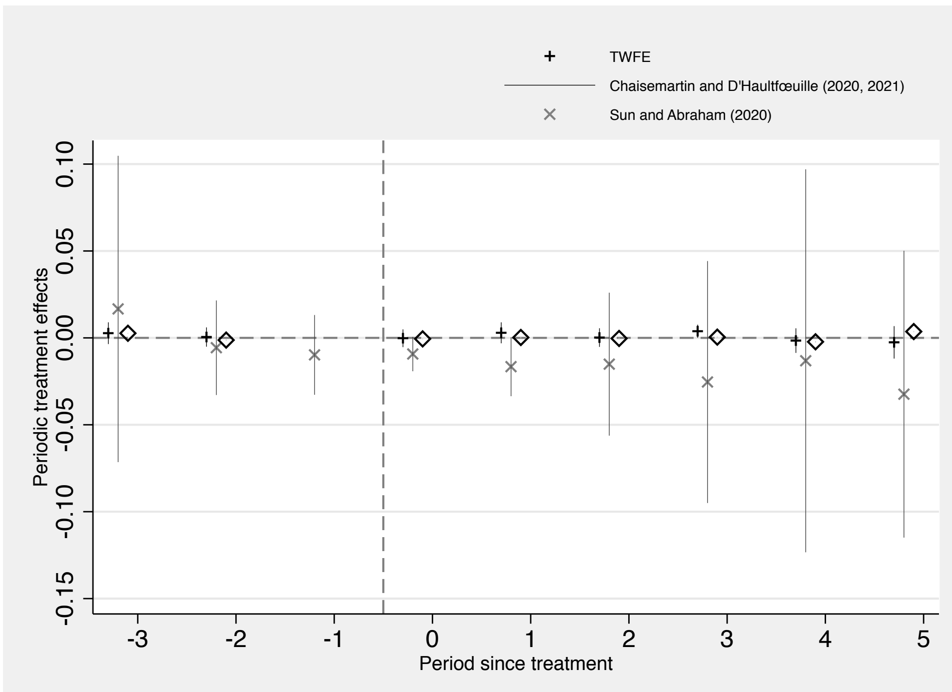


(a) Size (b) WW Index (c) SA Index

Notes: Figure OA3 shows the marginal effects of the minimum wage on employment for different values of the financial constraint variables. The vertical dashed lines indicate the median value of each financial constraint variable.

Alternative staggered difference-in-differences estimators. We focus our analysis on counties located along states' borders and present in Figure OA4 annual point estimates of the effect of minimum wage changes on employment obtained using the methodologies introduced by De Chaisemartin and d'Haultfoeuille (2024) and Sun and Abraham (2021). We also compare these estimators to the results of a two-way fixed effects (TWFE) model.

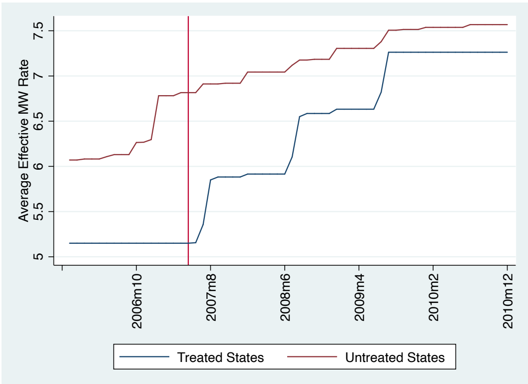
Figure OA4: Alternative estimators



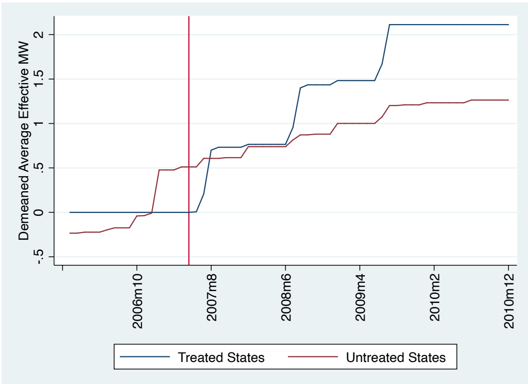
Notes: Figure OA4 depicts the annual treatment effects utilizing the two-way fixed effects (TWFE) estimator and compares them with the estimators introduced by De Chaisemartin and d'Haultfoeuille (2024) and Sun and Abraham (2021). The outcome variable is the natural logarithm of employment in the establishment. The plot exhibits yearly point estimates and 95% confidence intervals based on standard errors clustered by state.

Effective minimum wage changes of treated and untreated states. We show in Figure OA5(a) the effective average minimum wage in states treated to the federal minimum wage in 2007 and states untreated to the federal minimum wage. Figure OA5(b) shows the divergence patterns for the two groups when we demean each time series using the pre-crisis average for the treatment and the control groups.

Figure OA5: Effective minimum wage changes of treated and untreated states



(a) Effective MW rates



(b) Divergence in the effective MW rates

Notes: Figure OA5(a) shows the effective average minimum wage in states treated by the increase in the federal minimum wage in 2007 and states untreated by the new federal minimum wage rate. Figure OA5(b) shows the divergence patterns for the two groups when we demean each time series using the pre-crisis average for the treatment and control groups.

OA2 Tables

Variable definition. Table OA1 contains detailed information on the variables that we use in the empirical analysis, their description, and their sources.

Table OA1: Variable description

Variable name	Description	Source
	Panel A: Establishment Level	
Minimum Wage (MW)	State-level effective minimum wage.	Vaghul and Zipperer (2021)
Employment	The number of employees working at the establishment.	NETS
Sales	Total establishment's sales in dollar amounts.	NETS
Firm Size	A measure of the firm's total assets in millions of dollars (item AT).	Compustat
SA Index	Following Hadlock and Pierce (2010), the SA index is defined as (-0.737 $\times$ log(AT))+(0.043 $\times$ log(AT)^2) $-$ (0.040$\times$ Age). Age is defined as the number of years the firm is present in Compustat. Size is winsorized at (the log of) \$4.5 billion, and Age is winsorized at 37 years. A higher value of the index indicates a greater financial constraints.	Compustat
WW Index	Following Whited and Wu (2006), the WW index is defined as (-0.091 $\times$ CF) - (0.062 $\times$ DIVPOS)+(0.021 $\times$ TLTD) - (0.044 $\times$ log(AT)) + (0.102 $\times$ ISG) - (0.035 $\times$ SG), where CF is the ratio of cash flow (item IB + item DP) to total assets (item AT), DIVPOS is an indicator variable equal to one if the firm pays cash dividends (item DVT) and zero otherwise, TLTD is the ratio of long-term debt (item DLTT) to total assets (item AT), ISG is the firm's 3-digit SIC industry sales (item SALE) growth, SG is firm sales (item SALE) growth. A higher value of the index indicates a greater financial constraints.	Compustat
KZ Index	Following Kaplan and Zingales (1997), the KZ index is defined as: -1.001$\times$[(IB+DP)/PPEGT] +0.282$\times$[(AT+MktVal-CEQ-TXDB)/AT]+3.139 $\times$[(DLTT+DLC)/(DLTT+DLC+AT)]-39.367 $\times$[(DVC+DVP)/PPEGT]-1.314$\times$[CH/PPEGT].MktVal is defined as [CSHO$\times$PRCC]. MktVal and PPEGT are lagged. A higher value of the index indicates a greater financial constraints.	Compustat

Notes: This table shows a detailed description of the variables and their data sources.

