Do CEOs Benefit from Employee Pay Raises? Evidence from a Federal Minimum Wage Law*

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Abstract

Using a U.S. federal minimum wage hike as a natural experiment, I analyze the spillover effect of employee pay on CEO pay. I employ a triple-differences empirical strategy that exploits the distribution of workers across states. After an about 40% hike in the federal minimum wage, a 10% increase in employment share in states bound by federal minimum wage leads to an about 7% increase in CEO total pay for firms in minimum-wage-sensitive industries relative to other industries. The CEO pay increase is more pronounced for smaller firms and firms with lower CEO-employee pay ratios, consistent with the relative proximity between smaller firm CEOs and rank-and-file employees in terms of responsibilities in the workplace. Moreover, younger CEOs experience an about 2.5 times larger increase in pay than older CEOs. The results are consistent with CEOs demanding a pay increase following an exogenous employee pay raise and robust to using a sample of firms matched by observable characteristics and a sample of firms headquartered in counties located along borders of contiguous states. The results are inconsistent with the efficiency wages or the CEO bargaining power driving the CEO pay increases. Additionally, the estimate of the elasticity of CEO pay with respect to minimum wages implies that the federal minimum wage hike leads to an about 1.8% increase in median smaller firm CEO pay, providing important policy implications.

Key words: executive compensation, employee pay, minimum wages, fairness

JEL classification: G34, J31, J38, D63

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1. Introduction

The rising disparity between CEO and worker pay over the past few decades has been attracting considerable attention from academics (e.g., Edmans, Gabaix, and Jenter (2017), Murphy (2013), Frydman and Jenter (2010), Core, Guay, and Larcker (2003)). Figure 1 shows that over the last two decades, CEO to median employee pay ratio has been increasing for small (S&P SmallCap 600), medium (S&P MidCap 400), and large (S&P 500) firms. The literature names the managerial rent extraction (Bebchuk and Fried (2003)), the rare talent of CEOs (Gabaix and Landier (2008)), and CEO strategic choice of peer group firms (Faulkender and Yang (2010)) among the plausible explanations of the widening CEO-employee pay gap. This issue is widely discussed in the media (e.g., Gelles (2021), Saul (2022)), and the politicians refer to it as "a national disgrace"[1] and attempt to address it by curbing CEO pay through tax policies[2] and promoting binding say-on-pay votes.

At the same time, the question of whether CEO pay responds to changes in worker pay has not been addressed in the literature.[3] Understanding the connection between CEO pay and worker pay is important because it allows for evaluating the efficacy of measures aimed at reducing the CEO-employee pay gap. In this paper, I aim to shed light on one such measure: a mandated increase in worker pay. For example, if the elasticity of CEO pay with respect to employee pay is less than one, then increasing employee pay will result in a drop in the CEO-employee pay ratio, which may be desirable; otherwise, this policy will only exacerbate the pay disparity. I attempt to fill the gap and investigate how shocks to employee pay affect CEO pay and also try to identify the underlying mechanism.

CEO pay could potentially change in three directions following an employee pay raise. An increase in CEO pay will be consistent with several hypotheses, including the fairness hypothesis of executive compensation (Edmans, Gosling, and Jenter (2021), Chaigneau, Edmans, and Gottlieb (2022)) and the efficiency wage hypothesis (Yellen (1984), Akerlof and Yellen (1986)). The fairness hypothesis states that CEOs assess whether their pay is "fair" with respect to a set of reference points, including employee pay, and request a pay increase when employees receive an unexpected pay raise. According to the hypothesis, if the board does not grant this request, CEO motivation will be undermined, which will adversely affect firm value. Alternatively, the efficiency wage hypothesis posits that a pay raise will incentivize employees to be more productive. It follows that CEOs will receive larger pay due to enhanced firm performance.

Conversely, a decrease in CEO pay will be consistent with the idea that the firm treats a

[1] Source: https://www.sanders.senate.gov/press-releases/news-sanders-and-colleagues-introduce-legislation- to-combat-corporate-greed-and-end-outrageous-ceo-pay/.

[2] For example, the Tax Excessive CEO Pay Act introduced in the Senate in March 2021 aims to impose increased corporate income tax rates on firms with CEO-employee pay ratio values exceeding 50. The text of the Act is available here: https://www.congress.gov/bill/117th-congress/senate-bill/794/.

[3] Theoretically, rent-sharing models (e.g., Christofides and Oswald (1992), Abowd and Lemieux (1993)) predict that productivity shocks should be shared with employees, and the pay-for-luck literature (e.g., Bertrand and Mullainathan (2001)) documents that CEOs are rewarded for "lucks."

worker's pay raise as an extra unplanned wage expense mandated by the government. This expense reduces firm profits, leaving a smaller "piece of the pie" for the CEO (Draca, Machin, and Van Reenen (2011), Harasztosi and Lindner (2019)). Finally, no change in CEO pay will be consistent with CEO pay being unrelated to employee pay (e.g., because CEOs and employees operate in different labor markets). However, it is empirically challenging to test these hypotheses and establish the direction of causality because employee and CEO pay are endogenously determined (e.g., affected by common factors).

This paper provides novel evidence on the causal effect of changes in employee pay on CEO pay using an increase in the federal minimum wage (MW) in the U.S. as a natural experiment. The analysis exploits the Fair Minimum Wage Act of 2007, which raised the federal MW from \$5.15 to \$5.85, \$6.55, and \$7.25 per hour each year from July 2007. Notably, the law triggered a significant overall increase in the federal MW of about 40%, and it is the only federal MW law that took place post the early 2000s. An increase in MW leads to an increase in pay for rank-and-file employees who are paid at or below MW without any effect on CEO pay. Importantly, a federal MW hike does not depend on the performance of a particular firm, which alleviates the endogeneity concern. Thus, the introduction of a MW law signed on May 25, 2007, provides an ideal opportunity to estimate the effect of employee pay on CEO pay.

To identify the link between employee pay and CEO pay, I employ a triple-differences (DDD) empirical strategy that exploits the distribution of workers across states. Since an employee has the legal right to receive the larger of state and federal MWs, the law impacts only employees in states where the initial state MW was lower than the federal MW (referred to as "bound states"). Therefore, I compare changes in CEO pay across firms with different proportions of employees working at establishments located in bound states (first difference). The second difference compares CEO pay before and after the MW hike. Importantly, the law only affects firms that rely largely on MW labor, referred to as "affected firms" and defined as those in the leisure and hospitality and retail trade industries (e.g., grocery stores, hotels, and restaurants).[4] Hence, the third difference compares changes in CEO pay across affected versus unaffected firms. To summarize, the DDD strategy estimates changes in CEO pay after the MW hike between firms with different employment shares in bound states and with different exposure to MW labor.

I start by verifying important presumptions for my analysis. I show that the MW law indeed raises employee pay: a 10-percentage point increase in the share of employees in bound states leads to a 42% increase in the average employee wage for affected vs. unaffected firms after the MW law. I also show that employment drops by 44% and that the parallel pre-trends assumption holds, which is crucial for the DDD validity (Olden and Møen (2022)).

Analyzing changes in CEO pay around the MW law effective date, I find that a 10-percentage point increase in the share of employees in bound states leads to a 6.5% increase in CEO total pay

[4] Section 2 presents a detailed discussion of the definitions of affected firms and bound states.

for affected vs. unaffected firms post-MW law. Both salary and incentive pay increase, consistent with the permanent nature of salary (Murphy (2013)) and with CEOs demanding incentive pay raises since they boost CEO reputation (Edmans et al. (2021)).[5] Moreover, the CEO and employee pay increases are more pronounced for smaller firms, consistent with the relative proximity between small firm CEOs and rank-and-file employees in terms of responsibilities in the workplace. It is well-documented that smaller firms adopt less formal human resource management practices (e.g., Wilkinson (1999)) and are less hierarchical (e.g., Kotey and Slade (2005)), so CEOs and employees should play more similar roles in smaller firms.

This weaker hierarchy between CEOs and employees should also be reflected in smaller wage differentials since employees require less supervision from the CEOs (e.g., Calvo and Wellisz (1979)). Therefore, if my story based on the connection between CEO and employees is true, a MW hike should lead to more significant pay increases for CEOs of firms with smaller CEO-employee pay ratios. I confirm the previous finding: in affected vs. unaffected firms with smaller CEO-employee pay ratios, a 10-percentage point increase in the share of employees located in bound states results in a 7.2% increase in CEO pay. This evidence supports my conjecture that the similarity of CEO and employee functions and, thus, pay is a primary driver of the CEO pay raise in smaller firms following the MW hike.

My results are also robust to using different samples – a matched sample based on observable firm characteristics and a sample of bound/unbound firms headquartered in counties belonging to the borders of contiguous states (e.g., Dube, Lester, and Reich (2010), Pence (2006), Holmes (1998)). These robustness checks alleviate the concerns that firm characteristics or different economic conditions across regions may drive the results.

Having established a robust result that employee pay raises lead to CEO pay raises, I confirm that a price floor for workers can narrow the pay gap between CEOs and employees. I provide evidence that an increase in employment share in bound states leads to a drop in the CEO-employee pay ratio for smaller, affected vs. unaffected firms after the MW law. However, to effectively implement the price floor policy through the MW hikes, it is also important to understand the underlying mechanism for the link between employee and CEO pay.

I consider several competing mechanisms for the CEO pay increase I find. The first mechanism is based on the fairness hypothesis, which suggests that younger CEOs should receive a larger increase in pay than older CEOs following the MW shock. This is because younger CEOs should care more about being compensated fairly: they face longer horizons, and for them, current pay affects the present value of future pay more by signaling their ability in the executive labor market (Boschen and Smith (1995)), for example, due to career concerns (Gibbons and Murphy (1992)) or risk aversion (Edmans, Gabaix, Sadzik, and Sannikov (2012)).[6] Consistent with this logic, I find

[5] Since incentive pay awards have to be approved by the board and presented to shareholders, increases in incentive pay improve CEO reputation in the executive labor market (Edmans et al. (2021)).

[6] Related, Francis, Huang, Rajgopal, and Zang (2008) document a positive correlation between CEO age and

that in smaller firms, younger CEOs experience an about 2.5 times larger increase in total pay after the MW law than older CEOs.

Alternatively, one may argue that younger CEOs are paid more because they have greater bargaining power (e.g., due to better outside options, Edmans et al. (2017)). If so, CEOs of high-rent firms should face larger pay increases after the MW law than those of low-rent firms. In contrast, I find that pay of low-rent firm CEOs increases more than that of high-rent firm CEOs, and for both groups, pay increases are insignificant. This evidence is inconsistent with the mechanism based on the bargaining hypothesis.

Another mechanism that can potentially explain the CEO pay increases driven by employee pay increases is based on the efficiency wage hypothesis. The hypothesis posits that pay hikes should motivate employees to work harder. In turn, improved productivity will lead to stronger firm performance, allowing CEOs to reap larger compensation benefits. However, my results show no evidence of smaller firms having significantly better performance after the MW law, which does not support the efficiency wage hypothesis predictions.

Moreover, my analysis has important policy implications. Quantifying the semi-elasticity of the CEO pay with respect to MW is crucial for successfully targeting income inequality since the estimate allows evaluating the magnitude of the spillover effect of MW on CEO pay. I show that the CEO pay-MW semi-elasticity is equal to 0.045. The economic magnitude of this effect translates into the 41% hike in federal MW leading to a 1.84% relative increase in pay for a median small firm CEO. In comparison, the corresponding relative increase in pay for a median small firm employee is over seven times larger and is equal to 12.97%.

This paper contributes to the broad strand of literature on the efficacy of the MW policy in reducing pay inequality. Existing papers study the distributional aspects of MW and its benefits to the low-wage regular workers compared to the high-wage regular workers (Lee (1999), Freeman (1996)). DiNardo, Fortin, and Lemieux (1995) document heterogeneous effects based on gender, while others consider different country settings (e.g., Rinz, Voorheis, et al. (2018), Fortin, Lemieux, and Lloyd (2021) and Autor, Manning, and Smith (2016) for the U.S., Engbom and Moser (2021) for Brazil, Butcher, Dickens, and Manning (2012) for the U.K.). In contrast, I document a positive impact of MW on the redistribution of pay between rank-and-file workers and *top executives*.

Second, this paper is related to the literature on fairness within the firm. Previous work on relative performance evaluation has focused on the fairness across firms, comparing CEO pay to peer CEO pay. This paper explores CEO pay with respect to employee pay, a different facet of fairness, and provides novel causal evidence of spillover effects of employee pay on CEO pay. Related, Dittmann, Schneider, and Zhu (2020) document an opposite direction of causality based on the relative wealth concerns of employees. Other work connecting CEO and employee pay largely focuses on the effects of the pay ratios on firm value and operating performance (Faleye, Reis, and

reputation, measured as the number of times the CEO name appears in the media. Also, Joos, Leone, and Zimmerman (2003) and Bizjak, Brickley, and Coles (1993) show that CEO age matters for boards when appointing CEOs.

Venkateswaran (2013), Mueller, Ouimet, and Simintzi (2017)) and documents correlations.[7]

Finally, my results are consistent with the fairness hypothesis of executive compensation of Edmans et al. (2021). This paper may open up a new stream of empirical research testing recently developed theories of executive compensation, in addition to the well-established optimal contracting and managerial rent extraction theories.[8]

The rest of this paper is organized as follows. Section 2 discusses the federal MW policy in the U.S. and its effect on firms. Section 3 describes the data sources used in the analysis and the sample construction. Section 4 details the empirical specification and presents the main results. Section 5 tests potential mechanisms underlying the results. Section 6 covers robustness checks. Section 7 concludes.

2. Background

In this section, I provide background information on the federal MW policy in the U.S., particularly on the Fair Minimum Wage Act of 2007.

2.1. History of the minimum wage policy in the U.S.

As documented by the U.S. Department of Labor, any employee has the right to receive a maximum of state and federal MW.[9] Therefore, a federal MW increase mandated by the law will only affect the pay of employees at establishments located in states with state MW being *lower* than federal MW. Following the MW literature, I call these states "bound" (e.g., Gustafson and Kotter (2022), Chava, Oettl, and Singh (2019), Dai and Qiu (2022)).

Before the mid-1980s, the majority of the states were bound. Hence, federal MW laws enacted throughout that period do not provide enough variation to create an appropriate counterfactual group for bound states - the number of unbound states in the sample is too small. This fact limits the set of federal MW laws that could be used to model exogenous shocks to employee pay to three laws enacted on November 17, 1989, August 20, 1996, and May 25, 2007 (Gustafson and Kotter (2022)). These laws raised the federal MW level in total by 26.9%, 21.2%, and 40.8%, respectively. Many academics have emphasized the importance of using large-magnitude shocks for establishing causality, including the 2021 Nobel Prize for Economics winner David Card.[10] Related, Harasztosi

[7]One notable exception is De Vito and Gómez (2022), who use a quasi-exogenous shock to within-firm pay inequality through labor regulation reform in Italy to study the causal effect on firm performance – however, their research question and outcome variables are different from mine.

[8]See, e.g., Holmström (1979) for development of the optimal contracting theory, and Bertrand and Mullainathan (2001), Bebchuk and Fried (2003), Bebchuk, Fried, and Walker (2002), among others, for the establishment of the rent extraction theory. Murphy (2013) and Edmans et al. (2017) provide excellent overviews of the executive compensation literature.

[9]Source: U.S. Department of Labor website, https://www.dol.gov/general/topic/wages/minimumwage.

[10]The prize lecture entitled "Design-based research in empirical macroeconomics" is available here: https://www.youtube.com/watch?v=wD48p6m8U-8.

and Lindner (2019), Aaronson, French, Sorkin, and To (2018), and Sorkin (2015) highlight challenges coming from using small MW shocks for identification and estimation of their effects. Hence, the federal MW law of May 25, 2007, which affects the federal MW level the most out of the three laws, is the best choice to use as a natural experiment for estimating the spillover effects of employee pay on CEO pay.

This law is a component of the Fair Minimum Wage Act of 2007, a part of the U.S. Troop Readiness, Veterans' Care, Katrina Recovery, and Iraq Accountability Appropriations Act. It was introduced in the House on January 5 and passed five days later, voted "for" by all the Democrats and by 41% of Republicans. President George W. Bush signed it on May 25, 2007.[11] The law consisted of three stages (referred to as "events"), gradually raising the level of federal MW from \$5.15 to \$5.85, \$6.55, and \$7.25 per hour, respectively. Each event became effective with a yearly time difference, with event one becoming effective on July 24, 2007, and events two and three on July 24, 2008, and July 24, 2009, respectively. Details of the law are summarized in Table 1. Figure 2 presents the distribution of states classified as bound or unbound pre-introduction of the law.