Table OA1: Variable description (continued)

Composite Index	A combined measure of corporate financial constraints defined as equal to one if the majority of the firm's financial constraint indicators are equal to one (above-median annual values for the KZ, SA, and WW indices; below-median annual values for firm size), and zero otherwise.	Compustat
PCA Index	An index based on the principal component analysis (PCA) of the four measures of financial constraints (KZ, SA, WW indices, and Size).	Compustat
Constraining Words (log)	A textual analysis measure of financial constraint based on the dictionary of words associated with constraint in 10-K (Bodnaruk et al. (2015)). The measure is defined as the natural logarithm of the number of constraining words in the firm's 10-K.	Bodnaruk et al. (2015)
Document Length (log)	A textual analysis measure of document length of the 10-K report. The measure is defined as the natural logarithm of the number of words in the firm's 10-K in a previous year.	Bodnaruk et al. (2015)
Tangibility	Ratio of a firm's property, plant, and equipment (item PPENT) to total assets (item AT).	Compustat
ROA	Ratio of firm's income before extraordinary items (item IB) to total assets (item AT).	Compustat
ROI	Ratio of firm's income before extraordinary items (item IB) to invested capital (item ICAPT).	Compustat
Treated	An indicator variable equal to one if the establishment is located in a bounded state to the federal minimum wage change, zero otherwise.	Authors
Constraints (short)	Ratio of firm's long-term debt due in one year (item DD1) to sales (item SALE). This variable is measured in the fiscal year 2007. We report the value in % by multiplying it by 100.	Compustat
Constraints (long)	Ratio of firm's long-term debt due in two, three, four, and five years (item DD2, DD3, DD4, and DD5) to sales (item SALE). This variable is measured as in the fiscal year 2007. We report the value in % by multiplying it by 100.	Compustat

Notes: This table describes the variables and their data sources.

Table OA1: Variable description (continued)

Log(Population)	Natural logarithm of the county,Äôs population as reported by the Bureau of Economic Analysis (BEA).	BEA
Income	County's income per capita as reported by the Bureau of Economic Analysis (BEA).	BEA
Unemployment	County's unemployment rate as reported by the Bureau of Labor Statistics (BLS).	BLS
Deregulation	Index of interstate branching deregulation ranging from 0 (most restricted) to 4 (least restricted).	Rice and Strahan (2010)
	Panel B: Corporate Level	
MW Exposure	Corporate exposure to minimum wage policies are measured as state minimum wages weighted by the number of employees at the firm's establishments across the states.	Vaghul and Zipperer (2021)
Employment	The number of employees working at the firm measured in thousands.	Compustat
Log(Firm Size)	Natural logarithm of the firm's total assets in millions of dollars (item AT).	Compustat
SA Index	Following Hadlock and Pierce (2010), the SA index is defined as (-0.737 $\times$ log(AT))+(0.043 $\times$ log(AT)^2)$-$(0.040$\times$ Age). Age is defined as the years the firm is present in Compustat. Size is winsorized at (the log of) $4.5 billion, and Age is winsorized at 37 years. A higher value of the index indicates a greater financial constraint.	Compustat
WW Index	Following Whited and Wu (2006), the WW index is defined as (-0.091 $\times$ CF) - (0.062 $\times$ DIVPOS)+(0.021 $\times$ TLTD) - (0.044 $\times$ log(item AT)) + (0.102 $\times$ ISG) - (0.035 $\times$ SG), where CF is the ratio of cash flow (item IB + item DP) to total assets (item AT), DIVPOS is an indicator variable equal to one if the firm pays cash dividends (item DVT) and zero otherwise, TLTD is the ratio of long-term debt (item DLTT) to total assets (item AT), ISG is the firm's 3-digit SIC industry sales (item SALE) growth, SG is firm sales (item SALE) growth. A higher value of the index indicates a greater financial constraint.	Compustat
Cash	Ratio of cash and marketable securities (item CHE) to the lag of physical capital (item AT).	Compustat
R&D	Ratio of research and development expenses (item XRD) to the lag of total assets (item AT).	Compustat

Notes: This table describes the variables and their data sources.

Table OA1: Variable description (continued)

Capital Expenditures	Ratio of capital expenditures (item CAPX) to the lag of total assets (item AT).	Compustat
Leverage	Ratio of current (item DLC) and long-term debt (item DLTT) to the lag of stockholders' equity (item SEQ.	Compustat
Leverage (Short)	Ratio of debt in current liabilities (item DLC) to the lag of stockholders equity (item SEQ).	Compustat
Leverage (Long)	Ratio of long-term debt (item DLTT) to the lag of stockholders equity (item SEQ).	Compustat
Trade Credit	Ratio of accounts payable (item AP) to the lag of stockholders equity (item SEQ).	Compustat

Notes: This table describes the variables and their data sources.

Heterogeneous effect across sectors. We conduct separate analyses for establishments operating in companies that belong to minimum wage-exposed industries and those that are not. We define minimum wage-exposed industries following the work of Gustafson and Kotter (2023). The high-exposure industries include restaurant, retail, and entertainment, which employ more than 70% of minimum wage labor. We report estimation results in Tables OA2 and OA3.

Table OA2: Minimum Wage - Non-exposed industries

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.010	-0.021	-0.028
	(0.007)	(0.014)	(0.018)
MW × Log(Firm Size)	0.001	0.003	0.003
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,197,458	363,087	323,936
Adjusted R-squared	0.923	0.924	0.925
	Panel B: Interaction with the WW index		
MW	-0.010*	-0.017	-0.026
	(0.006)	(0.013)	(0.016)
MW × WW Index	-0.017	-0.045	-0.057
	(0.015)	(0.031)	(0.039)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,141,421	346,718	309,263
Adjusted R-squared	0.925	0.925	0.926
	Panel C: Interaction with the SA index		
MW	-0.004	-0.004	-0.008
	(0.003)	(0.005)	(0.006)
MW × SA Index	-0.001	-0.003	-0.004
	(0.002)	(0.003)	(0.003)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,197,458	363,087	323,936
Adjusted R-squared	0.923	0.924	0.925

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). We further restrict our sample to establishments that operate in industries that are not very exposed to minimum wage raises. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Table OA3: Minimum wage - Exposed industries

	(1) All counties Employment (log)	(2) Counties on borders Employment (log)	(3) Counties on borders ($\leq$ 75 km) Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.020**	-0.025**	-0.033***
	(0.010)	(0.011)	(0.011)
MW × Log(Firm Size)	0.002*	0.003**	0.004**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,086,404	327,634	286,696
Adjusted R-squared	0.938	0.941	0.940
	Panel B: Interaction with the WW index		
MW	-0.024**	-0.024**	-0.034***
	(0.011)	(0.009)	(0.011)
MW × WW Index	-0.058**	-0.061**	-0.076***
	(0.029)	(0.023)	(0.027)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,044,791	315,059	275,566
Adjusted R-squared	0.939	0.942	0.941
	Panel C: Interaction with the SA index		
MW	-0.007***	-0.009**	-0.013***
	(0.002)	(0.004)	(0.003)
MW × SA Index	-0.004*	-0.006***	-0.006**
	(0.002)	(0.002)	(0.003)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,086,404	327,634	286,696
Adjusted R-squared	0.945	0.941	0.940

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). We further restrict our sample to establishments operating in industries more exposed to minimum wage raises. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Alternative measures of financial constraint. We investigate whether our results hold when considering alternative measures of financial constraints. We start by considering the first index of corporate financial constraints that have been considered in the literature, that is, the KZ index (Kaplan and Zingales, 1997). We report the results in Panel A of Table OA4. To further assess the robustness of our findings, we adopt a methodology similar to that presented by Bartram et al. (2022) by constructing a composite financial constraints measure. This method relies on the rankings of firms based on the KZ, SA, and WW indices and their asset size. We categorize a firm as financially constrained if it exhibits above-median annual values for the KZ, SA, and WW indices alongside below-median annual values for firm size. If most of these indicators point to financial constraints, the composite indicator is set to one; otherwise, it is set to zero. We present the results in Panel B of Table OA4. Our results still hold when we consider a composite index based on the principal component analysis (PCA) of these four alternative measures of financial friction. We report the results in Panel C of Table OA4. Finally, we demonstrate the robustness of our results when considering an alternative measure of financial constraint based on a textual analysis of a firm's annual 10-K filing. We use the dictionary of 'constraining' words proposed in Bodnaruk et al. (2015) to do so. We take the natural logarithm of the word frequency of this dictionary and control for differences in the length of the 10-K filings. We present the results in panel D of Table OA4.

Table OA4: Financial constraints - Alternative variables

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel A: Interaction with the KZ Index		
MW	-0.000	0.003	-0.002
	(0.002)	(0.002)	(0.003)
MW × KZ Index	0.000	-0.000	-0.000
	(0.000)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,800,543	544,259	480,704
R-squared	0.942	0.945	0.945
	Panel B: Interaction with the composite index		
MW	0.000	0.006**	0.003
	(0.002)	(0.003)	(0.004)
MW × Composite Index	-0.002	-0.008**	-0.010**
	(0.002)	(0.003)	(0.004)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,796,861	543,217	479,772
R-squared	0.942	0.945	0.945
	Panel C: Interaction with the PCA index		
MW	-0.003*	-0.001	-0.005
	(0.001)	(0.002)	(0.003)
MW × PCA Index	-0.003**	-0.005***	-0.005**
	(0.001)	(0.001)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,796,861	543,217	479,772
R-squared	0.942	0.945	0.945
	Panel D: Interaction with the textual analysis index		
MW	-0.054*	-0.104*	-0.141**
	(0.029)	(0.056)	(0.064)
MW × Constraining Words (log)	-0.007**	-0.011*	-0.017**
	(0.003)	(0.006)	(0.007)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,485,721	453,328	399,748
R-squared	0.952	0.954	0.955

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable and alternative measures of financial constraints as our independent variables. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Measurement errors in the NETS database. We conduct a robustness check by removing establishments with fewer than five employees, as these observations are more likely to have imputed data according to Barnatchez et al. (2017). In an additional test, we remove establishments with round numbers of employees (5, 10, 100, 200, ... 1000), as these observations are also more likely to be based on imputation. The results are reported in Tables OA5 and OA6.

Table OA5: Removing establishments with less than five employees

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.007*	-0.019**	-0.022**
	(0.004)	(0.009)	(0.010)
MW × Log(Firm Size)	0.001	0.002**	0.002*
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	1,868,500	564,088	499,315
Adjusted R-squared	0.953	0.955	0.954
	Panel B: Interaction with the WW index		
Annual State Average	-0.007	-0.015*	-0.020**
	(0.004)	(0.008)	(0.008)
c.mean_mw#c.lag_ww	-0.013	-0.040**	-0.047**
	(0.012)	(0.019)	(0.021)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	1,788,561	540,296	478,036
Adjusted R-squared	0.954	0.956	0.956
	Panel C: Interaction with the SA index		
Annual State Average	-0.003**	-0.006*	-0.008**
	(0.001)	(0.003)	(0.003)
MW × SA Index	-0.001	-0.004**	-0.004*
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	1,868,500	564,088	499,315
Adjusted R-squared	0.953	0.955	0.954

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include establishment and year-fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Additionally, we remove establishments with less than five employees. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Table OA6: Removing establishments with round number

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.017***	-0.025***	-0.031***
	(0.006)	(0.008)	(0.010)
MW × Log(Firm Size)	0.002**	0.003***	0.003**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	1,909,044	575,366	508,474
Adjusted R-squared	0.933	0.935	0.936
	Panel B: Interaction with the WW index		
MW	-0.020***	-0.021***	-0.030***
	(0.007)	(0.007)	(0.009)
MW × WW Index	-0.044**	-0.055***	-0.066***
	(0.018)	(0.019)	(0.023)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	1,825,809	550,687	486,460
Adjusted R-squared	0.935	0.936	0.937
	Panel C: Interaction with the SA index		
MW	-0.005***	-0.005*	-0.010***
	(0.002)	(0.003)	(0.003)
MW × SA Index	-0.002	-0.004**	-0.005*
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	1,909,044	575,366	508,474
Adjusted R-squared	0.933	0.935	0.936

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year-fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Additionally, we remove establishments that report a round number of employees (5, 10, 100, 200, ... 1000), as these are more likely to involve imputation. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Alternative fixed effects specification. We show that our results hold when we consider alternative models. More specifically, we consider the following alternative fixed effect specifications: (i) establishment and year fixed effects; (ii) state and year fixed effects; and (iii) county and year fixed effects. We report estimation results in Tables OA7, OA8, and OA9.

Table OA7: Alternative FE - Establishments and year

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.026***	-0.034***	-0.040***
	(0.009)	(0.009)	(0.009)
Log(Firm Size)	0.036***	0.025***	0.023***
	(0.007)	(0.008)	(0.008)
MW × Log(Firm Size)	0.003***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,283,871	703,268	623,075
Adjusted R-squared	0.924	0.924	0.924
		Panel B: Interaction with the WW index	
MW	-0.026***	-0.031***	-0.037***
	(0.008)	(0.008)	(0.008)
WW	0.060	0.177	0.220*
	(0.102)	(0.114)	(0.116)
MW × WW Index	-0.068***	-0.089***	-0.097***
	(0.017)	(0.018)	(0.018)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,186,234	672,897	595,830
Adjusted R-squared	0.926	0.926	0.926
		Panel C: Interaction with the SA index	
MW	-0.008**	-0.008**	-0.012***
	(0.003)	(0.003)	(0.003)
SA Index	-0.020**	-0.003	-0.001
	(0.009)	(0.008)	(0.009)
MW × SA Index	-0.006***	-0.008***	-0.008***
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,283,871	703,268	623,075
Adjusted R-squared	0.924	0.924	0.924

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year-fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Table OA8: Alternative FE - State and year

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.140***	-0.153***	-0.153***
	(0.043)	(0.039)	(0.040)
Log(Firm Size)	0.085***	0.071***	0.070***
	(0.026)	(0.026)	(0.025)
MW × Log(Firm Size)	0.015***	0.018***	0.018***
	(0.005)	(0.005)	(0.005)
State FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,285,340	703,767	623,534
Adjusted R-squared	0.0782	0.0806	0.0783
		Panel B: Interaction with the WW index	
MW	-0.160***	-0.170***	-0.171***
	(0.040)	(0.036)	(0.038)
WW Index	-1.062**	-0.831*	-0.797
	(0.499)	(0.485)	(0.476)
MW × WW Index	-0.367***	-0.409***	-0.413***
	(0.100)	(0.089)	(0.089)
State FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,191,990	674,675	597,454
Adjusted R-squared	0.0770	0.0792	0.0770
		Panel C: Interaction with the SA index	
MW	-0.061***	-0.059***	-0.059***
	(0.011)	(0.011)	(0.014)
SA Index	-0.003	0.014	0.011
	(0.032)	(0.032)	(0.032)
MW × SA Index	-0.033***	-0.036***	-0.036***
	(0.008)	(0.007)	(0.006)
State FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,285,340	703,767	623,534
Adjusted R-squared	0.0499	0.0504	0.0503

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the state and year-fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Table OA9: Alternative FE - County and year

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.138***	-0.148***	-0.148***
	(0.042)	(0.039)	(0.040)
Log(Firm Size)	0.089***	0.076***	0.076***
	(0.025)	(0.025)	(0.024)
MW × Log(Firm Size)	0.015***	0.017***	0.017***
	(0.005)	(0.005)	(0.004)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,285,340	703,767	623,534
Adjusted R-squared	0.0992	0.104	0.101
		Panel B: Interaction with the WW index	
MW	-0.158***	-0.165***	-0.165***
	(0.038)	(0.035)	(0.037)
WW Index	-1.115**	-0.912*	-0.897*
	(0.479)	(0.461)	(0.465)
MW × WW Index	-0.366***	-0.403***	-0.402***
	(0.097)	(0.086)	(0.087)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,191,982	674,670	597,450
Adjusted R-squared	0.0980	0.103	0.0998
		Panel C: Interaction with the SA index	
MW	-0.061***	-0.058***	-0.058***
	(0.011)	(0.011)	(0.013)
SA Index	-0.002	0.013	0.008
	(0.030)	(0.030)	(0.031)
MW × SA Index	-0.034***	-0.037***	-0.036***
	(0.007)	(0.006)	(0.006)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,285,340	703,767	623,534
Adjusted R-squared	0.0713	0.0745	0.0737

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the county and year firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Matching of paired counties. We follow Dube et al. (2010) and propose an alternative identification strategy. We match each county on a state border to potentially multiple pairs of other counties. We then control for pair- or pair-by-period fixed effects. The results are reported in Table OA10, and notably, all results exhibit the same sign and significance as our baseline results.