As of November 2022, federal MW constitutes \$7.25 per hour, and the Fair Minimum Wage Act of 2007 is the only federal MW law since the early 2000s. However, the debate about raising the federal MW level to \$15 per hour has been active since 2012.[12] If the initiative of raising the federal MW to \$15 per hour is introduced into life in the coming years, it will provide an additional opportunity for future researchers to expand the set of MW event studies.

2.2. Which firms are affected by the minimum wage law the most?

It is important to acknowledge that the MW law affects firms differentially. Intuitively, the larger the share of MW workers in the firm employment, the more this firm will be affected by the MW law. In order to identify industries with the largest concentration of firms relying on MW labor, I collect data on the monthly average employment by industry, as well as the annual average number of MW workers (total, and for workers at, below, and at/below MW) by industry, from the Bureau of Labor Statistics reports based on Current Population Survey estimates. I further calculate the annual relative share of MW employment in total employment by industry.

Figure 3 illustrates that the two industries employing the largest share of MW labor are Leisure and Hospitality (NAICS2 codes 71-72) and Retail Trade (NAICS2 codes 44-45). In 2006, which is the year before the MW law introduction, 7.9% and 0.9% of workers were employed at or below MW in the Leisure and Hospitality and Retail Trade industries, respectively, while the same statistics for all other industries combined were only 0.1% (Figure 3a). This finding is consistent with Dai and Qiu (2022), Chava et al. (2019), and similar to Gustafson and Kotter (2022).[13] Hence, I define

[11] Fair Minimum Wage Act of 2007 text and summary is available at https://www.congress.gov/bill/110th-congress/house-bill/2.

[12] See, e.g., the official website of the political movement "Fight for \$15": https://fightfor15.org/.

[13] Dai and Qiu (2022) and Chava et al. (2019) use Accommodation and Food Services and Retail Trade industries

"affected" firms as firms belonging to NAICS2 industries with codes 71, 72, 44, and 45. I expect the effect of the MW law to be more pronounced in the subsample of affected firms.

Importantly, the Current Population Survey data reflect employee wages without overtime pay, tips, or commissions.[14] Hence, tipped workers are classified as below federal MW workers, despite being subject to lower cash MW level. Figure 3b shows that even excluding tipped workers, Leisure and Hospitality and Retail Trade industries remain the most reliant on MW labor.

3. Data and sample construction

3.1. Data sources

This study combines multiple data sources. CEO compensation data are obtained from Capital IQ, MSCI (formerly GMI Ratings), Execucomp, ISS Incentive Lab, and BoardEx. Compustat annual data file is used to construct yearly firm-level control variables and access historical firm headquarters locations and industry codes. Information on establishment-level employment and state location comes from YTS (Your-economy Time Series) data.

3.2. Sample construction

I start with the sample of all publicly traded corporations in Compustat. Firm historical location is obtained from Compustat historical records (*hstate*) available through WRDS server data. I leave only firms located in the U.S. I require that these firms have non-missing total assets and have at least two annual observations before and after July 24, 2007, which is when the MW law becomes effective.

I then define time dummies for the event of the MW increase, focusing on fiscal years rather than calendar years, following Gustafson and Kotter (2022).[15] I do this to ensure that the MW shock effect is reflected in firms' CEO compensation filings as soon as the MW law becomes effective. I define an event window $t \in [-2; 2]$ to keep it wide enough to capture the effects of the MW law and narrow enough to exclude other potential confounding events. I keep only observations belonging to the event window. Figure 4 presents the timeline definition.

I further merge the sample to compensation datasets. Following Bloom, Ohlmacher, Tello-Trillo, and Wallskog (2021), I define the highest paid executive in the fiscal year per firm to be CEO (confirmed in 97.4% of cases where CEO is identified).[16] I require all observations to have positive

(NAICS2 codes 72 and 44-45). Gustafson and Kotter (2022) use the Entertainment, Retail, and Restaurants, Hotels, and Motels industries (FF48 codes 7, 42, and 43).

[14]Source: the Bureau of Labor Statistics, https://www.bls.gov/opub/reports/minimum-wage/2020/home.htm.

[15]Note that if a firm's fiscal year (FY) ends in January-May of year t, then FY is defined as t-1; while if a firm's FY ends in June-December of year t, then FY is defined as t.

[16]CEO is defined in Execucomp (*CEOANN* variable), in MSCI (all observations correspond to CEOs), and in ISS (*currentCEO* variable). Excluding MSCI, the highest-paid executive per fiscal year per firm is identified as CEO in 82.58% cases.

CEO total pay.[17] I start matching with Capital IQ, keep matched observations, and continue the matching procedure for unmatched observations with MSCI, Execucomp, ISS, and BoardEx. This matching order is defined by the relative firm coverage presented in Figure A1a.

Matching different datasets is complicated when a unique identifier is unavailable. In order to increase the match rate, I use several different identifiers to match each CEO compensation dataset to the Compustat sample. I start with matching Compustat to Capital IQ by GVKEY. Secondly, I match the residual sample to MSCI by Ticker. Next, I match to Execucomp by GVKEY and to ISS by CIK. Finally, I match the remaining observations to BoardEx by GVKEY using the BoardEx-CRSP-Compustat link table by WRDS.[18] The distribution of the matched number of firms per period is given in Figure A1b.

I obtain firm location data from YTS. First, I group establishments located in bound and unbound states for each firm×headquarters, as per Figure 2. Establishments located in bound states are referred to as "bound establishments." Then, I calculate the percentage of employees working at bound establishments for each firm×headquarters. Next, following Flynn and Ghent (2021), I match YTS headquarters data to Compustat in three rounds. First, I match by Ticker. Second, I match by company name and zip code of the firm×headquarters. Third, I fuzzy match (Raffo (2020)) by standardized company name (Wasi and Flaaen (2015)), with exact matching required by industry, and leave only matches with a similarity score of 0.7 or higher. I keep the observation with the largest similarity score in case of duplicate firm-year observations.

I further clean the constructed sample as follows. I match to control variables from Compustat and CRSP. I winsorize all the current and lagged control variables at the 0.5% and 99.5% levels and keep only observations with non-missing lagged control variables. Control variables are standard for the executive compensation literature and include lagged firm size, ROA, profitability, Tobin's Q, market-to-book, and log(1+annualized return). All controls are similarly balanced in terms of observation availability. I keep only firms with non-missing observations for each period $t \in [-2; 2]$ and define industries at the NAICS3 level at $t = -2$. This ensures that firms do not change industries throughout the event window. Figure A1c presents the number of matched firms per period to YTS, CRSP, and post-additional filters. The final sample consists of 1,732 firms per period, as shown in Figure A1d. Table A1 presents the definitions of the variables.

3.3. Baseline: Main sample

I obtain a sample of 1,732 U.S. public firms per period with available CEO pay data, employment levels, and non-missing control variables for the five-period time horizon of $t \in [-2; 2]$, covering dates from July 24, 2005, to July 24, 2010. I refer to it as the "main sample," and I focus on the main sample throughout the paper since it is constructed with the least number of filters possible. In a

[17] For a detailed description of the construction of CEO pay datasets, see Section A.1 in the Appendix.

[18] In the BoardEx-CRSP-Compustat link table, I leave only observations with *score* less than or equal to 8 out of 10 and with *preferred* equal to 1 to ensure the best matching quality.

robustness analysis, I consider a matched sample and a sample of firms headquartered in counties on the borders of contiguous states (results for these samples are included in Section 6).

Table 2, Panel A shows summary statistics for firm characteristics and CEO pay variables for all firms in the main sample. During this period, 5.1% of firms belong to the affected industries, and an average firm has 28.6% of employees located in bound states. During the sample period, a median CEO has received $608.8K in total pay, with cash pay and incentive pay accounting for 77% and 23% of compensation, respectively. The median firm in the sample has $1.09MM in total assets, an ROA of 0.029, and a market-to-book ratio of 1.30. About 94% of the main sample CEO compensation data comes from Capital IQ.

3.4. Smaller and larger firms subsamples

Intuitively, CEOs and rank-and-file employees have very different roles within the firm, which creates a gap between them. However, this gap is expected to be less pronounced in smaller firms. The literature has established that smaller firms adopt less formal human resource management practices (e.g., Wilkinson (1999)) and are less hierarchical (e.g., Kotey and Slade (2005)). Hence, at smaller firms, CEOs and employees are closer in terms of responsibilities and functions, and there is less need for the CEOs to control employees, leading to smaller corresponding wage differences between them (Calvo and Wellisz (1979)). Thus, I conjecture that the connection between CEOs and employees plays an important role in establishing the relationship between CEO and employee pay and that the effect of the increase in MW will be more pronounced in smaller firms.

To test this conjecture, I present the results for the analysis based on three subsamples: smaller firms, larger firms, and all firms. To construct the subsample of smaller firms, I split the main sample into terciles by the number of employees. As discussed above, the effect of employee pay on CEO pay is less likely to exist for large firms. Hence, I focus on the bottom two terciles and refer to them as "smaller" firms. Firms in the top tercile form the sample of larger firms.

Table 2, Panels B and C present summary statistics for firm characteristics and CEO pay variables for the smaller and larger firms, respectively, in the main sample. A median smaller firm has 518 employees, 4.9% of which are located in bound states, while a median larger firm has 8,479 employees, with 28% of them working in bound states. A median smaller firm has $602K in total assets, an ROA of 0.015, a market-to-book ratio of 1.24, and pays the CEO $438.1K per year. In contrast, the total assets of a median larger firm constitute $4.59MM, ROA is equal to 0.053, the market-to-book ratio is equal to 1.39, and the CEO receives $1.7MM in total pay per year.

4. Empirical analysis

In this section, I develop the hypotheses connecting employee and CEO pay and identify the empirical approach which allows for establishing causality between the two. I also present the main empirical results establishing the effects of the MW law on employee pay and CEO pay and discuss their implications.

4.1. Hypothesis development

Intuitively, CEO pay could be positively related, negatively related, or unrelated to employee pay. Three potential scenarios reflecting these outcomes are summarized in hypotheses 1, 2, and 3:

Hypothesis 1 *When employees receive an exogenous increase in pay, CEOs demand a corresponding increase in their pay.*

Hypothesis 2 *When employees receive an exogenous increase in pay, the unexpected wage expense decreases firm value, and hence CEOs receive smaller compensation.*

Hypothesis 3 *When employees receive an exogenous increase in pay, CEO pay does not change.*

A positive relationship between CEO and employee pay is consistent with hypothesis 1. Several underlying mechanisms can justify this outcome. For example, one mechanism is based on the "fairness hypothesis" proposed by Edmans et al. (2021) (and further theoretically developed by Chaigneau et al. (2022)). They survey a sample of U.K. directors and investors and conclude that fairness is important to executives in assessing their pay levels. Fairness is determined with respect to a set of reference points (e.g., employee pay,[19] peer company CEO pay, CEO value added to the firm, and CEO last year's compensation). A fair reward would signal a CEO's reputation in the executive labor market and motivate her to put in effort. Another mechanism is based on the "efficiency wage hypothesis" developed in the seminal papers of Yellen (1984) and Akerlof and Yellen (1986). According to the hypothesis, an increase in wages will incentivize employees to work more efficiently. Improved productivity will translate into better firm performance, allowing the CEOs to enjoy higher compensation.

Alternatively, hypothesis 2, referred to as the "profitability hypothesis," predicts a negative effect of employee pay on CEO pay. In the case of the MW law introduction, since it is initiated by the government, it may present an unplanned expenditure in terms of firm payroll. As a result, an extra wage expense will negatively affect the firm profit and, consequently, its share that could be awarded to the CEO. Consistent with this hypothesis, existing research has shown a drop in firm profitability following MW hikes (e.g., Draca et al. (2011), Harasztosi and Lindner (2019)).

Finally, CEO pay may be independent of employee pay. A potential reason for this outcome, summarized in hypothesis 3 (referred to as the "independence hypothesis"), is that CEOs and employees operate in different labor markets.

In this paper, I test the three hypotheses by estimating the sign and magnitude of the effect of an increase in employee pay driven by the MW law on CEO compensation.

[19] Note that since small firms are the focus of this paper (see Section 3.4), employee pay is a plausible reference point for CEOs in evaluating whether their compensation levels are fair.

4.2. Methodology

To estimate the causal effect of employee pay on CEO pay, I use a DDD specification (e.g., Gustafson and Kotter (2022), Luca and Luca (2019)). The first difference allows for estimating an effect on CEO pay of an increase in the share of employees in bound states. For a given firm, the more employees are located in bound states, the more employees will receive a larger pay following the MW law becoming effective. Therefore, the first difference represents a continuous assignment to treatment at the firm-period level: a larger employment share in bound states represents a larger treatment dose. The second difference compares outcomes after vis-a-vis before the effective date of the law. The third difference compares outcomes for firms affected vis-a-vis unaffected by the law. The DDD model is given by Equation 1:

$$\begin{aligned}\log(Y_{i,j,t}) = \beta_0(\text{Post}_t \times \text{Affected}_j \times \%\text{Bound}_i) + \beta_1(\text{Post}_t \times \text{Affected}_j) + \beta_2(\text{Post}_t \times \%\text{Bound}_i) \\ +\beta_3(\text{Affected}_j \times \%\text{Bound}_i) + \beta_4\text{Post}_t + \beta_5\text{Affected}_j + \beta_6\%\text{Bound}_i \\ +\Gamma X_{i,t-1} + \text{Industry-Year FE} + \text{Firm FE} + \epsilon_{i,j,t}\end{aligned} \tag{1}$$

The dependent variable Y corresponds to each CEO pay component: total pay, 1+cash pay (equal to salary plus bonus), 1+salary, 1+bonus, and 1+incentive pay (equal to total pay less cash pay). One is added to the CEO total pay component values to account for meaningful zero values. Typically, incentive pay consists of stock and option awards, long-term incentive plan, and other compensation[20] - however, due to inconsistencies in the definitions among different CEO pay datasets, I assign the difference between total pay and cash pay to incentive pay.[21]

"Post" is a dummy variable equal to 1 for observations belonging to periods $t = 0, 1, 2$. "Affected" is a dummy variable equal to 1 if a firm is affected by the MW law. Affected firms belong to the Leisure and Hospitality (i.e., Arts, Entertainment, and Recreation, NAICS2 71, and Accommodation and Food Services, NAICS2 72) and Retail Trade (NAICS2 44-45) industries.[22] "%Bound" stands for the share of firm workers employed at establishments located in bound states. Following the executive compensation literature, the control variables include lagged firm size, ROA, profitability, Tobin's Q, market-to-book, and log(1+annualized return). Industry-year fixed effects control for time-varying industry shocks (Gormley and Matsa (2014)), and firm fixed effects control for unobserved firm heterogeneity. The variable "Affected" drops out of the regression estimates since the firm industry is fixed over the sample period, and hence it is collinear with firm fixed effects.

The key advantage of the DDD specification is that it allows for estimating and comparing the effect of a MW increase shock on CEO pay on two dimensions: industry and employee location. This

[20]As per Execucomp definition, which is the dataset commonly used in executive compensation literature.
[21]Section A.1 discusses pay variables construction and definitions in detail.
[22]See Section 2.2 for a detailed discussion.

effect is representative of the coefficient β_0. More precisely, the interpretation of β_0 is the following: a one percentage point increase in the share of employees located in bound states results in β_0% extra compensation post-MW law for CEOs of affected vis-a-vis unaffected firms.

Importantly, the DDD strategy is valid if the relative CEO pay of affected firms with a high (high-%Bound) and low (low-%Bound) share of workers located in bound states evolves in parallel compared to the relative CEO pay of high-%Bound and low-%Bound unaffected firms during the pre-treatment period (Olden and Møen (2022)). To test this assumption, I split the subsamples of smaller and larger firms into high-%Bound and low-%Bound firms based on the median %Bound values. Figure A2 illustrates that the parallel trends in ratios assumption is satisfied: relative CEO pay ratios based on the level of %Bound for affected and unaffected firms largely co-move during $t = -2$ and $t = -1$ time periods.

4.3. Did the minimum wage law raise employee pay?

The conclusion that the MW law altered employee pay, which in turn altered CEO pay in smaller firms, relies on the assumption that the MW hike *actually* raised employee wages. To test this assumption, I estimate a version of Equation 1, with employee wages proxied by total labor expense (Compustat variable *xlr*) per employee as a dependent variable.