Table OA10: Pair matching

	(1) Emp. (log)	(2) Emp. (log)	(3) Emp. (log)	(4) Emp. (log)	(5) Emp. (log)	(6) Emp. (log)
MW	-0.022**	-0.023**	-0.017**	-0.018**	-0.006*	-0.009**
	(0.009)	(0.009)	(0.008)	(0.009)	(0.003)	(0.004)
MW × Log(Size)	0.003**	0.002**				
	(0.001)	(0.001)				
MW × WW Index			-0.044**	-0.040**		
			(0.019)	(0.019)		
MW × SA Index					-0.004**	-0.004***
					(0.002)	(0.001)
Establishment FE	✓	✓	✓	✓	✓	✓
Year × Firm FE	✓	✓	✓	✓	✓	✓
Pair Counties FE	✓		✓		✓	
Pair Counties × Year FE		✓		✓		✓
Observations	1,616,819	1,614,208	1,550,225	1,547,462	1,637,757	1,635,170
Adjusted R-squared	0.940	0.940	0.941	0.941	0.940	0.940

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. Furthermore, we alternatively include pair counties fixed effects and pair counties × year fixed effects. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their data sources.

First difference model. We estimate Equation (4) and report the results in Tables OA11. These results are consistent with our baseline specification.

Table OA11: Alternative model - Delta difference

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Δ Employment (log)	Δ Employment (log)	Δ Employment (log)
	Panel A: Interaction with corporate size		
Δ MW	-0.012***	-0.013***	-0.015***
	(0.004)	(0.004)	(0.005)
Δ MW $\times$ Log(Size)	0.001***	0.001**	0.001**
	(0.000)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,083,448	641,769	568,800
R-squared	0.014	0.021	0.021
	Panel B: Interaction with the WW index		
Δ MW	-0.011***	-0.015***	-0.020***
	(0.004)	(0.005)	(0.005)
Δ MW $\times$ WW Index	-0.026**	-0.033***	-0.040***
	(0.010)	(0.011)	(0.012)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,026,657	623,854	552,811
R-squared	0.014	0.020	0.021
	Panel C: Interaction with the SA index		
Δ MW	-0.003***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.002)
Δ MW $\times$ SA Index	-0.002**	-0.002**	-0.002**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	2,083,448	641,769	568,800
R-squared	0.014	0.021	0.021

Notes: This table shows regression results for Equation (4). We use Δ*Employment (log)* as our outcome variable. All the regressions include the establishment and year-fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Alternative clustering of standard errors. Our main specification employs clustering of standard errors at the state level, which we have selected as the appropriate cluster level since it is where the minimum wage treatment is assigned (Abadie et al., 2023; Bertrand et al., 2004). In this paragraph, we show that our results hold when we cluster standard errors at the establishment level, at the firm level, and also at the county level. We report estimation results in Tables OA12, OA13, and OA14.

Table OA12: Alternative clustering approach - Establishment level

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.015***	-0.023**	-0.031***
	(0.005)	(0.009)	(0.010)
MW $\times$ Log(Firm Size)	0.002**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933
	Panel B: Interaction with the WW index		
MW	-0.017***	-0.020**	-0.030***
	(0.005)	(0.008)	(0.009)
MW $\times$ WW Index	-0.036***	-0.053**	-0.066***
	(0.012)	(0.021)	(0.024)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,186,212	661,777	584,829
Adjusted R-squared	0.932	0.934	0.934
	Panel C: Interaction with the SA index		
MW	-0.005**	-0.006*	-0.010***
	(0.002)	(0.003)	(0.004)
MW $\times$ SA Index	-0.002*	-0.005**	-0.005**
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects.We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the establishment level. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. See Table OA1 for a detailed description of the variables and their data sources.

Table OA13: Alternative clustering - Firm level

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.015**	-0.023**	-0.031***
	(0.007)	(0.010)	(0.011)
MW $\times$ Log(Firm Size)	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933
		Panel B: Interaction with the WW index	
MW	-0.017**	-0.020**	-0.030***
	(0.008)	(0.009)	(0.010)
MW $\times$ WW Index	-0.036*	-0.053**	-0.066***
	(0.020)	(0.023)	(0.024)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,186,212	661,777	584,829
Adjusted R-squared	0.932	0.934	0.934
		Panel C: Interaction with the SA index	
MW	-0.005**	-0.006	-0.010**
	(0.002)	(0.004)	(0.004)
MW $\times$ SA Index	-0.002	-0.005*	-0.005*
	(0.002)	(0.002)	(0.003)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year $\times$ firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the firm level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Table OA14: Alternative clustering - County level

	(1) All counties Employment (log)	(2) Counties on borders Employment (log)	(3) Counties on borders (≤ 75 km) Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.015***	-0.023**	-0.031***
	(0.005)	(0.010)	(0.011)
MW × Log(Firm Size)	0.002**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933
	Panel B: Interaction with the WW index		
MW	-0.017***	-0.020**	-0.030***
	(0.005)	(0.008)	(0.009)
MW × WW Index	-0.036***	-0.053***	-0.066***
	(0.013)	(0.020)	(0.024)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,186,212	661,777	584,829
Adjusted R-squared	0.932	0.934	0.934
	Panel C: Interaction with the SA index		
MW	-0.005***	-0.006*	-0.010***
	(0.002)	(0.003)	(0.004)
MW × SA Index	-0.002*	-0.005***	-0.005**
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,862	690,721	610,632
Adjusted R-squared	0.931	0.933	0.933

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the county level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Other corporate characteristics. We present evidence that our primary results hold when we incorporate alternative corporate performance and tangibility measures in our regression analyses. We report our results in Table OA15.

Table OA15: Confounding corporate characteristics

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.018***	-0.023**	-0.031***
	(0.006)	(0.009)	(0.010)
MW $\times$ Log(Firm Size)	0.001**	0.003***	0.003**
	(0.001)	(0.001)	(0.001)
MW $\times$ Tangibility	0.009**	0.002	0.004
	(0.005)	(0.009)	(0.008)
MW $\times$ ROI	-0.002	-0.002	-0.003
	(0.003)	(0.007)	(0.008)
MW $\times$ ROA	0.005	0.024**	0.028**
	(0.006)	(0.010)	(0.011)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,274,684	688,080	608,340
Adjusted R-squared	0.931	0.933	0.933
		Panel B: Interaction with the WW index	
MW	-0.020***	-0.020**	-0.030***
	(0.007)	(0.008)	(0.010)
MW $\times$ WW Index	-0.035**	-0.048**	-0.062***
	(0.016)	(0.018)	(0.023)
MW $\times$ Tangibility	0.010**	0.002	0.004
	(0.005)	(0.009)	(0.009)
MW $\times$ ROI	-0.001	-0.002	-0.003
	(0.003)	(0.007)	(0.009)
MW $\times$ ROA	0.003	0.023**	0.024**
	(0.006)	(0.010)	(0.012)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,178,840	659,676	583,030
Adjusted R-squared	0.932	0.934	0.934

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year $\times$ firm fixed effects. We also control for several major corporate characteristics. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. $*p<0.10$, $**p<0.05$, $***p<0.01$. See Table OA1 for a detailed description of the variables and their data sources.

Table OA15: Confounding corporate characteristics (continued)

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel C: Interaction with the SA index	
MW	-0.008***	-0.006	-0.011**
	(0.003)	(0.004)	(0.004)
MW $\times$ SA Index	-0.002	-0.004**	-0.004*
	(0.001)	(0.002)	(0.002)
MW $\times$ Tangibility	0.009*	0.001	0.003
	(0.005)	(0.009)	(0.009)
MW $\times$ ROI	-0.002	-0.002	-0.003
	(0.003)	(0.007)	(0.008)
MW $\times$ ROA	0.006	0.024**	0.029**
	(0.006)	(0.010)	(0.011)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,274,684	688,080	608,340
Adjusted R-squared	0.931	0.933	0.933

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year $\times$ firm fixed effects. We also control for several major corporate characteristics. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

County characteristics. We demonstrate that our results remain robust even when explicitly controlling for several local economic characteristics. More specifically, we control for the (log of the) county population, income per capita, and unemployment rate and report our results in Table OA16.