An immediate estimation challenge is that firms are not required to disclose labor expense, and hence *xlr* is available only for a quarter of the main sample (as discussed in Section A.2). Generally, the affected firms, as a subset of non-manufacturing firms, disclose wages in selling, general, and administrative (SG&A) expenses part of the income statement[23] - however, in some cases, wages are included in the operating expense section, which is a sum of COGS and SG&A expenses. For robustness, I also use SG&A per employee and operating expenses per employee as dependent variables in the estimation of Equation 1. In a specification with SG&A per employee as an outcome, I exclude all manufacturing firms from the main sample to ensure an adequate comparison of treated and control groups. Data on operating expense (variable *xopr*) and SG&A expense (variable *xsga*) are from Compustat.

Table 3 presents the estimation results. Panel A shows that for smaller firms, the DDD coefficient is estimated to range from 0.6 to 4.2, depending on the wage proxies used. The economic magnitude of the effect is that for smaller, affected vs. unaffected firms, a 10-percentage point increase in the employment share in bound states driven by the MW shock leads to an increase in employee wages of 6% to 42%. The effects are insignificant for larger firms, consistent with the documented evidence that firms with at most a hundred workers hire over 90% of the MW worker pool.[24] Indeed, in the main sample, a median smaller firm employs 518 workers, while a median

[23]In contrast, manufacturing firms disclose employee wages in the cost of goods sold (COGS) part of the income statement.

[24]Source: the Economic Policy Institute report, https://epionline.org/oped/who-really-employs-minimum-wage-workers/.

larger firm employs 8,479 workers, consistent with smaller firms having a larger absolute and relative concentration of MW workers among the employee pool (see Table 2). This evidence validates the identification strategy underlying the DDD analysis of the impact of employee pay on CEO pay.

4.3.1. The effect of the minimum wage law on firm employment

Moreover, Table 3 shows that a MW shock also leads to a drop in employment. In specifications with firm employment used as a dependent variable, the DDD coefficient is estimated to range from -0.8 to -4.4, depending on the labor expense proxy. The economic magnitude of the effect is that a 10-percent change in MW results in an 8% to 44% reduction in employment for smaller, affected vs. unaffected firms.

The result of a decline in employment after a MW shock adds to the long-standing debate in the labor literature on the effects of MW hikes on firm employment and is consistent with a "competitive labor market" view (e.g., Neumark and Wascher (2008), Draca et al. (2011)). However, opponents of this view state that the controversy of the existing studies comes from the fact that the current literature does not differentiate between low- and high- concentration labor markets. Azar, Huet-Vaughn, Marinescu, Taska, and Von Wachter (2019) show that in a retail sector, low concentrated markets face a reduction in employment post increase in MW, consistent with the "monopsony" view. They also argue that aggregated, these results could give zero estimated effect (consistent with, e.g., Card and Krueger (1994), Card and Krueger (1995)) but would be essentially non-representative. I contribute to this debate and provide new evidence supporting the competitive labor market view, using variation in the federal-level MW. My estimates also complement the recent results of Karabarbounis, Lise, and Nath (2022), who use variation in the city-level MW to estimate MW elasticity of employment.

4.4. Main estimates: how does the minimum wage law affect CEO pay?

Table 4 presents the main results of estimating Equation 1 on the main sample. Panels A, B, and C show the results for smaller, larger, and all firms based on the number of firm employees.[25] Panel A shows that the result of an increase in CEO total pay is more pronounced for smaller firms, and the DDD coefficient β_0 is estimated to be 0.65. Its economic magnitude can be interpreted in the following way: a 10-percentage point increase in the share of employees located in bound states results in a 6.5% increase in CEO total pay for affected vs. unaffected firms, following a positive MW shock.

For smaller firms, both the salary and incentive pay components of compensation increase: the corresponding DDD coefficients are 0.50 and 4.58, respectively. The economic magnitude of these coefficients implies that following the shock, a 10-percentage point increase in %Bound leads to 5% and 45.8% increase in salary and incentive pay, respectively, for affected vs. unaffected firms.

[25]See Section 3.4 for a discussion on the role of smaller firms in the analysis.

The coefficients are statistically significant at 5% and 1% for salary and incentive pay, respectively. The corresponding salary increase is consistent with the similar persistent nature of MW shocks and executive salary (Murphy (2013)): both represent a fixed component of pay, though MW shocks affect rank-and-file employees while executive salary shocks affect CEOs. The increase in incentive pay is consistent with the idea that CEOs care most about incentive pay since incentive pay increases are presented to shareholders and have to be approved by the board, providing ex-post recognition incentives for CEOs (Edmans et al. (2021)).

The effect for all firms is less pronounced than for smaller firms. This result is consistent with the expectation that CEOs of large firms, despite relying on MW labor, are unlikely to be affected by a several-dollar increase in MW. This result gives rise to the next section, which tests whether my strong findings for smaller firms are driven by the fact that CEOs and employees have more similar roles in smaller firms and hence are more connected.

4.5. Does CEO-employee connection matter?

CEOs and rank-and-file employees occupy different positions within the firm: CEOs are top executives, while rank-and-file employees are non-executive workers. This difference affects their pay levels through the hierarchy effect: CEOs are paid a premium for having to supervise regular employees (Calvo and Wellisz (1979)). Therefore, if my story based on the connection of CEOs and employees is true, the effect of a shock to employee pay should be transmitted to CEO pay to a larger extent if CEOs and employees are "comparable" with respect to their pay before the shock. One way to compare the proximity of the CEO-employee relationship is to use the CEO-employee pay ratio. Therefore, in this section, I test the following prediction:

Hypothesis 4 *Post introduction of the MW law, the effect of CEO pay increases for affected vs. unaffected firms is stronger for firms with a lower CEO-employee pay ratio before the law.*

Table 5 presents the results of estimating Equation 1 on the main sample.[26] Panels A and B split the main sample into firms with smaller (bottom two terciles at $t = -1$) and larger (top tercile at $t = -1$) *CEO-employee pay ratios.* Panel A shows that for firms with smaller pay ratios, the DDD coefficient is estimated to be 0.72. The economic magnitude of the effect implies that a 10-percentage point increase in %Bound leads to a 7.2% increase in CEO total pay for affected vs. unaffected firms post-MW hike. The salary component of CEO pay drives this result: in a specification with salary being a dependent variable, the DDD coefficient equals 0.97 and is statistically significant at the 5% level. The economic magnitude of the effect on salary is that a 10-percentage point increase in %Bound leads to a 9.7% increase in CEO salary for affected vs. unaffected firms post-MW hike. Panel B shows that the CEO total pay increase effect is insignificant for firms with larger pay ratios. Overall, the outcomes are economically and statistically similar to the baseline outcomes of Table 4

[26]Section A.2 describes the construction procedure of the CEO-employee pay ratios.

and are consistent with hypothesis 4. This evidence supports the idea that the connection between the CEO and employees, based on the similarity of their job responsibilities, drives the CEO pay increase result after the MW hike for smaller firms.

4.6. Semi-elasticity of CEO pay with respect to minimum wages

Estimating the semi-elasticity of CEO pay with respect to MW is important for policymakers targeting income inequality: the estimate will help quantify the intensity of spillover effects from MW policy on CEO pay. Previous analysis of this paper has established a significant effect on CEO pay driven by a federal MW hike with an aggregate magnitude of 41%, coming from the MW law of 2007. However, this change in MW was not uniform: the law consisted of three consequent federal MW raise stages represented by events 1, 2, and 3 (Table 1). Each event corresponded to an increase in MW equal to 13.6% (event #1), 12.0% (event #2), or 10.7% (event #3). Time series variation in MW hikes provides an ideal opportunity to estimate the impact of a relative increase in MW on CEO pay.

To do that, I construct a variable %Δ MW, which is equal to the percentage increase in MW for the relevant time period. For example, %Δ MW is equal to 13.6 for t = 0.[27] I estimate a DDD specification similar to Equation 1 on the main sample, substituting the time difference presented by the "Post" variable for the change in MW difference presented by %Δ MW variable (e.g., Gustafson and Kotter (2022), Chava et al. (2019)).

Table 6 presents the estimation results. Panel A provides evidence that for smaller firms, the DDD coefficient is estimated to be 0.045, implying that the semi-elasticity of CEO pay with respect to MW is 0.045. The economic magnitude of the effect is that for smaller firms, a 10-percentage point increase in MW for affected vs. unaffected firms with the same employment share in bound states leads to a 0.45% change in CEO total pay. Panel B shows that the effect is insignificant for larger firms.

The estimated elasticity value reflects that a 40.78% total increase in federal MW triggered by the Fair Minimum Wage Act of 2007 results in a \$8,039 ($0.045 \times 40.78\% \times \$438,080$) increase in total pay for the CEO of a median smaller firm. This effect corresponds to a 1.84% relative increase in CEO total pay.

4.6.1. Semi-elasticity of employee pay with respect to minimum wages

It is interesting to compare the economic magnitudes of CEO pay and employee pay raises in response to the 41% MW hike. Table A3 shows that in smaller firms, the semi-elasticity of *employee pay* with respect to MW ranges from 0.051 to 0.318, depending on the worker wage proxies used. This evidence translates into a \$6,918 ($0.318 \times 40.78\% \times \$53,328$) absolute, or 12.97% relative, increase in pay for a median small firm employee following the Fair Minimum Wage Act of 2007

[27] All values of %Δ MW are summarized in Table 1, Column "Δ Min Wage %."

becoming effective, if disclosed labor expense per employee is used as a proxy for wages. If the SG&A and the operating expense per employee proxy for wages instead, the corresponding economic magnitude of the median small firm employee pay increase is $1,111 (2.08%) and $1,227 (2.3%), respectively.

4.7. Elasticity of CEO pay with respect to employee pay: an instrumental variables approach

My main finding that an employee pay raise coming from a MW hike results in a CEO pay raise is vital for analyzing the effect of MW policy on the CEO-employee pay gap. However, to understand whether the MW hike helped to widen or narrow the gap, it is important to estimate the value of the elasticity of CEO pay with respect to employee pay. As discussed previously, estimates in Table 3, Panel A indicate that the share of workers employed in bound states for affected vs. unaffected firms impacts labor, SG&A, and operating expenses per employee through a MW increase. This setting creates an opportunity to evaluate the elasticity of CEO-employee pay through an instrumental variables approach. I choose operating expense as a proxy for employee wages to maximize sample size and improve estimation precision. The MW shock acts as an instrument to estimate employee wages, and the strong statistical significance of the coefficient on the DDD interaction term (Table 3, Panel A, column (5)) indicates that inclusion restriction is satisfied.

I conduct a placebo test to support the assumption that exclusion restriction is satisfied. The test is based on the idea that if the MW shock is a strong instrument for employee wages for affected firms relative to unaffected firms, then the MW shock should have no impact on employee wages of *only* unaffected firms. To show this, I exclude all affected firms from the main sample and collapse the methodology to a double differences (DD, hereafter) specification of Equation 1, presented in Equation 2:

$$\begin{aligned} \log(Y_{i,t}) = \beta_0(\text{Post}_t \times \%\text{Bound}_i) + \beta_1 \text{Post}_t + \beta_2 \%\text{Bound}_i \\ + \Gamma X_{i,t-1} + \text{Industry-Year FE} + \text{Firm FE} + \epsilon_{i,t} \end{aligned} \tag{2}$$

Table A4, Panel A, column (1) shows that for smaller firms, the coefficient on DD interaction is close to zero and not statistically significant. This evidence implies that the share of employees located in bound states has limited, if any, effect on operating expense per employee for *unaffected* firms post the MW shock, consistent with the assumption that the exclusion restriction is satisfied. Moreover, the fact that I use a relatively narrow event window of five periods around the MW increase shock to isolate the effect of the MW law and exclude other potential confounding events in the DDD specification adds extra credibility to the validity of the exclusion restriction. Notably, in the placebo test, the employment effects established previously disappear (Table A4, Panel A, column (2)).

I further regress the logarithm of CEO total pay on the logarithm of the instrumented operating expense per employee (*"IV log(XOPR per emp.)"*), including controls, industry-year fixed effects, and firm fixed effects as before. Table A4, Panel A, column (3) shows that for small firms, the DDD coefficient is estimated to be 0.077, meaning that the elasticity of CEO pay with respect to employee pay is 0.077. The economic magnitude of the effect implies that a 1% increase in employee pay results in a 0.077% increase in CEO pay. Since the elasticity value is lower than 1, this evidence indicates that the MW increases should effectively narrow the CEO-employee pay gap, consistent with the reasoning discussed in Section 1. However, the coefficient is not precisely estimated; hence, this value should be treated as a qualitative estimate of the elasticity of CEO pay with respect to employee pay.

4.8. How does the minimum wage law affect the CEO-employee pay gap?

The previous analysis shows that the MW law triggers a hike in employee pay, leading to the CEO pay increase. In relative terms, a 10-percentage point increase in federal MW leads to a 42% increase in employee pay and a 7% increase in CEO pay for smaller, affected vs. unaffected firms post-MW law. Moreover, an instrumental variables approach allows for estimating the value of the elasticity of CEO pay to employee pay which turns out to be lower than 1. As discussed above, this evidence provides a base for a conclusion that the MW law is a valid policy measure to reduce the CEO-employee pay gap. To confirm this conjecture, I estimate Equation 1 using the CEO-employee pay ratio as a dependent variable.

Table 7 presents the estimation results: columns (1)-(3) are based on the original CEO-employee pay ratio constructed using the labor expense disclosed by firms in *xlr* variable, while columns (4)-(6) are based on the imputed CEO-employee pay ratio per the Donangelo (2016) procedure.[28] Columns (1) and (4) show that the estimated DDD coefficient ranges from -4.5 to -0.66, depending on the specification used. The economic magnitude of the effect is that a 10-percentage point increase in %Bound leads to a drop in the CEO-employee pay ratio ranging from 45% to 6.6% for smaller firms. This evidence further supports the efficiency of price floors for workers in narrowing the CEO-to-employee pay gap in smaller firms.

5. Why does CEO pay increase?

So far, I have documented an increase in CEO total pay due to an increase in the share of employees located in bound states, post MW hike for affected vs. unaffected firms, as well as evidence in favor of the proximity of the CEO-employee roles driving the result. In this section, I explore a mechanism explaining *why* CEO pay goes up and address alternative explanations.

[28] Construction of the pay ratio is described in detail in Section A.2.

5.1. Fairness hypothesis

The fairness hypothesis developed by Edmans et al. (2021) and summarized in hypothesis 1 posits that CEOs compare their compensation to employee pay when assessing the fairness of their own pay. Therefore, when employees receive an exogenous wage increase as the MW shock induces, CEOs may feel that their compensation is unfair if it is not adjusted upwards. The role of this extra pay increase is not to sponsor consumption but to acknowledge the CEO's reputation in the executive labor market (Edmans et al. (2021)).

The result of an increase in CEO pay for affected vs. unaffected firms post the MW shock is consistent with the fairness hypothesis. Moreover, if this hypothesis is correct, the CEO pay increase should be larger for younger CEOs. This is because, for younger CEOs, the impact of current pay on the present value of future pay is more significant (e.g., Boschen and Smith (1995), Gibbons and Murphy (1992), Edmans et al. (2012)), so younger CEOs should care about receiving fair compensation more than older CEOs. To test this conjecture, I split the main subsample of smaller firms into bottom 50% and top 50% based on CEO age at $t = -1$: the threshold median CEO age is 58. I focus on smaller firms since I have shown that the proximity of CEO and employee roles, which drives the effect of the MW increase on CEO pay, is more pronounced in a subsample of smaller firms. In smaller firms, the average age of younger CEOs at $t = -1$ is 49, while for older CEOs, it is 65.

I estimate Equation 1 on the subsamples of firms with younger and older CEOs. Table 8 shows that the estimated DDD coefficient is 1.02 and 0.41 for younger and older CEOs, respectively. The economic magnitude of the effect is that after the MW shock, a 10-percentage point increase in %Bound leads to a 10.2% increase in total pay for younger CEOs in affected vs. unaffected, smaller firms. In contrast, for older CEOs, the economic magnitude of the CEO pay increase is only 4.1%. The evidence based on the CEO age test is consistent with the implications of the fairness hypothesis. After the MW shock, younger CEOs of affected vs. unaffected firms receive a pay increase of about 2.5 times larger in relative terms than the pay increase of older CEOs of affected vs. unaffected firms, indicating that younger CEOs are more concerned with fair pay.

5.2. Bargaining hypothesis

At this point, I have established that in a subsample of smaller firms, an increase in employee pay leads to an increase in CEO pay for affected vs. unaffected firms after the MW law. I provide evidence that younger CEOs face larger pay increases than older CEOs and claim that it is consistent with the fairness hypothesis. An alternative mechanism that could be driving my result is summarized in hypothesis 5:

Hypothesis 5 *Younger CEOs have better outside options than older CEOs. Hence, younger CEOs have more bargaining power to increase their pay.*

I refer to the hypothesis 5 as a "bargaining hypothesis." It is possible that CEOs that receive larger pay after the MW shock (i.e., younger CEOs) have more bargaining power (Edmans et al. (2017)) and hence can extract larger rents from the firm. If this alternative mechanism is true, I expect that CEOs of smaller firms with more rents to be shared should face larger pay increases after the MW shock compared to CEOs of firms with fewer rents to be shared.

To test this mechanism, I use average industry-adjusted ROA at the pre-treatment period ($t = -1$ and $t = -2$) as a proxy for firm rents to be shared. I focus on the subsample of smaller firms and split it into the top 50% and bottom 50% by the industry-adjusted ROA. Firms in the top 50% of the subsample are high-rent firms compared to firms in the bottom 50% of the subsample, which are low-rent firms. The threshold median average industry-adjusted ROA is -0.004. For high-rent and low-rent firms, the mean average industry-adjusted ROA is 0.073 and -0.098, respectively.

Table 9 presents the estimation results of Equation 1 on the subsamples of smaller firms with high and low rents. The effect of an increase in the MW level is less pronounced in the subsample of smaller firms with high rents. Moreover, the effect is statistically and economically insignificant both for firms with high and low rents. This evidence is inconsistent with the alternative mechanism claiming that the difference in firm rents to be shared is responsible for the established increase in CEO pay for smaller, affected vs. unaffected firms after the MW hike.

5.3. Efficiency wage hypothesis

Another alternative explanation that could be driving the result of the CEO pay increase after a MW shock is presented in hypothesis 6:

Hypothesis 6 *An increase in employee pay resulting from a MW shock makes employees more productive. Consequently, as firm performance improves, CEO pay increases.*

I refer to the hypothesis 6 as an "efficiency wage hypothesis." The hypothesis stems from a broad strand of literature (e.g., Yellen (1984), Akerlof and Yellen (1986)) and posits that an increase in wages will motivate employees to work better and hence lead to an increased productivity level. If the efficiency wage story is true, productivity should rise following a MW shock for affected vs. unaffected firms.

I use firm industry-adjusted ROA (Hermalin and Weisbach (2012)) and logarithm of one plus firm annualized return as proxies for productivity and estimate Equation 1 on subsamples of smaller, larger, and all firms. Table 10 presents the estimation results: columns (1) and (4) show that for smaller firms, the DDD estimates of both specifications are statistically and economically insignificant. Moreover, the coefficient magnitude implies that the industry-adjusted ROA has not changed for affected vs. unaffected smaller firms around the MW shock. This evidence is inconsistent with the predictions of the efficiency wage hypothesis since it illustrates that firm productivity did not increase significantly after the MW shock.

Based on the CEO age test, the bargaining test, and the efficiency wage test, I conclude that the fairness hypothesis is a plausible mechanism driving the established CEO pay increase. A MW hike results in an increase in wages for employees of smaller, affected vs. unaffected firms, and CEOs of these firms demand a pay raise as well in order to receive a fair compensation package with respect to employee wage change.

6. Robustness

In this section, I examine the robustness of the main results to alternative sample definitions.

6.1. Matched sample

A potential concern is that affected and unaffected firms with different levels of exposure to employment in bound states may be different on dimensions of observable characteristics. Moreover, an identification assumption of the triple difference framework is that treated and control firm-years must be comparable to exclude the possibility of differences in characteristics driving the results. To mitigate this concern, I construct a matched sample of treated and control firm-years from the universe of firms belonging to the main sample.

I match firms at $t = -1$ with covariate-vector nearest neighbor matching technique with replacement using the Mahalanobis weighting metric (Abadie and Imbens (2002)), with the bias-correcting matching estimator (Abadie, Drukker, Herr, and Imbens (2004)). Matching covariates include contemporaneous control variables (i.e., firm size, profitability, ROA, Tobin's Q, and market-to-book). For every bound (i.e., having more than the sample median of 15.34% employees located in bound states at $t = -1$) firm, I match one unbound firm and require that an affected/unaffected firm receives an affected/unaffected matched counterfactual. I choose the sample median at $t = -1$ as the cutoff for the share of employees located in bound states to ensure a balanced pool of bound and unbound firms to be matched to each other and to ensure that a firm does not switch from bound to unbound or vice versa. I also require an exact match on industry (indicated by the NAICS2 code). As a result, I obtain a matched sample of 1,694 firms per period.

Table A5 presents the results of a t-test of mean differences for unbound vs. bound firms comprising the matched sample, with standard errors clustered at the firm level. The table shows that the matching procedure ensures that matched unbound and bound firms have similar pre-treatment CEO pay levels and firm characteristics, validating the matching procedure's effectiveness. I further estimate Equation 1 on the matched sample of firms: Table A6 shows that the DDD coefficient is estimated to be 1.13 for smaller firms. The economic magnitude of the effect is that following the MW hike, a 10-percentage point increase in %Bound leads to an 11.3% increase in CEO total pay for affected vs. unaffected smaller firms. Similar to the baseline results, the effect is reflected in the salary and incentive pay components of CEO compensation, and the effect is insignificant for larger firms.

6.2. Bordering counties sample

Another potential concern is that the MW policy may have been regulated to impact firms differently in bound vs. unbound states. If that is the case, firms in the main sample may not be comparable on the CEO pay dimension, and the endogeneity problem may lead to biased coefficient estimates. To address this issue, I apply the border analysis method used in the literature (e.g., Holmes (1998), Dube et al. (2010), Gustafson and Kotter (2022), etc.). It allows excluding the possibility that a potential confounding factor of state-level economic conditions is driving the results since firms headquartered on opposing sides of the state border, close to each other, are exposed to similar economic shocks.

I construct a sample of firms headquartered in counties located on the borders of contiguous states, following several steps. Firstly, I identify a sample of state pairs consisting of bordering states. Next, out of the main sample, I identify all bound firms belonging to one state of the state pair and all unbound firms belonging to a corresponding paired state. I further create a sample of bound and unbound firms that share a state border. Finally, I leave only firms headquartered in the counties located on the borders of state pairs and obtain a sample of 826 firms per period. Note that if a county of state A shares a border with states B and C counties, then firms headquartered in the county of state A will appear in the sample twice - one time for state B and one time for state C.

I further estimate Equation 1 on the bordering counties sample. Table A7 shows that the DDD coefficient is estimated to be 0.76 for smaller firms. The economic magnitude of the coefficient is that following a MW shock, a 10-percentage point increase in employment share in bound states leads to a 7.6% increase in CEO total pay for smaller, affected vs. unaffected firms that are plausibly subject to similar economic conditions. The result is insignificant for larger firms, and the magnitude of the CEO pay raise is similar to the baseline results for smaller firms. For smaller firms, the effects on cash pay components and incentive pay are not presented due to missing original data on these pay components in CEO disclosed pay.[29]

7. Conclusion

The pay gap between CEOs and rank-and-file employees has been growing over time, generating interest among academics and providing a base for media, political, and public debates. It is important to understand the relationship between CEO and employee pay since this link can help policymakers assess the efficacy of mandated worker wage increases to narrow the CEO-employee pay gap. However, empirical research on the topic is limited. I provide novel evidence on the spillover effects of employee pay on CEO pay, using a hike in U.S. federal MW level as a natural experiment, and attempt to disentangle the underlying mechanism.

[29] See Section A.1 in the Appendix for details on the definitions of CEO pay variables and interpretation of missing values.

Using a DDD specification that exploits the distribution of employees across states, I show that a 10-percentage point increase in the share of employees located in bound states leads to a 6.5% increase in CEO total pay for affected vs. unaffected firms post-MW shock. The result is more pronounced for smaller firms, and I provide evidence supporting the explanation that the proximity between CEO and employees in terms of their roles in smaller firms drives CEO pay increases. The result is robust to using a sample of firms matched by observable characteristics and a sample of firms headquartered in counties located along borders of contiguous states. Exploiting time-series variation in the MW law introduction stages, I estimate the elasticity of CEO pay with respect to MW to be 0.045. This estimate implies that the 41% increase in federal MW coming from the Fair Minimum Wage Act of 2007 led to a 1.84% increase in median small firm CEO total pay.

To justify the identification strategy, I show that the MW law has a positive effect on employee wages, ranging from 6% to 42% for affected vs. unaffected smaller firms, as a result of a 10-percentage point increase in the share of employment in bound states. At the same time, firm employment sees a relative reduction, ranging from 8% to 44% in magnitude. This evidence contributes to the debate on the effects of MW on employment in labor literature and is consistent with the competitive labor market view.

The results established in this paper are consistent with the fairness hypothesis of executive compensation (Edmans et al. (2021)): following an exogenous raise of employee pay, CEOs demand a corresponding fair raise in pay. Moreover, younger CEOs require a larger pay increase since current pay affects the present value of future pay more for them. Future research may extend the current analysis on granular employer-employee pay data[30] and examine the effects of exogenous CEO pay changes on CEO motivation, in accordance with a positive relationship predicted by the fairness hypothesis.

[30] For example, access to these data will allow for estimating the share of MW workers per firm.

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Tables and figures

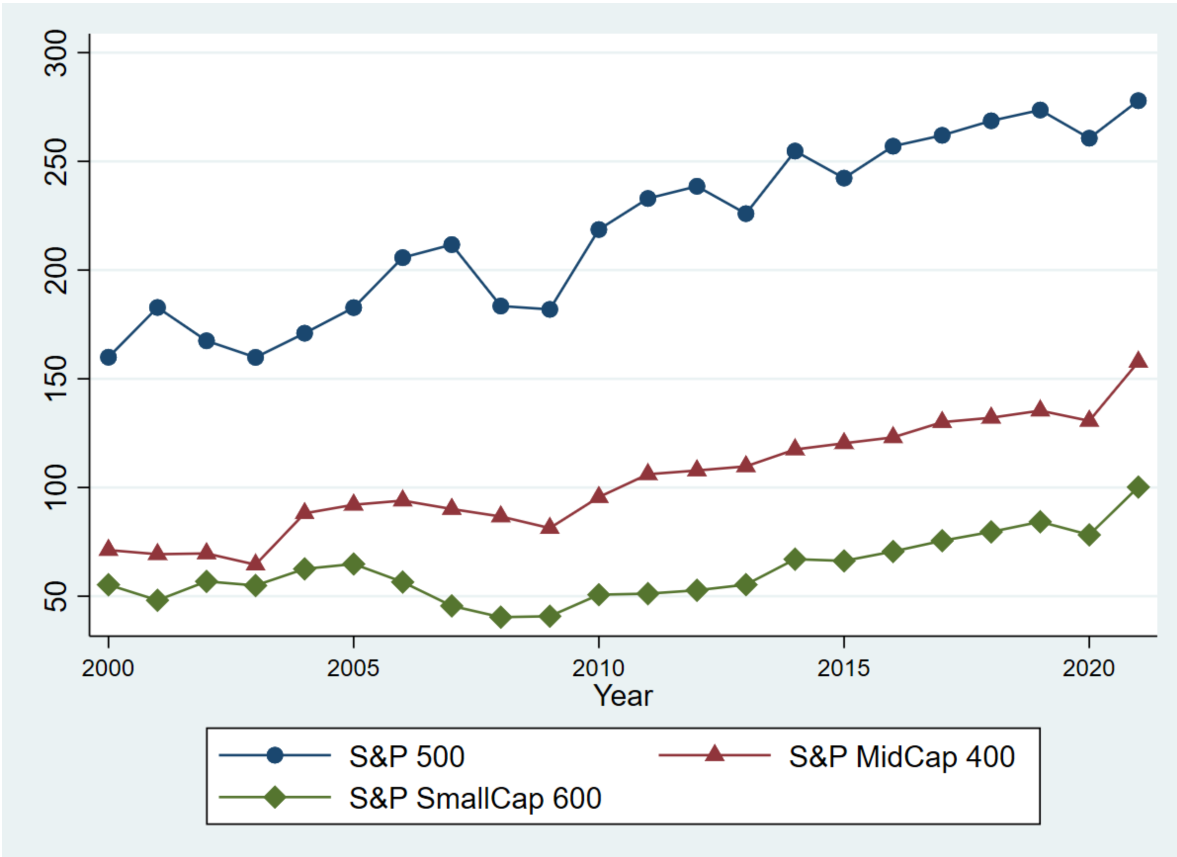


Figure 1: Evolution of the CEO-employee pay ratio, by firm size

The figure illustrates the trends in the CEO-employee pay ratio by firm size. Due to the lack of data on median employee pay by firm size, the ratio is defined as median CEO pay by firm size divided by aggregated median employee pay across firms. CEO pay data comes from Execucomp. Median employee pay data comes from the Bureau of Labor Statistics.

Table 1: Key features of the Fair Minimum Wage Act of 2007

The table displays the events corresponding to minimum wage increases triggered by the Fair Minimum Wage Act of 2007 and details of these increases. A discussion on the evolution of the minimum wage policy in the U.S. is found in Section 2.

Date of Enaction	Event number	Effective Date	Minimum Wage	Δ Min Wage %
		09/01/1997	\$5.15	8.42
05/25/2007	1	07/24/2007	\$5.85	13.59
	2	07/24/2008	\$6.55	11.97
	3	07/24/2009	\$7.25	10.69
			\$7.25	0.00

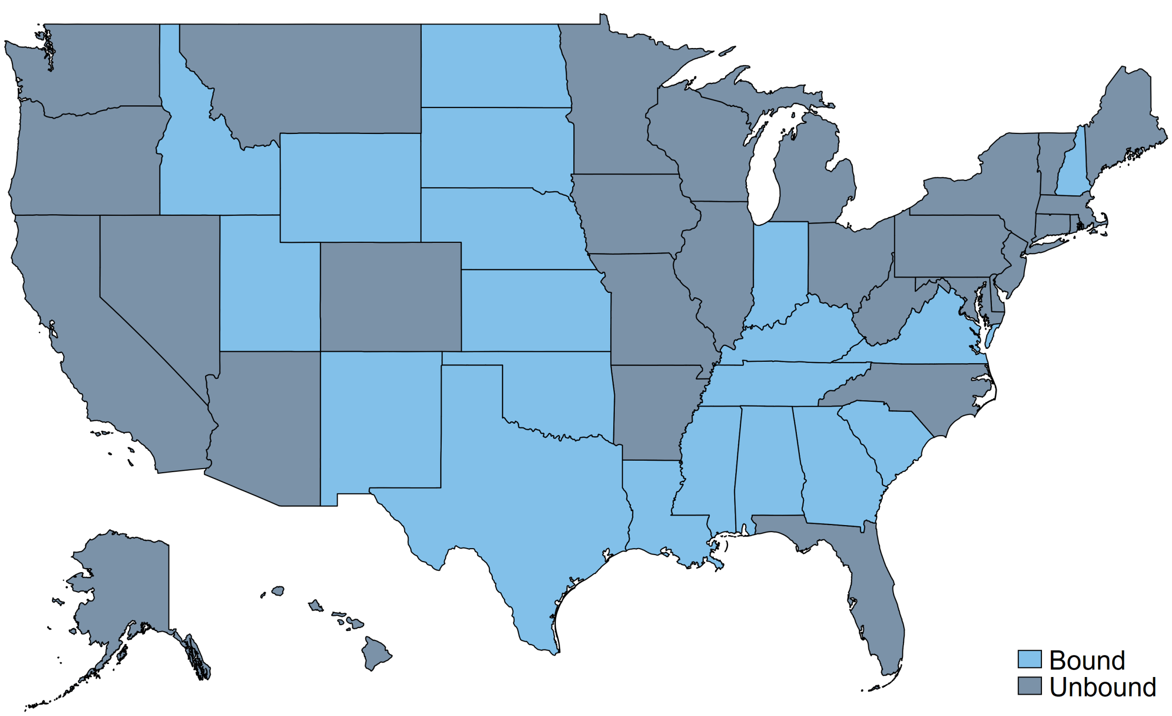
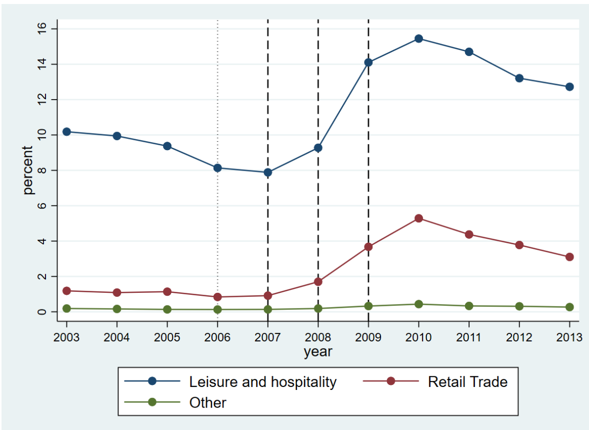
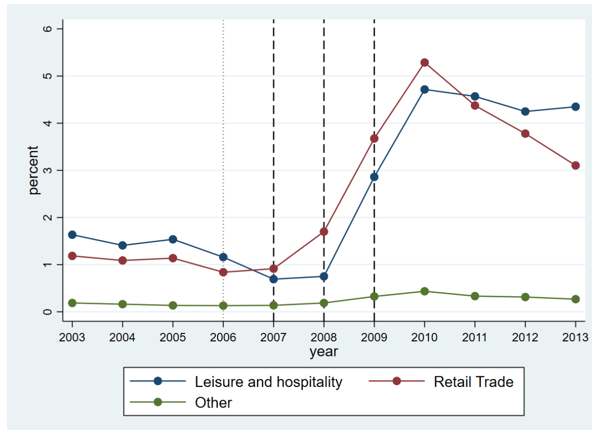


Figure 2: Map of bound and unbound states

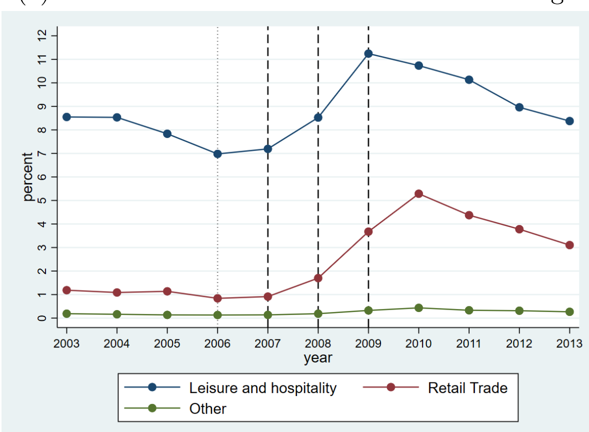
The figure illustrates the distribution of bound and unbound states. A state is bound if the state minimum wage is lower than the federal minimum wage. A state is unbound if the opposite holds. Since an employee receives the largest of state and federal minimum wage, the effect of the minimum wage increase policy would be reflected in outcomes for firms headquartered in bound states.