Table OA16: County characteristics

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.014**	-0.025***	-0.032***
	(0.006)	(0.009)	(0.011)
MW × Log(Firm Size)	0.002**	0.003***	0.004***
	(0.001)	(0.001)	(0.001)
Income	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Unemployment	0.000	0.001	0.002
	(0.001)	(0.001)	(0.001)
Log(Population)	0.158***	0.124***	0.134***
	(0.024)	(0.037)	(0.043)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,252,915	677,627	597,507
Adjusted R-squared	0.931	0.933	0.933
	Panel B: Interaction with the WW index		
MW	-0.016**	-0.021***	-0.030***
	(0.006)	(0.008)	(0.009)
MW × WW Index	-0.037**	-0.057***	-0.071***
	(0.016)	(0.018)	(0.022)
Income	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Unemployment	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)
Log(Population)	0.154***	0.119***	0.129***
	(0.024)	(0.038)	(0.044)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,156,576	649,319	572,343
Adjusted R-squared	0.932	0.934	0.934

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We also control for several major county characteristics. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. See Table OA1 for a detailed description of the variables and their data sources.

Table OA16: County characteristics (continued)

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel C: Interaction with the SA index	
MW	-0.004**	-0.006*	-0.010***
	(0.002)	(0.003)	(0.004)
MW $\times$ SA Index	-0.002**	-0.005***	-0.005**
	(0.001)	(0.002)	(0.002)
Income	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Unemployment	0.000	0.001	0.002
	(0.001)	(0.001)	(0.001)
Log(Population)	0.158***	0.124***	0.134***
	(0.024)	(0.037)	(0.042)
Establishment FE	✓	✓	✓
Year $\times$ Firm FE	✓	✓	✓
Observations	2,252,915	677,627	597,507
Adjusted R-squared	0.931	0.933	0.933

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year $\times$ firm fixed effects. We also additionally control for several county characteristics. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Considering other types of labor protection. We gather data on the UI benefit schedules of each state from the U.S. Department of Labor's "Significant Provisions of State UI Laws". More specifically, we follow Guo et al. (2024) and consider the overall UI benefit level in a given state and year by multiplying the maximum weekly benefit amount by the maximum duration of benefits provided under each state's regular UI program. Additionally, we collect information on union membership (UMR) rates from the Current Population Survey (CPS). We include controls for UI and UMR in Equation (3). Also, we further introduce an interaction term between these variables and corporate financial constraints. The outcomes are reported in Table OA17.

Table OA17: Minimum Wage, Unemployment Insurance, and Union Membership

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel A: Interaction with corporate size		
MW	-0.016***	-0.025***	-0.031***
	(0.005)	(0.008)	(0.010)
MW × Log(Firm Size)	0.002**	0.003***	0.003***
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Fully interacted UI and UMR	✓	✓	✓
Observations	2,283,862	690,721	610,632
R-squared	0.940	0.943	0.943
	Panel B: Interaction with the WW index		
MW	-0.018***	-0.023***	-0.032***
	(0.006)	(0.007)	(0.009)
MW × WW Index	-0.038**	-0.059***	-0.071***
	(0.015)	(0.018)	(0.023)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Fully interacted UI and UMR	✓	✓	✓
Observations	2,186,212	661,777	584,829
R-squared	0.941	0.944	0.944
	Panel C: Interaction with the SA index		
MW	-0.004***	-0.006**	-0.010***
	(0.002)	(0.003)	(0.003)
MW × SA Index	-0.002	-0.004**	-0.004*
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Fully interacted UI and UMR	✓	✓	✓
Observations	2,283,862	690,721	610,632
R-squared	0.940	0.943	0.943

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include establishment and year × firm fixed effects. We further control for UI policies and union membership rates (UMR) and interact these variables with the alternative proposed measures of corporate financial constraints. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Weights. We demonstrate the robustness of our results when we weight the observations in our sample by establishment size, which we proxy using the natural logarithm of establishment sales. Using these weights enables us to identify average partial effects even in the presence of unmodeled heterogeneity in effects (Solon et al., 2015). The detailed results are reported in Table OA18.

Table OA18: Minimum Wage - Weights

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.013***	-0.022**	-0.030***
	(0.005)	(0.008)	(0.010)
MW × Log(Firm Size)	0.001**	0.003***	0.003**
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,852	690,714	610,625
Adjusted R-squared	0.935	0.936	0.936
		Panel B: Interaction with the WW index	
MW	-0.015**	-0.019**	-0.029***
	(0.006)	(0.007)	(0.009)
MW × WW Index	-0.032**	-0.049***	-0.063***
	(0.015)	(0.018)	(0.022)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,186,203	661,772	584,824
Adjusted R-squared	0.936	0.938	0.938
		Panel C: Interaction with the SA index	
MW	-0.005***	-0.006*	-0.010***
	(0.002)	(0.003)	(0.003)
MW × SA Index	-0.002	-0.005**	-0.005**
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,852	690,714	610,625
Adjusted R-squared	0.935	0.936	0.936

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We weight the observations by the natural logarithm of establishment sales. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Minimum wage and the cost of labor. We collect total payroll information from the County Business Patterns (CBP) to estimate Equation (5). The results are reported in Table OA19. Our findings demonstrate that minimum wage increases lead to an increase in total payroll per employee by over $900.

Table OA19: Wages

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Wage	Wage	Wage
MW	704.152***	764.570***	929.820***
	(157.282)	(209.026)	(322.189)
County FE	✓	✓	✓
Year × Industry FE	✓	✓	✓
Observations	7,374,781	2,521,090	2,203,509
Adjusted R-squared	0.363	0.453	0.460

Notes: This table shows regression results for Equation (5). We use *Wage* as our outcome variable. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Extensive margin. We analyze the effect of changes in minimum wage policies and financial frictions on the number of establishments. To do so, we estimate Equation (6) and report the results in Table OA20.

Table OA20: Extensive margin

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Establishments (log)	Establishments (log)	Establishments (log)
	Panel A: Interaction with corporate size		
MW	-0.032**	-0.014	-0.013
	(0.014)	(0.011)	(0.011)
MW × Log(Firm Size)	0.005**	0.003*	0.002*
	(0.002)	(0.002)	(0.001)
County FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,348,843	429,731	381,424
Adjusted R-squared	0.304	0.273	0.272
	Panel B: Interaction with the WW index		
MW	0.008**	-0.009	-0.010
	(0.004)	(0.008)	(0.008)
MW × WW Index	-0.001	-0.043*	-0.040*
	(0.001)	(0.024)	(0.022)
County FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,290,814	412,308	365,941
Adjusted R-squared	0.305	0.274	0.274
	Panel C: Interaction with the SA index		
MW	0.000	0.003	0.003
	(0.002)	(0.003)	(0.004)
MW × SA Index	-0.007*	-0.004*	-0.003
	(0.004)	(0.003)	(0.003)
County FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,348,843	429,731	381,424
Adjusted R-squared	0.304	0.272	0.272

Notes: This table shows regression results for Equation (6). We use *Establishments (log)* as our outcome variable. All the regressions include the county and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Establishments' performance. We investigate the effect of a rise in the minimum wage on establishments' sales and how corporate financial frictions affect this relationship. To do so, we use as outcome variable the natural logarithm of establishments' total sales in Equation (2) and Equation (3) and report the results in Tables OA21 and OA22.