(a) Share of workers at or below minimum wage



(b) Share of workers at minimum wage



(c) Share of workers below minimum wage

Figure 3: Share of minimum wage workers, by industry

The figures above illustrate the relative share of minimum wage workers by industry. Panel (a) displays the share of workers at/below minimum wage vs. total employment by industry. Panels (b) and (c) display the same for the share of workers at and below minimum wage, respectively. Data on the number of minimum wage workers by industry comes from the Bureau of Labor Statistics, yearly characteristics of minimum wage workers reports. Data on the total employment by industry comes from the Bureau of Labor Statistics. The dotted line corresponds to the year previous to the minimum wage law introduction, while the dashed lines correspond to years of events 1, 2, and 3 (see Table 1).

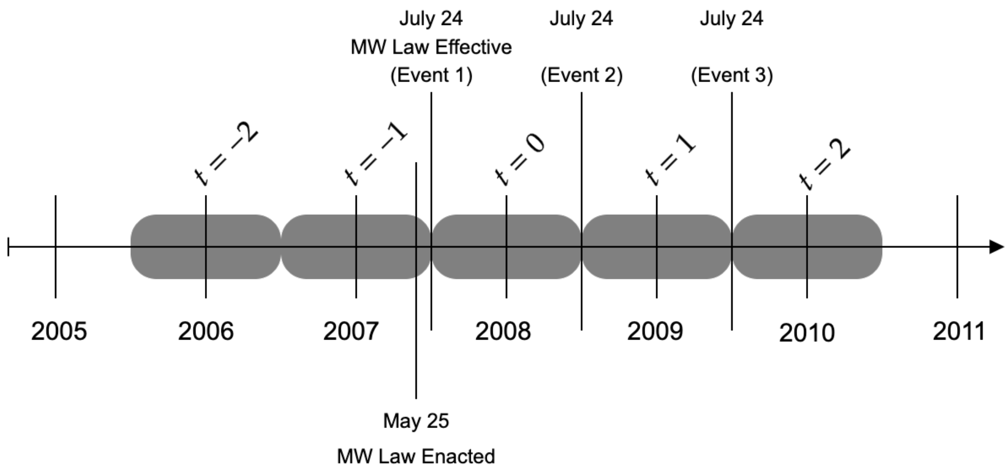


Figure 4: Timeline

The figure illustrates the timeline of the events associated with the MW law. Details on the events are provided in Table 1.

Table 2: Summary statistics

The table displays summary statistics for the main sample. Panels A, B, and C split the sample into all firms, smaller firms (bottom two terciles by employment at $t = -1$) and larger firms (top tercile by employment at $t = -1$). The main sample includes all firms with positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. Affected firms are defined as belonging to Leisure and hospitality (NAICS2 codes 71, 72) and Retail Trade (NAICS2 codes 44, 45) industries. All other variables are defined in Table A1. All lagged and non-lagged control variables are winsorized at the 0.5% and 99.5% levels.

Panel A: All firms

	Mean	Median	Std	5th	95th	N
Affected	0.051	0.000	0.221	0.000	1.000	8,660
%Bound	0.286	0.154	0.332	0.000	1.000	8,660
Bound	0.500	0.500	0.500	0.000	1.000	8,660
# employees	7,198.474	1,282.500	24,796.131	37.500	31,173.000	8,660
Total pay ('000)	1,953.383	608.787	4,108.388	52.020	8,412.156	8,660
Cash pay ('000)	820.770	467.035	1,403.597	132.750	2,539.647	5,422
Salary ('000)	442.233	350.000	321.039	107.000	1,037.499	6,267
Bonus ('000)	323.776	0.000	1,217.916	0.000	1,512.013	5,978
Incentive pay ('000)	1,914.411	395.844	4,319.146	0.000	8,953.165	5,422
Total assets ('000)	13,670.284	1,090.670	95,112.251	42.655	33,978.500	8,660
Size	7.062	6.995	1.977	3.752	10.436	8,659
Profitability	0.078	0.093	0.171	-0.146	0.271	8,659
ROA	0.016	0.029	0.170	-0.246	0.183	8,659
Q	1.774	1.306	1.292	0.865	4.224	8,659
M/B	1.769	1.299	1.292	0.854	4.222	8,659
log(1+annual return)	-0.050	0.018	0.542	-1.005	0.674	8,660
CEO age	57.084	57.000	9.226	43.000	73.000	8,581
CapIQ indicator	0.943	1.000	0.233	0.000	1.000	8,660
MSCI indicator	0.050	0.000	0.218	0.000	1.000	8,660
Execucomp indicator	0.006	0.000	0.079	0.000	0.000	8,660
ISS indicator	0.001	0.000	0.026	0.000	0.000	8,660
BoardEx indicator	0.000	0.000	0.015	0.000	0.000	8,660
Labor expense per employee ('000)	180.509	56.678	1,312.754	18.918	430.643	2,228
SG&A expense per employee ('000)	1,043.375	88.416	12,632.955	12.714	922.052	7,263
Operating expense per employee ('000)	5,431.372	355.164	74,818.478	73.817	3,267.563	8,659

Panel B: Smaller firms, bottom 2/3 sample by # employees

	Mean	Median	Std	5th	95th	N
Affected	0.027	0.000	0.162	0.000	0.000	5,775
%Bound	0.268	0.049	0.365	0.000	1.000	5,775
Bound	0.386	0.000	0.487	0.000	1.000	5,775
# employees	864.616	518.000	1,299.666	23.000	2,497.000	5,775
Total pay (′000)	1,025.370	438.080	2,140.369	40.752	3,823.366	5,775
Cash pay (′000)	525.026	373.253	621.640	114.877	1,386.310	3,479
Salary (′000)	336.589	284.073	224.840	96.961	750.000	4,060
Bonus (′000)	153.785	0.000	483.817	0.000	730.000	3,912
Incentive pay (′000)	864.693	184.430	2,292.767	0.000	3,886.960	3,479
Total assets (′000)	1,901.188	601.952	4,555.068	31.636	7,733.100	5,775
Size	6.302	6.400	1.648	3.454	8.953	5,774
Profitability	0.050	0.066	0.193	-0.256	0.262	5,774
ROA	-0.004	0.015	0.197	-0.345	0.196	5,774
Q	1.813	1.249	1.425	0.835	4.709	5,774
M/B	1.809	1.243	1.425	0.826	4.709	5,774
log(1+annual return)	-0.069	-0.006	0.568	-1.076	0.722	5,775
CEO age	56.888	57.000	9.670	42.000	74.000	5,710
CapIQ indicator	0.947	1.000	0.223	0.000	1.000	5,775
MSCI indicator	0.046	0.000	0.209	0.000	0.000	5,775
Execucomp indicator	0.006	0.000	0.075	0.000	0.000	5,775
ISS indicator	0.001	0.000	0.032	0.000	0.000	5,775
BoardEx indicator	0.000	0.000	0.013	0.000	0.000	5,775
Labor expense per employee ('000)	164.720	53.328	880.940	23.569	457.003	1,716
SG&A expense per employee ('000)	1,388.387	97.617	14,695.992	21.820	1,344.473	4,821
Operating expense per employee ('000)	7,559.963	349.939	90,900.417	84.953	4,178.741	5,774

Panel C: Larger firms, top 1/3 sample by # employees

	Mean	Median	Std	5th	95th	N
Affected	0.101	0.000	0.301	0.000	1.000	2,885
%Bound	0.322	0.280	0.248	0.009	0.877	2,885
Bound	0.728	1.000	0.445	0.000	1.000	2,885
# employees	19,877.165	8,479.000	40,018.960	3,044.000	68,885.000	2,885
Total pay ($'000$)	3,811.017	1,704.958	6,027.364	122.715	15,141.705	2,885
Cash pay ($'000$)	1,350.311	822.047	2,090.470	215.625	4,049.792	1,943
Salary ($'000$)	636.575	554.486	376.043	180.385	1,275.769	2,207
Bonus ($'000$)	645.658	0.000	1,921.362	0.000	2,800.000	2,066
Incentive pay ($'000$)	3,793.962	1,516.079	6,095.202	0.000	14,673.364	1,943
Total assets ($'000$)	37,228.874	4,593.600	162,132.288	421.937	114,837.000	2,885
Size	8.584	8.432	1.680	6.045	11.651	2,885
Profitability	0.134	0.127	0.090	0.013	0.283	2,885
ROA	0.055	0.053	0.081	-0.069	0.172	2,885
Q	1.694	1.397	0.970	0.914	3.466	2,885
M/B	1.688	1.390	0.969	0.905	3.441	2,885
log(1+annual return)	-0.014	0.054	0.485	-0.854	0.586	2,885
CEO age	57.473	57.000	8.258	44.000	72.000	2,871
CapIQ indicator	0.933	1.000	0.250	0.000	1.000	2,885
MSCI indicator	0.059	0.000	0.236	0.000	1.000	2,885
Execucomp indicator	0.008	0.000	0.087	0.000	0.000	2,885
ISS indicator	0.000	0.000	0.000	0.000	0.000	2,885
BoardEx indicator	0.000	0.000	0.019	0.000	0.000	2,885
Labor expense per employee ('000)	233.426	76.866	2,214.101	7.521	372.663	512
SG&A expense per employee ('000)	362.252	71.627	6,901.338	4.060	422.908	2,442
Operating expense per employee ('000)	1,171.239	364.892	15,418.699	34.597	1,911.178	2,885

Table 3: Did the minimum wage law raise employee pay?

The table displays the results of DDD regressions with the logarithm of labor expense per employee, selling, general, and administrative expense (SG&A) per employee, operating expense per employee, and employment as dependent variables. Columns (2), (4), and (6) use the same employment variable and correspond to different sample sizes. Employment data comes from YTS. Labor expense, operating expense, and SG&A expense are measured by *xlr*, *xopr*, and *xsga* variables from Compustat, respectively. In columns (1), (2), (5), and (6), the sample is main and includes all firms that have positive total pay observations and non-missing lagged control variable observations. In columns (3) and (4), all manufacturing firms are additionally excluded (since manufacturing firms do not disclose labor expense in the SG&A section of the income statement). All columns include firms with positive corresponding labor cost measure per employee and labor cost measure. Time horizon is $t \in [-2; 2]$. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Smaller firms, bottom 2/3 sample by # employees

	Labor expense		SG&A expense		Operating expense	
	(1) log(XLR per emp.)	(2) log(emp.)	(3) log(SG&A per emp.)	(4) log(emp.)	(5) log(XOPR per emp.)	(6) log(emp.)
PostXAffectedX%Bound	4.175***	-4.446***	0.640***	-0.754***	0.709***	-0.928***
	(8.03)	(-8.27)	(3.53)	(-3.15)	(2.84)	(-3.10)
PostXAffected	0.070	0.147	-0.294	0.326	-0.257	0.273
	(0.28)	(0.54)	(-0.79)	(0.91)	(-0.60)	(0.78)
PostX%Bound	0.033	-0.024***	0.056**	-0.034	-0.013	-0.034
	(1.36)	(-3.19)	(2.58)	(-1.43)	(-0.32)	(-1.11)
AffectedX%Bound	-18.096***	20.224***	-3.900***	4.361***	-4.291***	4.592***
	(-32.80)	(33.27)	(-3.20)	(3.41)	(-3.93)	(3.87)
Post	-0.011	-0.017	0.138	-0.111	0.072***	-0.042*
	(-1.54)	(-1.47)	(1.58)	(-1.48)	(2.93)	(-1.69)
%Bound	-0.223	-0.241***	-0.053	0.091	0.449**	-0.433**
	(-0.52)	(-5.92)	(-0.30)	(0.47)	(2.40)	(-2.36)
Constant	3.822***	2.804***	4.172***	3.489***	4.590***	4.233***
	(10.58)	(7.23)	(9.48)	(8.14)	(17.64)	(18.72)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.91	.96	.94	.95	.92	.94
N	1,440	1,440	2,815	2,815	5,765	5,765

Panel B: Larger firms, top 1/3 sample by # employees

	Labor expense		SG&A expense		Operating expense	
	(1) log(XLR per emp.)	(2) log(emp.)	(3) log(SG&A per emp.)	(4) log(emp.)	(5) log(XOPR per emp.)	(6) log(emp.)
PostXAffectedX%Bound	-0.270	-0.026	0.107	0.077	-0.101	0.020
	(-0.47)	(-0.05)	(0.37)	(0.28)	(-0.48)	(0.09)
PostXAffected	0.108	-0.105	-0.030	0.010	-0.006	0.008
	(0.48)	(-0.52)	(-0.27)	(0.10)	(-0.08)	(0.10)
PostX%Bound	-0.050	-0.051	0.114	-0.179	0.119	-0.133
	(-0.87)	(-0.83)	(0.54)	(-0.85)	(0.91)	(-1.07)
AffectedX%Bound	-3.029***	1.922**	-2.097	1.710	-1.123	0.616
	(-4.62)	(2.64)	(-1.46)	(1.07)	(-1.35)	(0.69)
Post	-0.001	0.138*	-0.007	0.047	-0.027	0.052
	(-0.01)	(1.93)	(-0.09)	(0.62)	(-0.55)	(1.16)
%Bound	0.547**	-0.452	1.828	-1.911	0.828	-0.821
	(2.44)	(-1.70)	(1.31)	(-1.24)	(1.16)	(-1.13)
Constant	4.183***	4.727***	3.361***	4.972***	4.510***	5.638***
	(4.07)	(9.17)	(5.26)	(8.62)	(7.97)	(13.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.96	.97	.96	.95	.95	.94
N	720	720	1,405	1,405	2,880	2,880

Panel C: All firms

	Labor expense		SG&A expense		Operating expense	
	(1) log(XLR per emp.)	(2) log(emp.)	(3) log(SG&A per emp.)	(4) log(emp.)	(5) log(XOPR per emp.)	(6) log(emp.)
PostXAffectedX%Bound	0.860	-1.148	0.523***	-0.559***	0.387**	-0.521***
	(0.63)	(-0.81)	(2.89)	(-2.84)	(2.49)	(-2.81)
PostXAffected	0.399**	-0.231**	-0.303**	0.292**	-0.230	0.223
	(2.64)	(-2.13)	(-2.28)	(2.19)	(-1.51)	(1.66)
PostX%Bound	0.017	-0.027**	0.077	-0.065	0.018	-0.061*
	(0.58)	(-2.09)	(1.59)	(-1.25)	(0.41)	(-1.80)
AffectedX%Bound	-13.255***	14.260***	-2.901***	3.019***	-3.312***	3.290***
	(-29.98)	(29.33)	(-2.81)	(2.76)	(-3.46)	(3.27)
Post	0.054	-0.079	0.093**	-0.069*	0.047**	-0.018
	(1.04)	(-1.36)	(2.07)	(-1.86)	(2.63)	(-1.07)
%Bound	0.063	-0.318**	0.335	-0.371	0.527**	-0.508**
	(0.24)	(-2.57)	(0.88)	(-0.92)	(2.49)	(-2.45)
Constant	4.071***	3.446***	4.081***	4.004***	4.571***	4.762***
	(7.97)	(8.75)	(9.60)	(9.68)	(17.99)	(20.16)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.93	.98	.94	.97	.92	.97
N	2,160	2,160	4,220	4,220	8,645	8,645

Table 4: The effects of the minimum wage law on CEO pay

The table displays the results of DDD regressions with the logarithm of total pay, 1+cash pay (i.e., 1+salary+bonus), 1+salary, 1+bonus, 1+incentive pay (i.e., 1+total pay-salary-bonus) as dependent variables. One is added to the CEO total pay component values to account for meaningful zero values. The sample is main and includes all firms with positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Smaller firms, bottom 2/3 sample by # employees

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.651***	0.079	0.498**	-2.100	4.575***
	(3.21)	(0.16)	(2.15)	(-1.08)	(4.39)
PostXAffected	-1.209**	-0.150	-0.470	0.573	-2.268
	(-2.27)	(-0.58)	(-1.18)	(0.48)	(-1.54)
PostX%Bound	-0.078	0.076	0.060	-0.044	-0.222
	(-1.56)	(1.26)	(1.20)	(-0.25)	(-0.82)
AffectedX%Bound	0.385	-1.928***	0.664*	1.036	10.493*
	(0.68)	(-2.87)	(1.92)	(0.14)	(1.85)
Post	0.276*	-0.076	0.042	-0.170	0.418
	(1.91)	(-0.62)	(0.52)	(-0.50)	(0.98)
%Bound	0.188	-0.082	-0.131	-0.064	0.230
	(1.05)	(-0.23)	(-0.41)	(-0.09)	(0.43)
Constant	4.431***	5.589***	4.878***	6.710***	1.531
	(9.12)	(12.57)	(12.46)	(4.86)	(1.39)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.45	.42	.36	.42	.56
N	5,775	3,479	4,060	3,912	3,479

Panel B: Larger firms, top 1/3 sample by # employees

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.641	0.777	0.617	-0.413	1.479
	(1.40)	(1.11)	(1.28)	(-0.22)	(0.70)
PostXAffected	0.208	-0.565*	-0.481**	-1.643*	3.822***
	(0.85)	(-1.81)	(-2.48)	(-1.81)	(4.10)
PostX%Bound	-0.164	-0.083	0.007	0.594	-0.655*
	(-0.95)	(-0.48)	(0.07)	(1.06)	(-1.67)
AffectedX%Bound	1.534	-1.076	-1.760*	2.664	-7.190
	(0.52)	(-1.05)	(-1.69)	(1.65)	(-1.48)
Post	-0.209	-0.056	-0.001	-0.139	0.675
	(-1.06)	(-0.38)	(-0.01)	(-0.25)	(1.49)
%Bound	0.080	-0.665**	-0.353	-3.581***	-1.254
	(0.17)	(-2.11)	(-1.52)	(-2.81)	(-1.35)
Constant	6.585***	7.457***	5.683***	7.421**	5.321**
	(4.89)	(5.53)	(6.14)	(2.07)	(2.50)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.39	.36	.27	.48	.53
N	2,885	1,943	2,207	2,066	1,943

Panel C: All firms

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.477*	0.340	0.470	-0.926	2.074*
	(1.93)	(0.77)	(1.61)	(-0.72)	(1.83)
PostXAffected	-0.550	-0.071	-0.400	-0.214	0.795
	(-1.09)	(-0.26)	(-1.28)	(-0.36)	(0.46)
PostX%Bound	-0.088	0.014	0.030	-0.072	-0.400
	(-1.64)	(0.29)	(0.69)	(-0.39)	(-1.57)
AffectedX%Bound	0.918	-0.470	0.134	1.940	-0.434
	(0.88)	(-0.52)	(0.36)	(0.80)	(-0.07)
Post	0.065	-0.064	0.017	-0.083	0.327
	(0.58)	(-0.61)	(0.28)	(-0.28)	(0.96)
%Bound	0.155	-0.074	-0.075	-0.352	0.102
	(0.89)	(-0.29)	(-0.30)	(-0.53)	(0.25)
Constant	4.828***	5.705***	5.017***	5.976***	2.203**
	(10.68)	(12.93)	(13.83)	(3.67)	(2.23)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.52	.49	.44	.43	.62
N	8,660	5,422	6,267	5,978	5,422

Table 5: Does CEO-employee proximity matter?

The table displays the results of DDD regressions with the logarithm of total pay, 1+cash pay (i.e., 1+salary+bonus), 1+salary, 1+bonus, 1+incentive pay (i.e., 1+total pay-salary-bonus) as dependent variables. One is added to the CEO total pay component values to account for meaningful zero values. The sample is main and includes all firms that have positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. The sample is split by the CEO-employee pay ratio into subsamples of firms with smaller pay ratio (i.e., belonging to the bottom two terciles of the sample at $t = -1$) in Panel A and firms with larger pay ratio (i.e., belonging to the top tercile of the sample at $t = -1$) in Panel B. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Bottom 2/3 sample by the CEO-employee pay ratio

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.724***	0.616	0.969**	0.798	2.003
	(3.30)	(0.61)	(2.19)	(0.25)	(0.91)
PostXAffected	-1.525***	-0.200	-0.453	0.267	-2.785**
	(-3.33)	(-0.70)	(-0.82)	(0.15)	(-2.25)
PostX%Bound	-0.113*	0.010	0.051	-0.189	-0.227
	(-1.85)	(0.13)	(1.05)	(-0.79)	(-1.00)
AffectedX%Bound	-0.625	-1.166	0.233	2.255	-12.683***
	(-0.69)	(-1.15)	(0.37)	(0.55)	(-3.86)
Post	0.494***	-0.046	0.081	0.093	0.529
	(2.85)	(-0.40)	(0.83)	(0.24)	(1.00)
%Bound	0.502	-0.286	-0.374	0.512	-0.348
	(1.60)	(-0.36)	(-0.64)	(0.66)	(-0.44)
Constant	3.771***	5.495***	4.574***	6.781***	0.300
	(8.84)	(9.61)	(11.29)	(3.96)	(0.23)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.44	.46	.39	.45	.56
N	3,900	2,299	2,687	2,630	2,299

Panel B: Top 1/3 sample by the CEO-employee pay ratio

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.791	-0.086	0.427	-3.312***	3.751***
	(1.31)	(-0.18)	(1.03)	(-3.40)	(5.65)
PostXAffected	-0.660	0.045	-0.582**	0.540	1.056
	(-0.80)	(0.16)	(-2.21)	(0.41)	(0.39)
PostX%Bound	-0.147	-0.052	0.002	0.267	0.052
	(-0.88)	(-0.43)	(0.03)	(0.39)	(0.18)
AffectedX%Bound	4.637***	-0.973	-0.521	4.722	10.125
	(3.14)	(-1.20)	(-0.47)	(0.87)	(1.52)
Post	0.016	-0.369	-0.190	-0.521	0.883
	(0.07)	(-1.40)	(-1.21)	(-0.55)	(1.33)
%Bound	-0.434	-0.508	-0.423	-1.790	-1.564
	(-1.12)	(-1.40)	(-1.33)	(-1.02)	(-1.55)
Constant	6.265***	5.852***	5.760***	8.243*	3.363
	(3.96)	(4.46)	(5.95)	(1.82)	(1.07)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.44	.35	.36	.46	.54
N	1,945	1,428	1,588	1,537	1,428

Table 6: Semi-elasticity of CEO pay with respect to minimum wages

The table displays the results of DDD regressions with the logarithm of total pay, 1+cash pay (i.e., 1+salary+bonus), 1+salary, 1+bonus, 1+incentive pay (i.e., 1+total pay-salary-bonus) as dependent variables. One is added to the CEO total pay component values to account for meaningful zero values. The sample is main and includes all firms that have positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Δ MW is the percentage change in MW level and is defined in Table 1. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. The coefficient on the DDD interaction represents the semi-elasticity of CEO pay with respect to minimum wages. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Smaller firms, bottom 2/3 sample by # employees

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
AffectedX%Δ MWX%Bound	0.0450**	0.0123	0.0445**	-0.1435	0.3698***
	(2.24)	(0.34)	(2.54)	(-0.96)	(5.09)
AffectedX%Δ MW	-0.0792*	-0.0047	-0.0340	0.0477	-0.1681
	(-1.86)	(-0.22)	(-1.07)	(0.56)	(-1.44)
%Δ MWX%Bound	-0.0053	0.0051	0.0043	-0.0061	-0.0168
	(-1.13)	(1.12)	(1.10)	(-0.44)	(-0.78)
AffectedX%Bound	0.4133	-1.9429***	0.6176*	1.1611	10.0900*
	(0.71)	(-2.85)	(1.78)	(0.16)	(1.81)
%Δ MW	0.0144	-0.0079	0.0005	-0.0166	0.0427
	(1.48)	(-0.90)	(0.08)	(-0.73)	(1.42)
%Bound	0.1818	-0.0775	-0.1278	-0.0548	0.2247
	(1.01)	(-0.22)	(-0.40)	(-0.08)	(0.42)
Constant	4.4905***	5.5912***	4.8951***	6.7159***	1.4889
	(9.39)	(12.68)	(12.47)	(4.82)	(1.37)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.45	.42	.35	.42	.56
N	5,775	3,479	4,060	3,912	3,479

Panel B: Larger firms, top 1/3 sample by # employees

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
AffectedX%Δ MWX%Bound	0.0308	0.0621	0.0486	-0.0438	0.1109
	(0.82)	(1.00)	(1.16)	(-0.27)	(0.60)
AffectedX%Δ MW	0.0219	-0.0363	-0.0332**	-0.1063	0.2410***
	(1.17)	(-1.37)	(-2.03)	(-1.45)	(3.05)
%Δ MWX%Bound	-0.0134	-0.0075	-0.0004	0.0440	-0.0506
	(-0.94)	(-0.52)	(-0.04)	(0.97)	(-1.62)
AffectedX%Bound	1.5979	-1.0422	-1.7464	2.6341	-7.1087
	(0.53)	(-0.98)	(-1.64)	(1.65)	(-1.46)
%Δ MW	-0.0174	-0.0043	-0.0005	-0.0154	0.0580*
	(-1.22)	(-0.41)	(-0.07)	(-0.40)	(1.79)
%Bound	0.0814	-0.6619**	-0.3463	-3.5439***	-1.2948
	(0.17)	(-2.10)	(-1.49)	(-2.80)	(-1.40)
Constant	6.5850***	7.4416***	5.6791***	7.4172**	5.3742**
	(4.89)	(5.55)	(6.15)	(2.07)	(2.53)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.39	.36	.27	.48	.53
N	2,885	1,943	2,207	2,066	1,943

Panel C: All firms

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
AffectedX%Δ MWX%Bound	0.0242	0.0260	0.0394	-0.0788	0.1751*
	(1.07)	(0.72)	(1.63)	(-0.76)	(1.80)
AffectedX%Δ MW	-0.0331	-0.0008	-0.0294	0.0022	0.0374
	(-0.86)	(-0.04)	(-1.26)	(0.05)	(0.30)
%Δ MWX%Bound	-0.0063	0.0004	0.0018	-0.0082	-0.0318
	(-1.40)	(0.11)	(0.53)	(-0.55)	(-1.54)
AffectedX%Bound	0.9849	-0.4708	0.1216	1.8904	-0.3769
	(0.91)	(-0.52)	(0.32)	(0.78)	(-0.06)
%Δ MW	0.0005	-0.0067	-0.0007	-0.0119	0.0356
	(0.06)	(-0.94)	(-0.15)	(-0.59)	(1.51)
%Bound	0.1496	-0.0709	-0.0712	-0.3445	0.0932
	(0.86)	(-0.28)	(-0.29)	(-0.52)	(0.23)
Constant	4.8608***	5.7080***	5.0281***	5.9949***	2.1760**
	(10.87)	(13.00)	(13.85)	(3.68)	(2.22)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.52	.49	.44	.43	.62
N	8,660	5,422	6,267	5,978	5,422

Table 7: The effects of the minimum wage law on CEO-employee pay ratio

The table displays the results of DDD regressions with the natural logarithm of the CEO-employee pay ratio as a dependent variable. In columns (1)-(3), the pay ratio is based on the original labor expense *xlr*. In columns (4)-(6), the pay ratio is imputed following Donangelo (2016) (see Section 4.5 for more details). The sample is main and includes all firms that have positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

	log(Pay ratio) based on XLR			log(Pay ratio) based on imputation		
	(1) Small firms	(2) Large firms	(3) All firms	(4) Small firms	(5) Large firms	(6) All firms
PostXAffectedX%Bound	-4.500***	3.847***	0.050	-0.661	2.283***	0.278
	(-10.42)	(3.63)	(0.03)	(-1.09)	(3.42)	(0.53)
PostXAffected	-0.757**	-1.487	-1.433***	-1.580***	-0.431	-0.795**
	(-2.73)	(-1.59)	(-5.40)	(-3.68)	(-1.22)	(-2.26)
PostX%Bound	-0.216***	0.017	-0.166***	-0.126*	-0.081	-0.154**
	(-13.24)	(0.16)	(-4.58)	(-1.83)	(-0.31)	(-2.08)
AffectedX%Bound	18.301***	13.385***	12.784***	2.658	4.152	2.398
	(34.08)	(6.25)	(13.92)	(1.37)	(1.27)	(0.95)
Post	0.089	-0.532	0.106	0.679***	-0.067	0.375**
	(0.55)	(-0.65)	(0.54)	(3.45)	(-0.27)	(2.48)
%Bound	0.625	0.446	0.368	0.306	-1.029	0.090
	(1.06)	(0.24)	(0.47)	(0.88)	(-1.36)	(0.30)
Constant	2.175**	-0.938	1.603	0.913	0.042	0.723
	(2.21)	(-0.28)	(1.18)	(1.27)	(0.03)	(1.09)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.5	.5	.58	.48	.55	.56
N	1,440	720	2,160	3,900	1,945	5,845

Table 8: Mechanism: Fairness hypothesis

The table displays the results of DDD regressions with the logarithm of total pay, 1+cash pay (i.e., 1+salary+bonus), 1+salary, 1+bonus, 1+incentive pay (i.e., 1+total pay-salary-bonus) as dependent variables. One is added to the CEO total pay component values to account for meaningful zero values. The sample is main and includes all small firms that have positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. The sample is split by CEO age into subsamples of firms with younger CEOs (i.e., belonging to the bottom half of the sample at $t = -1$) in Panel A and firms with older CEOs (i.e., belonging to the top half of the sample at $t = -1$) in Panel B. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Younger CEOs of smaller firms, bottom 1/2 sample by age

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	1.018***	3.488***	3.173***	12.351***	12.476***
	(7.13)	(5.00)	(19.46)	(11.37)	(9.67)
PostXAffected	-1.720***	-3.254***	-2.952***	-11.985***	-13.944***
	(-13.07)	(-4.49)	(-15.62)	(-10.66)	(-12.26)
PostX%Bound	0.029	0.225**	0.197**	-0.092	-0.101
	(0.24)	(2.39)	(2.59)	(-0.49)	(-0.32)
AffectedX%Bound	0.516	-5.906***	-1.984***	-24.571***	-2.493*
	(0.41)	(-8.26)	(-4.56)	(-17.07)	(-1.81)
Post	-0.153	-0.115	0.005	-0.414	0.542
	(-1.19)	(-0.66)	(0.05)	(-0.91)	(1.56)
%Bound	0.083	0.479***	0.099	0.423	-0.431
	(0.26)	(2.76)	(0.52)	(0.43)	(-0.61)
Constant	4.895***	5.573***	5.246***	5.887***	2.907*
	(7.83)	(14.12)	(14.18)	(4.12)	(1.97)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.44	.42	.37	.38	.57
N	2,945	1,884	2,220	2,065	1,884