Table OA21: Difference-in-differences - Sales

	(1) All counties	(2) Counties on borders	(3) Counties on borders ($\leq 75km$)
	Sales (log)	Sales (log)	Sales (log)
MW	-0.002	0.001	-0.003
	(0.002)	(0.002)	(0.004)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,340,491	707,706	625,672
Adjusted R-squared	0.940	0.941	0.941

Notes: This table shows regression results for Equation (2). We use *Sales (log)* as our outcome variable.We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

Table OA22: Minimum wages - Sales

	(1) All counties Sales (log)	(2) Counties on borders Sales (log)	(3) Counties on borders (≤ 75 km) Sales (log)
		Panel A: Interaction with corporate size	
MW	-0.019***	-0.026***	-0.037***
	(0.005)	(0.009)	(0.011)
MW × Log(Firm Size)	0.002**	0.003***	0.004***
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,852	690,714	610,625
Adjusted R-squared	0.940	0.942	0.942
		Panel B: Interaction with the WW index	
MW	-0.020***	-0.022**	-0.034***
	(0.006)	(0.009)	(0.010)
MW × WW Index	-0.042**	-0.056***	-0.074***
	(0.017)	(0.021)	(0.026)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,186,203	661,772	584,824
Adjusted R-squared	0.941	0.943	0.943
		Panel C: Interaction with the SA index	
MW	-0.007***	-0.008**	-0.014***
	(0.002)	(0.003)	(0.004)
MW × SA Index	-0.003**	-0.006***	-0.006**
	(0.001)	(0.002)	(0.003)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	2,283,852	690,714	610,625
Adjusted R-squared	0.940	0.942	0.942

Notes: This table shows regression results for Equation (3). We use *Sales (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

The role of market power. We measure the market power within an industry using the Lerner index. Subsequently, we estimate Equation (3), focusing on the sample of firms with greater market power, which we define as establishments whose Lerner index value is above the median in our sample. Estimation results in Table OA23 are consistent with our baseline findings and do not support the alternative hypothesis that establishments in industries where transferring cost increases to consumers is easier can then alleviate the adverse effects of financial constraints on employment following an increase in the minimum wage rate.

Table OA23: Minimum wages - High market power

	(1) All counties	(2) Counties on borders	(3) Counties on borders($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.021***	-0.030***	-0.042***
	(0.007)	(0.009)	(0.010)
MW × Log(Firm Size)	0.002***	0.004***	0.005***
	(0.001)	(0.001)	(0.001)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,134,847	345,314	303,167
Adjusted R-squared	0.936	0.940	0.940
		Panel B: Interaction with the WW index	
MW	-0.023***	-0.025***	-0.038***
	(0.009)	(0.009)	(0.010)
MW × WW Index	-0.057**	-0.063***	-0.084***
	(0.022)	(0.021)	(0.023)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,089,565	331,389	290,880
Adjusted R-squared	0.937	0.941	0.941
		Panel C: Interaction with the SA index	
MW	-0.004***	-0.008**	-0.013***
	(0.002)	(0.004)	(0.004)
MW × SA Index	-0.003**	-0.006**	-0.006***
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	1,134,847	345,314	303,167
Adjusted R-squared	0.936	0.940	0.940

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We restrict our sample to establishments that operate in industries with high market power. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of the variables and their data sources.

The role of local banking markets. We explore the potential significance of local banking market conditions in our setting. To do so, we introduce an exogenous shock to local banking market conditions, that is the banking deregulation wave after the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994. This event allowed states to gradually remove interstate bank branching restrictions, thereby increasing banking market competition, efficiency, and ultimately credit supply (e.g., Favara and Imbs, 2015; Krishnan et al., 2015; Berger et al., 2021).

We report more information on the timing of the deregulation across states, the set of regulations adopted, and the related index score we use in our analysis in the Table OA24. Next, we limit our analysis to the spanning period 1994-2005 (the deregulation period), and introduce and fully interact the deregulation index with the minimum wage level and corporate financial constraints in Equation (3). We report the results in Table OA25.

Table OA24: Deregulation intensity (Rice and Strahan, 2010)

State	Branching Index	EÔ¨Äective Year	Minimum Age	De-novo Interstate Branching	Acquisition Single Branch	Deposit Cap
Alabama	1	1997	5 years	No	No	0,3
Alaska	2	1994	3 years	No	Yes	0,5
Arizona	2	2001	5 years	No	Yes	0,3
Arizona	1	1996	5 years	No	No	0,3
Arkansas	0	1997	5 years	No	No	0,25
California	1	1995	5 years	No	No	0,3
Colorado	0	1997	5 years	No	No	0,25
Connecticut	3	1995	5 years	Yes	Yes	0,3
Delaware	1	1995	5 years	No	No	0,3
DC	4	1996	No	Yes	Yes	0,3
Florida	1	1997	3 years	No	No	0,3
Georgia	1	2002	3 years	No	No	0,3
Georgia	1	1997	5 years	No	No	0,3
Hawaii	4	2001	No	Yes	Yes	0,3
Hawaii	1	1997	5 years	No	No	0,3
Idaho	1	1995	5 years	No	No	None
Illinois	1	1997	5 years	No	No	0,3
Indiana	3	1998	5 years	Yes	Yes	0,3
Indiana	4	1997	No	Yes	Yes	0,3
Iowa	0	1996	5 years	No	No	0,15
Kansas	0	1995	5 years	No	No	0,15
Kentucky	1	2004	No	No	No	0,15
Kentucky	1	2000	No	No	No	0,15
Kentucky	0	1997	5 years	No	No	0,15
Louisiana	1	1997	5 years	No	No	0,3
Maine	4	1997	No	Yes	Yes	0,3
Maryland	4	1995	No	Yes	Yes	0,3
Massachusetts	3	1996	3 years	Yes	Yes	0,3
Michigan	4	1995	No	Yes	Yes	None
Minnesota	1	1997	5 years	No	No	0,3
Mississippi	0	1997	5 years	No	No	0,25
Missouri	0	1995	5 years	No	No	0,13
Montana	0	2001	5 years	No	No	0,22
Nevada	1	1995	5 years	Limited	Limited	0,3
New Hampshire	4	2002	No	Yes	Yes	0,3
New Hampshire	3	2000	5 years	Yes	Yes	0,3
New Hampshire	0	1997	5 years	No	No	0,2
New Jersey	3	1996	No	No	Yes	0,3
New Mexico	1	1996	5 years	No	No	0,4
New York	2	1997	5 years	No	Yes	0,3
North Carolina	4	1995	No	Yes	Yes	0,3
North Dakota	3	2003	No	Yes	Yes	0,25
North Dakota	1	1997	No	No	No	0,25
Ohio	4	1997	No	Yes	Yes	0,3
Oklahoma	3	2000	No	Yes	Yes	0,2
Oklahoma	0	1997	5 years	No	No	0,15
Oregon	1	1997	3 years	No	No	0,3
Pennsylvania	4	1995	No	Yes	Yes	0,3
Rhode Island	4	1995	No	Yes	Yes	0,3
South Carolina	1	1996	5 years	No	No	0,3
South Dakota	1	1996	5 years	No	No	0,3
Tennessee	3	2003	3 years	Yes	Yes	0,3
Tennessee	3	2001	5 years	Yes	Yes	0,3
Tennessee	2	1998	5 years	No	Yes	0,3
Tennessee	1	1997	5 years	No	No	0,3
Texas	2	1999	No	Yes	Yes	0,2
Utah	3	2001	5 years	Yes	Yes	0,3
Utah	2	1995	5 years	No	Yes	0,3
Vermont	4	2001	No	Yes	Yes	0,3
Vermont	2	1996	5 years	No	Yes	0,3
Virginia	4	1995	No	Yes	Yes	0,3
Washington	1	1996	5 years	No	No	0,3
West Virginia	3	1997	No	Yes	Yes	0,25
Wisconsin	1	1996	5 years	No	No	0,3
Wyoming	1	1997	3 years	No	No	0,3

Notes: This table shows for each state the timing of the deregulation, the set of regulations adopted, and the related index score we use in our analysis.