Panel B: Older CEOs of smaller firms, top 1/2 sample by age

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.405**	-1.435***	-0.569***	-1.519	4.959***
	(2.10)	(-7.60)	(-3.22)	(-1.39)	(9.22)
PostXAffected	-1.630*	-0.276**	-0.355***	-0.036	-3.337***
	(-1.93)	(-2.23)	(-2.89)	(-0.05)	(-3.72)
PostX%Bound	-0.099	-0.053	-0.103	0.083	-0.095
	(-1.00)	(-0.46)	(-1.16)	(0.27)	(-0.21)
AffectedX%Bound	-0.842	-1.365	-2.492**	20.154***	8.345***
	(-0.32)	(-1.28)	(-2.58)	(10.24)	(5.86)
Post	0.633***	-0.228*	-0.000	0.143	0.882
	(3.45)	(-1.80)	(-0.00)	(0.24)	(0.99)
%Bound	-0.115	-1.433*	-1.233*	-0.952	1.355
	(-0.31)	(-1.83)	(-1.71)	(-1.25)	(1.20)
Constant	4.101***	6.245***	5.050***	6.440***	-0.498
	(5.89)	(8.11)	(6.83)	(3.14)	(-0.28)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.46	.39	.34	.45	.56
N	2,605	1,454	1,667	1,694	1,454

Table 9: Mechanism: Bargaining hypothesis

The table displays the results of DDD regressions with the logarithm of total pay, 1+cash pay (i.e., 1+salary+bonus), 1+salary, 1+bonus, 1+incentive pay (i.e., 1+total pay-salary-bonus) as dependent variables. One is added to the CEO total pay component values to account for meaningful zero values. The sample is main and includes all small firms that have positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. The sample is split by firm industry-adjusted ROA into subsamples of high-rent firms (i.e., belonging to the top half of the sample at pre-treatment periods) in Panel A and low-rent firms (i.e., belonging to the bottom half of the sample at pre-treatment periods) in Panel B. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: High-rent smaller firms, top 1/2 sample by industry-adjusted ROA

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.536	1.587	0.655	5.085*	7.567
	(1.50)	(0.62)	(0.57)	(1.75)	(0.88)
PostXAffected	0.710	0.075	0.565	-0.825	0.672
	(1.26)	(0.10)	(1.60)	(-0.83)	(0.26)
PostX%Bound	-0.060	0.103	0.050	0.293	-0.255
	(-0.54)	(1.28)	(0.73)	(0.86)	(-0.62)
AffectedX%Bound	3.860***	2.564	6.469***	-8.188	24.081*
	(2.99)	(0.48)	(2.70)	(-0.99)	(1.76)
Post	-0.019	-0.203	-0.087	-0.578	0.828
	(-0.09)	(-1.50)	(-0.82)	(-1.31)	(1.30)
%Bound	0.286	-0.648	-0.658	-0.204	0.594
	(1.24)	(-1.21)	(-1.40)	(-0.22)	(0.78)
Constant	4.894***	6.011***	5.278***	4.117**	0.992
	(8.98)	(14.39)	(20.93)	(2.50)	(0.99)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.46	.36	.3	.43	.56
N	2,885	1,745	2,050	1,928	1,745

Panel B: Low-rent smaller firms, bottom 1/2 sample by industry-adjusted ROA

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.891	-0.354	0.261	-4.650**	3.986***
	(1.65)	(-0.73)	(1.05)	(-2.55)	(4.66)
PostXAffected	-2.554***	-0.211	-0.582**	0.662	-3.452**
	(-5.13)	(-1.24)	(-2.43)	(1.03)	(-2.34)
PostX%Bound	-0.087	0.038	0.075	-0.411**	-0.112
	(-0.67)	(0.36)	(0.79)	(-2.31)	(-0.49)
AffectedX%Bound	0.689	-3.469***	-0.303	4.798	11.033*
	(1.24)	(-3.86)	(-0.76)	(0.89)	(1.98)
Post	0.506**	0.198*	0.213***	0.254	-0.549
	(2.63)	(1.83)	(2.98)	(0.60)	(-1.42)
%Bound	0.093	0.760**	0.509***	0.088	-0.623
	(0.27)	(2.64)	(2.80)	(0.09)	(-0.67)
Constant	3.496***	4.527***	4.388***	6.175***	2.544*
	(5.79)	(6.79)	(6.60)	(4.03)	(1.99)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.43	.44	.37	.38	.55
N	2,885	1,729	2,005	1,979	1,729

Table 10: Mechanism: Efficiency wage hypothesis

The table displays the results of DDD regressions with firm industry-adjusted ROA and logarithm of 1+annualized return as dependent variables. The sample is main and includes all firms that have positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

	Industry-adjusted ROA			log(1+annualized return)		
	(1) Small firms	(2) Large firms	(3) All firms	(4) Small firms	(5) Large firms	(6) All firms
PostXAffectedX%Bound	0.040	-0.023	0.012	0.221	-0.500**	-0.203
	(0.71)	(-0.82)	(0.36)	(0.77)	(-2.21)	(-1.50)
PostXAffected	0.054	0.047***	0.033	0.001	0.388***	0.093
	(1.30)	(2.86)	(0.98)	(0.00)	(4.06)	(0.50)
PostX%Bound	-0.002	-0.004	-0.002	0.075*	-0.013	0.066*
	(-0.16)	(-0.34)	(-0.16)	(1.75)	(-0.19)	(1.83)
AffectedX%Bound	0.011	-0.015	-0.005	-1.122***	0.524	-0.523
	(0.25)	(-0.37)	(-0.11)	(-3.31)	(0.99)	(-1.55)
Post	-0.017*	-0.018	-0.015**	0.112	0.093*	0.106**
	(-1.73)	(-1.38)	(-2.42)	(1.55)	(1.91)	(2.18)
%Bound	0.007	0.032	0.014	-0.109	-0.105	-0.100
	(0.43)	(1.38)	(1.10)	(-1.19)	(-0.65)	(-1.38)
Constant	0.163**	-0.078	0.129**	2.845***	2.953***	2.945***
	(2.64)	(-1.10)	(2.17)	(10.34)	(6.53)	(11.48)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.6	.72	.61	.45	.51	.46
N	5,770	2,885	8,655	5,775	2,885	8,660

A. Appendix

A.1. Constructing executive pay datasets

I use five executive compensation datasets to construct CEO pay variables: Capital IQ, MSCI, Execucomp, ISS Incentive Lab, and BoardEx.

Following Bloom et al. (2021), I define the highest-paid executive in a given year to be CEO. I require CEOs to have positive total pay, following Matveyev (2017), since zero CEO pay observations do not convey meaningful information. I use the firm identifier and fiscal year to link CEO pay data to the Compustat sample.

Since every dataset has slightly different definitions of pay components, in order to maintain consistency in the data, I use the following general principle throughout the sample construction:

1. Identify total pay variable
2. Impute missing salary and bonus variables from other compensation components (if possible)
3. Define cash pay as the sum of salary and bonus
4. Define incentive pay as the difference between total pay and cash pay
5. Keep only positive total pay observations, and non-negative or missing salary, bonus, cash pay, incentive pay observations

I use salary and bonus as a basis for splitting the total pay since salary and bonus are defined consistently within the different datasets. Most inconsistencies come from definitions of incentive pay (e.g., stock awards, option awards, long-term incentive plan), hence I assign total compensation in excess of cash pay to incentive pay. If, after going through Steps 1-5, the observation for any of the pay components (i.e., salary, bonus, cash pay, incentive pay) is still missing, I keep it as missing in the sample.

This section describes the detailed definitions I use to construct pay variables within each dataset and presents Steps 1 and 2 from the list above. Steps 3, 4, and 5 are identical for each dataset, so I omit them to save space. Variables in **bold** are constructed, while variables in *italics* are available in compensation datasets.

A.1.1. Capital IQ

1. (a) I start with defining **total pay** = *CTYPE23*, and impute it with first *CTYPE18*, and second *CTYPE30* if missing.
   (b) I define **bonus** = *CTYPE2*, and impute it with *CTYPE51* if missing.
   (c) I define **salary** = *CTYPE1*.
2. N/A

I divide **total pay**, **salary**, **bonus**, **cash pay**, **incentive pay** by 1000 in order to make the units consistent among datasets.

A.1.2. MSCI

Another difficulty in dealing with MSCI compensation data is that the reporting format has changed over time.

For years 2004, 2005, 2006, old disclosures of 2007 and old disclosures of 2008:

1. I start with defining **total pay** = *CEOTOTALANNUALCOMP*, **salary** = *CEOBASESALARY*, **bonus** = *CEOANNUALBONUS*.
2. (a) I impute **bonus** to be equal zero if **bonus** is missing and **salary**, *CEOOTHERANNUALCOMP*, **total pay** are non-missing.
   (b) I impute **salary** to be equal zero if **salary** is missing and **bonus**, *CEOOTHERANNUALCOMP*, **total pay** are non-missing.
   (c) I replace **bonus** = **total pay** - *CEOOTHERANNUALCOMP* - **salary** if **bonus** is missing.
   (d) I replace **salary** = **total pay** - *CEOOTHERANNUALCOMP* - **bonus** if **salary** is missing.

For new disclosures of 2007 onwards:

1. I start with defining **total pay** = *CEOTOTSUMCOMP*, **salary** = *CEOBASESALARY*, **bonus** = *CEOBONUS*.
2. (a) I impute **bonus** to be equal zero if **bonus** is missing and **salary**, *CEOSTOCKAWARDS*, *CEOOPTIONAWARDS*, *CEONONEQINCENTCOMP*, *CEOALLOTHERCOMP*, *CEOPENSIONNQDC*, **total pay** are non-missing.
   (b) I impute **salary** to be equal zero if **salary** is missing and **bonus**, *CEOSTOCKAWARDS*, *CEOOPTIONAWARDS*, *CEONONEQINCENTCOMP*, *CEOALLOTHERCOMP*, *CEOPENSIONNQDC*, **total pay** are non-missing.
   (c) I replace **bonus** = **total pay** - **salary** - *CEOSTOCKAWARDS* - *CEOOPTIONAWARDS* - *CEONONEQINCENTCOMP* - *CEOALLOTHERCOMP* - *CEOPENSIONNQDC* if **bonus** is missing.
   (d) I replace **salary** = **total pay** - **bonus** - *CEOSTOCKAWARDS* - *CEOOPTIONAWARDS* - *CEONONEQINCENTCOMP* - *CEOALLOTHERCOMP* - *CEOPENSIONNQDC* if **salary** is missing.

I divide **total pay**, **salary**, **bonus**, **cash pay**, **incentive pay** by 1000 in order to make the units consistent among datasets.

A.1.3. Execucomp

In constructing pay variables from Execucomp, I follow the methodology described in the Internet Appendix of Matveyev (2017).

1. (a) I start with defining **total pay** = *TDC1* for years up to 2005.
   (b) I replace **total pay** = **TDC2** for years up to 2005 if *TDC1* is missing.
   (c) I replace **total pay** = *TOTAL_SEC* for years 2006 onwards.
   (d) I replace **total pay** = *TDC1* for years 2006 onwards, if *TOTAL_SEC* is zero or missing, and *TDC1* is positive.
   (e) I replace **total pay** = *TDC2* for years 2006 onwards, if *TOTAL_SEC*, *TDC1* are missing.
   (f) I define **bonus** = *BONUS* and **salary** = *SALARY*.
2. (a) I impute **bonus** to be equal to zero if **bonus** is missing and *TOTAL_CURR* is non-missing.
   (b) I impute **salary** to be equal to zero if **salary** is missing and *TOTAL_CURR* is non-missing.
   (c) I replace **bonus** = *TOTAL_CURR* - **salary** if **bonus** is missing.
   (d) I replace **salary** = *TOTAL_CURR* - **bonus** if **salary** is missing.

A.1.4. ISS

1. I start with defining **total pay** = *totalComp*, **bonus** = *bonus* and **salary** = *salary*.
2. N/A

I divide **total pay**, **salary**, **bonus**, **cash pay**, **incentive pay** by 1000 in order to make the units consistent among datasets.

A.1.5. BoardEx

1. I start with defining **total pay** = *TotRemPeriod*, **salary** = *Salary*, **bonus** = *Bonus*.
2. (a) I impute **bonus** to be equal zero if **bonus** is missing and *TotalCompensation* is non-missing.
   (b) I impute **salary** to be equal zero if **salary** is missing and *TotalCompensation* is non-missing.
   (c) I replace **bonus** = *TotalCompensation* - **salary** if **bonus** is missing.
   (d) I impute **salary** = *TotalCompensation* - **bonus** if **salary** is missing.

A.2. Construction of the CEO-employee pay ratios

I follow the labor-finance literature (e.g., Faleye et al. (2013)) and proxy for average employee pay by $(xlr - \textit{Total CEO pay})/\#\textit{employees}$, where *xlr* is total staff expense from Compustat, and *#employees* is the number of workers in all establishments of a firm from YTS.[31] I define CEO-employee pay ratio as *total CEO pay/Average employee pay*.

I follow the *xlr* imputation procedure developed by Donangelo (2016).[32] I first define industries based on FF-17 codes, using SIC2 where FF-17 code is unavailable. Next, I separate firms into firm size buckets every year and create imputation groups based on industry and size buckets, following Hartman-Glaser, Lustig, and Xiaolan (2019), who use the approach to estimate labor costs for U.S. public firms. I use five size buckets for imputation to ensure a balanced firm distribution among the groups based on the relative distribution of firms by FF-17 industries in the main sample. This procedure results in the definition of average employee pay given in Equation 3:

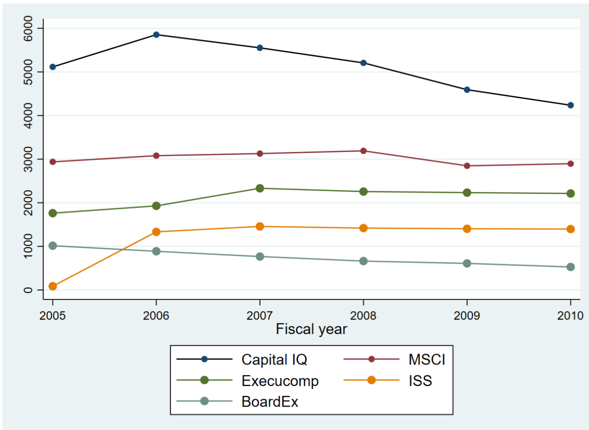
$$\text{Average employee pay} = \begin{cases} \text{(xlr - Total CEO pay)/\# employees, } \textit{if xlr is non-missing} \\ \text{average ((xlr - Total CEO pay)/\# employees),} \\ \qquad\qquad \text{by industry and firm size bucket, } \textit{if xlr is missing} \end{cases} \tag{3}$$

I keep only firms with non-missing observations for each $t = [-2; 2]$ period. Table A2 shows descriptive statistics for the CEO-employee pay ratio and its components for the main sample: based on the imputation procedure in Panel A and based on the original *xlr* variable in Panel B. By definition of Equation 3, average employee pay is the average pay of all employees except for the CEO, including non-CEO top executives. Moreover, *xlr* includes incentive compensation, other benefit plans, payroll taxes, pension costs, and profit sharing (when included in staff expense by the company) on top of employee salaries and wages. These two facts imply that the statistics for average employee pay are higher than median worker pay numbers cited in the media, which do not include top executives' pay and other labor expense apart from employee wages (Faleye et al. (2013)). However, the distribution of the CEO-employee pay ratio is similar before and after the imputation, indicating that the imputation procedure provides a reasonable approximation of CEO pay ratio values for the main sample while increasing the sample size by over 2.5 times.

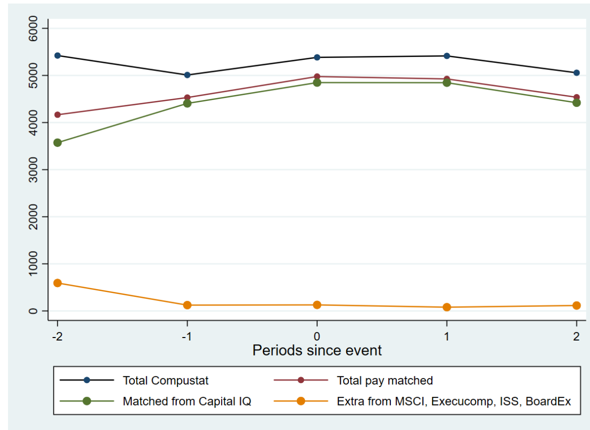
[31] In September 2013, the SEC meeting resulted in a majority vote for approval of Section 953(b) of the Dodd-Frank Act, which required all public firms filing annual reports with the SEC to disclose their CEO-to-median employee pay ratios. The final version of the rule was adopted in August 2015, and each firm was obliged to provide data on the CEO-employee pay ratio starting in its first full fiscal year beginning on or after January 1, 2017. However, in the time horizon surrounding the Fair Minimum Wage Act of 2007, the rule was not yet proposed by the SEC: the event window I use in this paper covers years from 2005 to 2011 (See Figure 4).