Table OA25: Deregulation events

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with the Size	
MW	-0.024	-0.068***	-0.068**
	(0.016)	(0.023)	(0.028)
MW × Log(Size)	0.002	0.008***	0.006
	(0.002)	(0.003)	(0.004)
Deregulation	-0.059	-0.151**	-0.146*
	(0.046)	(0.072)	(0.075)
MW × Deregulation	0.011	0.030**	0.030*
	(0.009)	(0.014)	(0.015)
Log(Size) × Deregulation	0.008	0.019**	0.016*
	(0.006)	(0.007)	(0.008)
MW × Deregulation × Log(Size)	-0.002	-0.004***	-0.003**
	(0.001)	(0.001)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	934,586	277,197	248,250
R-squared	0.951	0.953	0.953
		Panel B: Interaction with the WW Index	
MW	-0.027**	-0.066***	-0.063***
	(0.012)	(0.016)	(0.020)
MW × WW Index	-0.056	-0.157***	-0.114**
	(0.036)	(0.036)	(0.056)
Deregulation	-0.032	-0.116**	-0.109**
	(0.046)	(0.051)	(0.054)
MW × Deregulation	0.006	0.024**	0.024**
	(0.009)	(0.010)	(0.011)
WW Index × Deregulation	-0.101	-0.293***	-0.236**
	(0.125)	(0.095)	(0.115)
MW × WW Index × Deregulation	0.019	0.062***	0.052**
	(0.024)	(0.018)	(0.022)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	905,087	268,565	240,484
Adjusted R-squared	0.951	0.953	0.953

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We also introduce Deregulation and fully interact this variable with MW and corporate financial frictions. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Table OA25: Deregulation events (continued)

	(1) All counties	(2) Counties on borders	(3) Counties on borders (≤ 75 km)
	Employment (log)	Employment (log)	Employment (log)
	Panel C: Interaction with the SA index		
MW	-0.007	-0.015*	-0.025***
	(0.005)	(0.008)	(0.009)
MW × SA Index	-0.001	-0.007	-0.005
	(0.003)	(0.004)	(0.005)
Deregulation	-0.008	-0.023	-0.035
	(0.015)	(0.023)	(0.023)
MW × Deregulation	0.001	0.005	0.007
	(0.003)	(0.005)	(0.005)
SA Index × Deregulation	-0.010	-0.017**	-0.015*
	(0.007)	(0.008)	(0.009)
MW × SA Index × Deregulation	0.002	0.003**	0.003*
	(0.001)	(0.002)	(0.002)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	934,586	277,197	248,250
R-squared	0.951	0.953	0.953

Notes: This table shows regression results for Equation (3). We use *Employment (log)* as our outcome variable. We also introduce Deregulation and fully interact this variable with MW and corporate financial frictions. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

County level analysis. To account for general equilibrium effects in our empirical analysis, we adopt the methodology outlined by Giroud and Mueller (2017) by employing county-level regressions, as reported in Equation (7). We measure county-level (j) employment as the total employment of all the establishments within a county in our sample. The financial constraint measures at the county level are the employment-weighted average value derived from three alternative measures of financial constraints across all establishments in our sample within that county. The findings in Table OA26 echo our baseline results, revealing that counties with greater exposure to establishments with a high level of financial constraints face greater aggregate employment decline after a rise in the minimum wage.

Table OA26: County level regression

	(1) All counties	(2) Counties on state borders	(3) Counties on state borders ($\leq$ 75 km)
	Employment (log)	Employment (log)	Employment (log)
		Panel A: Interaction with corporate size	
MW	-0.265***	-0.272***	-0.264***
	(0.042)	(0.061)	(0.070)
Log(Size)	-0.128***	-0.122***	-0.123**
	(0.028)	(0.043)	(0.047)
MW × Log(Firm Size)	0.030***	0.034***	0.033***
	(0.004)	(0.006)	(0.007)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	79,112	28,323	24,741
Adjusted R-squared	0.897	0.892	0.893
		Panel B: Interaction with the WW index	
MW	-0.217***	-0.215***	-0.216***
	(0.040)	(0.058)	(0.065)
WW Index	2.134***	2.150***	2.334***
	(0.439)	(0.691)	(0.743)
MW × WW Index	-0.507***	-0.570***	-0.577***
	(0.066)	(0.114)	(0.126)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	77,967	27,874	24,365
Adjusted R-squared	0.900	0.895	0.897
		Panel C: Interaction with the SA index	
MW	-0.029	-0.006	-0.009
	(0.028)	(0.030)	(0.034)
SA Index	0.149***	0.136**	0.138*
	(0.041)	(0.061)	(0.069)
MW × SA Index	-0.033***	-0.036***	-0.037***
	(0.009)	(0.011)	(0.012)
County FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	79,112	28,323	24,741
Adjusted R-squared	0.896	0.890	0.891

Notes: This table shows regression results for Equation (7). We use *Employment (log)* as our outcome variable. All the regressions include the county and year fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their sources.

Employment measurement issues in Compustat. We exclude multinational firms from the sample. We classified a firm as multinational if it reported non-zero foreign income in the previous three years. Alternatively, we defined a firm as multinational if at least 5% of its sales were from outside the home country (according to the Compustat Geographic Segment database) (Nimier-David et al., 2023). We report in these tables results consistent with our primary evidence in the main text.

Table OA27: Firm level analysis - Employment - Excluding multinational considering the foreign income

	(1) Employment (log)	(2) Employment (log)	(3) Employment (log)
MW Exposure	-0.030**	-0.015	0.004
	(0.014)	(0.013)	(0.015)
Log(Firm Size)	0.196***		
	(0.016)		
MW Exposure × Log(Firm Size)	0.006**		
	(0.003)		
WW Index		-0.741***	
		(0.182)	
MW Exposure × WW Index		-0.077**	
		(0.034)	
SA Index			-0.125***
			(0.017)
MW Exposure × SA Index			-0.007**
			(0.003)
Firm FE	✓	✓	✓
Year × Industry FE	✓	✓	✓
Observations	34,570	30,840	34,570
Adjusted R-squared.	0.954	0.945	0.947

Notes: This table shows regression results for Equation (9). We use *Employment (log)* as our outcome variable. All the regressions include firm and year × industry fixed effects. We exclude from the sample firms that report non-zero foreign income in the previous three years. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their data sources.

Table OA28: Firm level analysis - Employment - Excluding multinational considering sales from foreign countries

	(1) Employment (log)	(2) Employment (log)	(3) Employment (log)
MW Exposure	-0.068***	-0.041**	-0.001
	(0.017)	(0.020)	(0.018)
Log(Firm Size)	0.203***		
	(0.016)		
MW Exposure × Log(Firm Size)	0.010***		
	(0.002)		
WW Index		-0.772***	
		(0.180)	
MW Exposure × WW Index		-0.121***	
		(0.039)	
SA Index			-0.107***
			(0.017)
MW Exposure × SA Index			-0.017***
			(0.003)
Firm FE	✓	✓	✓
Year × Industry FE	✓	✓	✓
Observations	25,039	22,439	25,039
Adjusted R-sq.	0.957	0.947	0.951

Notes: This table shows regression results for Equation (9). We use *Employment (log)* as our outcome variable. All the regressions include firm and year × industry fixed effects. e exclude from the sample firms that report at least 5% of their sales from outside the home country. Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of our variables and their data sources.

The financial crisis sample. We report the summary statistics for the financial crisis sample in Table OA29. We limit the spanning period of analysis to the period 2003-2011.

Table OA29: Descriptive statistics - Financial crisis sample

	(1) Count	(2) Mean	(3) SD	(4) p25	(5) p50	(6) p75
Employment	714,091	49.5111	92.3823	5.0000	15.0000	45.0000
Log(Employment)	714,091	2.9300	1.3470	1.7918	2.7726	3.8286
Bound	714,091	0.3436	0.4749	0.0000	0.0000	1.0000
Frictions (short) (%)	611,520	1.1970	2.2347	0.0089	0.2995	1.3910
Frictions (long) (%)	553,573	10.6749	15.6377	0.3172	3.4949	13.7329

Notes: This table shows descriptive statistics of our financial crisis analysis. See Table OA1 for a detailed description of the variables and their data sources.

Firm level employment, the financial crisis, and financial constraints. Our results hold when we estimate Equation (13) using firm-level information. In this setting, *Treated* is measured as the share of employees located in states affected by the 2006 federal minimum wage increase, as of 2006. We report the results in Table OA30.

Table OA30: Firm employment during the crisis and constraints

	(1) Employment (log)	(2) Employment (log)
Post × Treated	0.003	-0.003
	(0.024)	(0.023)
Post × Constraints (short)	0.007	0.026
	(0.075)	(0.071)
Post × Treated × Constraints (short)	-0.736*	-0.766**
	(0.375)	(0.368)
Firm FE	✓	✓
Year FE	✓	Subsumed
Year × Industry FE		✓
Observations	12,964	12,914
Adjusted R-squared	0.974	0.975

Notes: This table shows regression results for Equation (13). We use *Employment (log)* as our outcome variable. All the regressions include firm and year fixed effects; year × industry fixed effects are included in Column (2). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. **p* <0.10, ***p* <0.05, ****p* <0.01. See Table OA1 for a detailed description of these variables and their data sources.

Placebo test. We investigate the impact of long-term debt maturing after the financial crisis, denoted as *Constraints (long)*. This variable is calculated as the sum of long-term debt due in two, three, four, and five years relative to 2007, adjusted by total sales and expressed as a percentage by multiplying it by 100. The coefficients from Equation (13) are reported in Table OA31.

Table OA31: Placebo test

	(1) All counties Employment (log)	(2) Counties on borders Employment (log)	(3) Counties on borders (≤ 75 km) Employment (log)
Post × Bound	-0.003	-0.014**	-0.011
	(0.005)	(0.007)	(0.007)
Post × Bound × Constraints (long)	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Establishment FE	✓	✓	✓
Year × Firm FE	✓	✓	✓
Observations	553,573	168,518	150,449
Adjusted R-squared	0.958	0.959	0.960

Notes: This table shows regression results for Equation (13). We use *Employment (log)* as our outcome variable. All the regressions include the establishment and year × firm fixed effects. We focus on three alternative samples: (i) all counties in the United States in Column (1), (ii) all counties on state borders in Column (2), and (iii) all counties on state borders whose centroids are less than 75 km apart in Column (3). Robust standard errors, reported in parentheses below the coefficient estimates, are clustered at the state level. *p <0.10, **p <0.05, ***p <0.01. See Table OA1 for a detailed description of these variables and their data sources.

Weights used in the counterfactual analysis. The size categories of the establishments we consider in our analysis are grouped in intervals as follows: less than 1-9 employees, 10-19 employees, 20-29 employees, 30-39 employees, 40-49 employees, up to more than 100 employees. To calculate the weights, we consider the employment shares of these establishments in 2006 from the entire NETS database. We present the weights we utilize in Table OA32.

Table OA32: Weights

Employment size category	Weights
1-9 Employees	0.25
10-19 Employees	0.09
20-29 Employees	0.06
30-39 Employees	0.05
40-49 Employees	0.04
50-59 Employees	0.04
60-69 Employees	0.03
70-79 Employees	0.02
80-89 Employees	0.02
90-99 Employees	0.01
More than 100	0.38

Notes: The table shows the weights per employment category that we use for our counterfactual analysis.

OA3 Matching establishment, minimum wage, and corporate financial friction information

The 2020 version of the National Establishments Time Series (NETS) provides the legal business names of establishments, along with the names of the primary domestic firms within a hierarchical "Family Tree" of companies. To ensure the precise inclusion of relevant balance sheet attributes from the individual firms themselves, rather than their parent holding companies, we employ the legal business names of these establishments for cross-referencing with the Compustat database in our empirical analysis.

We utilize fuzzy matching, employing a similar threshold of 90%, to align company names between the two databases. Subsequently, we conduct manual verification to ensure the precision of these matches. With this methodology, we successfully merge 353,818 establishments. It results in a dataset of 4,231,721 establishment-year observations, each containing no missing information on employee count and geographical location.

We integrate this database with historical minimum wage data sourced from Vaghul and Zipperer (2021), leading to the exclusion of 6,348 establishment-year records. Among these are public corporation establishments located in Puerto Rico or the Virgin Islands, for which minimum wage information is unavailable. Subsequently, we merged the database with corporate balance sheet attributes sourced from Compustat, resulting in a dataset comprising 2,819,629 observations.

To refine the dataset, we remove establishments falling within the financial and utility sectors (SIC codes 60 and 49), eliminating 312,309 and 95,526 observations respectively. Furthermore, we employ the *reghfe* command from Correia (2016) to eliminate singleton observations, reducing the sample by an additional 62,291 observations.

Our final database is composed of 2,340,503 establishment-year observations, 231,552 establishments, and 5,615 companies from 1990 to 2020.

OA4 Conceptual Framework

To better frame our research hypothesis, we develop a simple model where firms maximize profits subject to a production function $Y_i = A_i F(L_i)$, where Y_i is the output, A_i is the firm specific productivity, and L_i is labor input. The average employee wage is denoted by w_{ave}.[22]

For unconstrained firms, the profit maximization problem is captured by Equation (17):

$$\max_{L_i} \pi_i = pA_iF(L_i) - w_{ave}L_i \tag{17}$$

The first-order condition (FOC) is given by Equation (18):

$$pA_iF_L(L_i) = w_{ave} \tag{18}$$

A minimum wage law increases the average wage of firm i from w_{ave} to w'_{ave}. Therefore, the standard model predicts a decrease in employment. However, within this framework, it is important to note that employment could be unaffected if, for example, firm productivity or output prices increase sufficiently, that is whether $\frac{A'_i}{A_i} > \frac{w'_{ave}}{w_{ave}}$ or $\frac{p'}{p} > \frac{w'_{ave}}{w_{ave}}$. These two solutions could, for example, be attributed to efficiency wage effects ($Y_i = A_i(w_{ave})F(L_i)$, where $A_i(w_{ave})$ is increasing in w_{ave}) or aggregate demand effects ($p = p(w_{ave})$, where $\frac{dp}{dw_{ave}} > 0$).

We next introduce financial constraints in the model by recognizing that a firm's ability to pay employee salaries is limited by its available financial resources and profits:

$$w_{ave}L_i \leq B_i + \pi_i \tag{19}$$

where B_i represents a firm's internal financial resources. This constraint highlights the crucial role internal resources play in determining a firm's ability to pay employee wages.

For financially constrained firms, the optimization problem becomes:

[22] The use of capital in the production function and any potential substitution effect of labor with capital are intentionally not considered to maintain model simplicity. However, including capital does not affect our conclusions.

$$\max_{L_i} \pi_i = pA_iF(L_i) - w_{ave}L_i \tag{20}$$

subject to $w_{ave}L_i \leq B_i + \pi_i$.

To solve this constrained optimization problem, we specify the following Lagrangian:

$$\mathcal{L} = pA_iF(L_i) - w_{ave}L_i + \lambda(B_i + pA_iF(L_i) - w_{ave}L_i - w_{ave}L_i) \tag{21}$$

where $\lambda \geq 0$ is the Lagrange multiplier, which in this setting represents the shadow cost of the financial constraint.

The FOC given a firm's financial constraint is reported in Equation (22):

$$\frac{\partial \mathcal{L}}{\partial L_i} = pA_iF_L(L_i)(1+\lambda) - w_{ave}(1+2\lambda) = 0 \tag{22}$$

Rearranging the FOC, we obtain the following Equation:

$$pA_iF_L(L_i) = w_{ave}\frac{1+2\lambda}{1+\lambda} \tag{23}$$

This condition shows that constrained firms behave as if they face a higher effective wage rate of $w_{ave}\frac{1+2\lambda}{1+\lambda}$, which exceeds w_{ave} for any positive λ. The shadow cost λ, therefore, captures the additional value of relaxing the financial constraint.

When the minimum wage increases the average wage from w_{ave} to w'_{ave}, it directly affects the firm's internal resources. For this reason, the constraint is adjusted to:

$$w'_{ave}L_i \leq B_i + \pi_i - \Delta_i \tag{24}$$

$\Delta_i = (w'_{ave} - w_{ave})L_i$ represents the reduction in internal resources due to the wage increase. This shows how the minimum wage increase reduces a firm's internal resources, potentially tightening its financial constraints.

For constrained firms, this leads to a FOC:

$$pA_iF_L(L_i) = w'_{ave}\frac{1+2\lambda'}{1+\lambda'} \tag{25}$$

where λ' is generally going to be larger than the original λ due to the tighter financial constraint, further increasing the effective wage rate faced by the firm.

This simple model suggests that unconstrained firms can adjust their employment levels, following minimum wage increases. However, financially constrained firms face an additional effective cost due to their financial constraint, captured by the shadow cost λ. Reducing internal resources (Δ_i) further tightens the operating constraint of financially constrained firms, potentially exacerbating their employment reduction by increasing λ.

Our empirical finding of no average effect on employment can still be reconciled through the mechanisms outlined in the original model. The key distinction is that financially constrained firms may exhibit stronger negative responses due to the added pressure exerted by their financial constraints, as captured by the shadow cost λ, leading to a potentially larger firm response following a minimum wage hike.