[32] In the main sample, *xlr* variable is available only for 25.7% of firm-years. This happens because the firms are not obliged to disclose their labor expenses, so firms with available data self-select to disclose it. To overcome the drawback of missing data, I employ the imputation procedure.

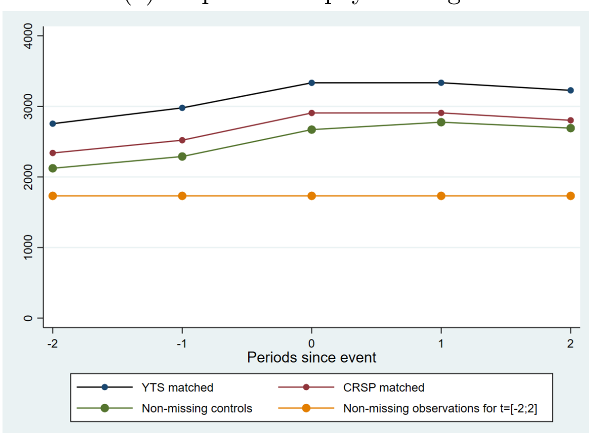
A.3. Tables and figures



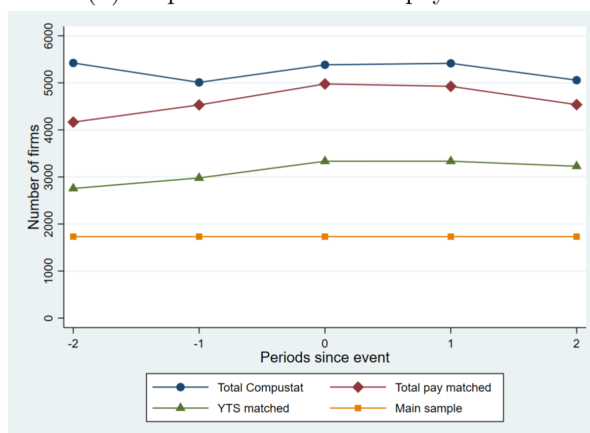
(a) Step 1: CEO pay coverage



(b) Step 2: Match to CEO pay data



(c) Step 3: Match to YTS and other filters



(d) Summary: Main sample

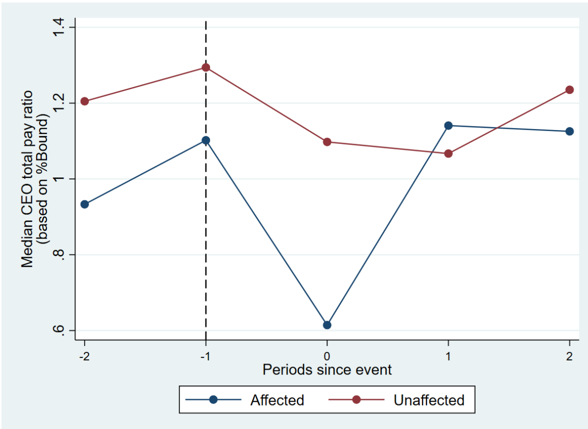
Figure A1: Sample construction

The figures above illustrate the sample construction used in the paper. Panel (a) displays the population of U.S. public firms each fiscal year with available CEO pay data by dataset. Panel (b) displays the number of U.S. public firms covered by Compustat that are headquartered in the U.S. and have non-missing total assets data for at least two periods before and after July 24, 2007 ("Total Compustat"), the number of firms matched to CEO pay datasets ("Total pay matched"), and the distribution of matched firms by dataset ("from Capital IQ" and "Extra from MSCI, Execucomp, ISS, BoardEx"). Panel (c) shows the number of firms that are matched to YTS data ("YTS matched"), that are covered by CRSP ("CRSP matched"), that have non-missing controls, and that have non-missing observations for each of the periods $t \in [-2; 2]$. Panel (d) summarizes the sample construction algorithm presented in Panels (a), (b), and (c): the main sample used throughout the body of the paper is illustrated by the "Main sample" line.

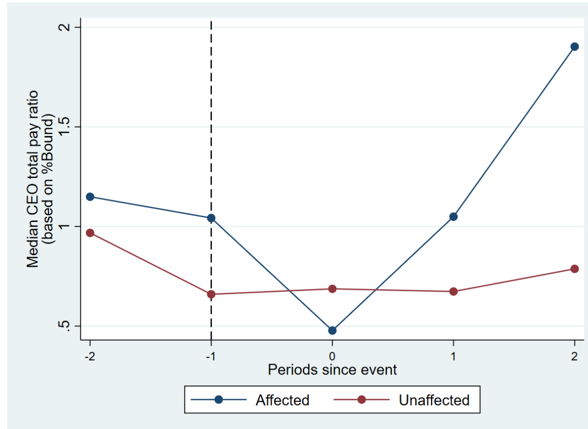
Table A1: Variable definitions

The table shows definitions of all variables used throughout the analysis, a corresponding reference in the literature, and the data source.

Variable	Definition	Reference
Affected	1 if firm belongs to NAICS2 industries 71, 72, 44, 45 (Leisure and Hospitality, Retail Trade)	Dai and Qiu (2022), Chava et al. (2019), Gustafson and Kotter (2022), personal calculations from the BLS data
%Bound	% of employees located in bound states establishments	Source: YTS
Bound	1 if %Bound $\geq$ 15.34% (sample median) at $t = -1$	Source: YTS
XLR	Total labor expense, *xlr*	Source: Compustat
XOPR	Operating expense, *xopr*	Source: Compustat
SG&A	Selling, general and administrative expense, *xsga*	Source: Compustat
Post	1 if t $\geq$ 0	Source: YTS
M/B	$(dltt + dlc + csho * \lvert prcc_f \rvert)/at$	Source: Compustat
Size	$log(at)$	Source: Compustat
ROA	$ni/at[n-1]$	Source: Compustat
Profitability	$(oibdp/at)$	Source: Compustat
Q	$(at + (prcc_f * csho) - ceq - txdb)/at$	Source: Compustat
Total pay, Salary, Bonus	See Appendix	Source: CEO pay datasets
Cash pay	Salary + Bonus	Source: CEO pay datasets
Incentive pay	Total pay - Salary - Bonus	Source: CEO pay datasets
Small/Large (by variable)	1 if firm belongs to the bottom two/top one terciles of the sample at $t = -1$ (by variable)	
Industry	NAICS3 code at $t = -2$	Source: Compustat
%Δ MW	percentage change in MW	See Table 1
Average employee pay	(XLR - Total CEO pay)/# employees if XLR is non-missing average (average employee pay) by FF-17 or SIC2 and size bucket, if XLR is missing	Donangelo (2016), Faleye et al. (2013)
CEO-employee pay ratio	Total CEO pay/Average employee pay	
Younger/Older CEO	1/0 if CEO age belongs to the bottom/top half of the sample at $t = -1$	
High-rent/Low-rent	1/0 if average industry-adj. ROA for $t \in [-2; -1]$ belongs to the top/bottom half of the sample	



(a) Smaller firms



(b) Larger firms

Figure A2: DDD identification: Parallel pre-trends

The figures above illustrate the evolution of the ratio of the median affected (unaffected), high-level of %Bound firm CEO total pay divided by the median affected (unaffected), low-level of %Bound firm CEO total pay over time. Panels (a) and (b) correspond to smaller and larger firms of the main sample, respectively. The dashed line corresponds to $t = -1$, separating pre- and post-treatment periods. Parallel trends in ratios for affected and unaffected firms to the left of the dashed line indicate that the DDD identification assumption is satisfied (Olden and Møen (2022)).
The main sample includes all firms that have positive total pay observations and non-missing lagged control variable observations. Affected firms belong to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade). A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. %Bound is equal to the share of employees located in bound states (where federal MW is greater than or equal to state MW) states. A firm has a high (low) level of %Bound if it belongs to the top (bottom) 50% of the sample by %Bound.

Table A2: Summary statistics for the CEO-employee pay ratio and its components

The table displays summary statistics for the CEO-employee pay ratio and its components, total CEO pay and average employee pay (in thousands), for the main sample. Panels A and B show the distributions obtained after applying the imputation procedure by Donangelo (2016) and after defining the variables based on the disclosed labor expense *xlr*, respectively. The variables are defined in Table A1.

Panel A: After the imputation procedure following Donangelo (2016)

	Mean	Median	Std	5th	95th	N
CEO total pay	1,967.611	595.608	4,258.957	49.051	8,443.611	5,845
Average employee pay	398.055	98.408	2,182.874	17.402	936.167	5,845
CEO-employee pay ratio	22.798	6.111	48.620	0.273	102.187	5,845

Panel B: No imputation, based on the original labor expense *xlr*

	Mean	Median	Std	5th	95th	N
CEO total pay	1,700.251	429.845	4,407.662	33.779	8,218.553	2,160
Average employee pay	153.382	55.113	763.376	18.972	408.166	2,160
CEO-employee pay ratio	21.883	7.918	46.562	0.410	90.021	2,160

Table A3: Semi-elasticity of employee pay with respect to minimum wages

The table displays the results of DDD regressions with the logarithm of labor expense per employee, selling, general, and administrative expense (SG&A) per employee, and operating expense per employee as dependent variables. Employment data comes from YTS. Labor expense, operating expense, and SG&A expense are measured by *xlr*, *xopr*, and *xsga* variables from Compustat, respectively. In columns (1) and (3), the sample is main and includes all firms that have positive total pay observations and non-missing lagged control variable observations. In column (2), all manufacturing firms are additionally excluded (since manufacturing firms do not disclose labor expense in the SG&A section of the income statement). All columns include firms with positive corresponding labor cost measure per employee and labor cost measure. Time horizon is $t \in [-2; 2]$. Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Δ MW is the percentage change in MW level and is defined in Table 1. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. The coefficient on the DDD interaction represents the semi-elasticity of employee pay with respect to minimum wages. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Smaller firms, bottom 2/3 sample by # employees

	Labor expense	SG&A expense	Operating expense
	(1)	(2)	(3)
	log(XLR per emp.)	log(SG&A per emp.)	log(XOPR per emp.)
AffectedX%Δ MWX%Bound	0.3181***	0.0511***	0.0564***
	(9.99)	(3.42)	(2.81)
AffectedX%Δ MW	0.0087	-0.0212	-0.0164
	(0.59)	(-0.81)	(-0.58)
%Δ MWX%Bound	0.0023	0.0039**	-0.0011
	(1.44)	(2.26)	(-0.38)
AffectedX%Bound	-17.6399***	-3.9081***	-4.2708***
	(-42.89)	(-3.16)	(-3.87)
%Δ MW	-0.0022***	0.0084	0.0046***
	(-3.48)	(1.43)	(2.78)
%Bound	-0.2216	-0.0501	0.4489**
	(-0.51)	(-0.28)	(2.41)
Constant	3.8228***	4.1913***	4.5985***
	(10.66)	(9.55)	(17.65)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.91	.94	.92
N	1,440	2,815	5,765

Panel B: Larger firms, top 1/3 sample by # employees

	Labor expense	SG&A expense	Operating expense
	(1)	(2)	(3)
	log(XLR per emp.)	log(SG&A per emp.)	log(XOPR per emp.)
AffectedX%Δ MWX%Bound	-0.0229	0.0087	-0.0059
	(-0.55)	(0.39)	(-0.38)
AffectedX%Δ MW	0.0110	-0.0026	-0.0014
	(0.68)	(-0.34)	(-0.27)
%Δ MWX%Bound	-0.0038	0.0093	0.0093
	(-0.84)	(0.53)	(0.88)
AffectedX%Bound	-3.0072***	-2.0863	-1.1295
	(-5.41)	(-1.45)	(-1.35)
%Δ MW	-0.0012	-0.0004	-0.0019
	(-0.17)	(-0.07)	(-0.50)
%Bound	0.5474**	1.8290	0.8314
	(2.43)	(1.31)	(1.16)
Constant	4.1833***	3.3620***	4.5058***
	(4.13)	(5.28)	(7.98)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.96	.96	.95
N	720	1,405	2,880

Panel C: All firms

	Labor expense	SG&A expense	Operating expense
	(1)	(2)	(3)
	log(XLR per emp.)	log(SG&A per emp.)	log(XOPR per emp.)
AffectedX%Δ MWX%Bound	0.0667	0.0397***	0.0299**
	(0.65)	(2.74)	(2.39)
AffectedX%Δ MW	0.0321***	-0.0214**	-0.0160
	(2.84)	(-2.26)	(-1.51)
%Δ MWX%Bound	0.0012	0.0057	0.0013
	(0.57)	(1.44)	(0.40)
AffectedX%Bound	-13.2556***	-2.8772***	-3.2957***
	(-29.29)	(-2.78)	(-3.44)
%Δ MW	0.0019	0.0055*	0.0031**
	(0.55)	(1.76)	(2.28)
%Bound	0.0644	0.3385	0.5282**
	(0.24)	(0.89)	(2.50)
Constant	4.0877***	4.0916***	4.5753***
	(8.03)	(9.64)	(17.99)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.93	.94	.92
N	2,160	4,220	8,645

Table A4: Elasticity of the CEO-employee pay: Instrumental variable analysis

Columns (1) and (2) display results of DD regressions with the logarithm of operating expense per employee and employment as dependent variables and show results for the placebo test of Stage 1 of the I.V. analysis. They compare outcomes for firms post-MW law becoming effective, excluding all affected firms and additionally including firms with positive operating expense per employee and operating expense. Column (3) displays the results of OLS regression with the logarithm of CEO total pay as the dependent variable and employee wage instrumented by operating expense per employee as the independent variable and illustrates Stage 2 of the I.V. analysis. Stage 1 of the I.V. analysis is presented in Table 3, column (5). Operating expense is measured by *xopr* variable from Compustat, and employment data comes from YTS. The sample is main and includes all firms that have positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. IV log(XOPR per emp.) is instrumented *xopr* per employee, and its coefficient represents the elasticity of CEO pay with respect to employee pay. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by industry. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Smaller firms, bottom 2/3 sample by # employees

	Placebo test: only unaffected firms		Stage 2
	(1) log(XOPR per emp.)	(2) log(emp.)	(3) log(total pay)
IV log(XOPR per emp.)			0.077
			(0.56)
PostX%Bound	-0.007	-0.038	
	(-0.18)	(-1.18)	
Post	0.068***	-0.040	
	(2.84)	(-1.65)	
%Bound	0.501***	-0.484**	
	(2.70)	(-2.62)	
Constant	4.388***	4.370***	4.234***
	(18.82)	(20.42)	(5.03)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.92	.95	.45
N	5,470	5,470	5,765

Panel B: Larger firms, top 1/3 sample by # employees

	Placebo test: only unaffected firms		Stage 2
	(1)	(2)	(3)
	log(XOPR per emp.)	log(emp.)	log(total pay)
IV log(XOPR per emp.)			-0.242
			(-0.47)
PostX%Bound	0.073	-0.098	
	(0.61)	(-0.89)	
Post	-0.014	0.039	
	(-0.31)	(0.97)	
%Bound	0.728	-0.727	
	(1.03)	(-1.00)	
Constant	4.851***	5.349***	7.838***
	(9.41)	(12.46)	(3.26)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.93	.93	.4
N	2,730	2,730	2,880

Panel C: All firms

	Placebo test: only unaffected firms		Stage 2
	(1)	(2)	(3)
	log(XOPR per emp.)	log(emp.)	log(total pay)
IV log(XOPR per emp.)			-0.093
			(-0.49)
PostX%Bound	0.016	-0.059*	
	(0.37)	(-1.76)	
Post	0.047**	-0.018	
	(2.59)	(-1.06)	
%Bound	0.528**	-0.509**	
	(2.49)	(-2.45)	
Constant	4.430***	4.859***	5.326***
	(18.41)	(22.24)	(5.21)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.92	.97	.52
N	8,200	8,200	8,645

Table A5: Matched sample identification: CEO pay and firm characteristics at $t = -1$

This table presents the results of clustered t-test of difference in means for unbound vs. bound firms at the pre-treatment period $t = -1$, with standard errors clustered at the firm level. The table shows the outcomes for the matched sample of firms. The matched sample is constructed from the main sample firms with covariate-vector nearest neighbor matching technique with replacement using the Mahalanobis weighting metric (Abadie and Imbens (2002)), with the bias-correcting estimator of Abadie et al. (2004) at $t = -1$. A firm is bound if it has more than 15.34% (sample median at $t = -1$) employees in bound states. Every bound firm is matched to one unbound firm, with affected firms being matched to affected firms and unaffected firms being matched to unaffected firms. Matching covariates include firm size, profitability, ROA, Q, and M/B. An exact match is required on industry (presented by NAICS2 codes).

	Unbound	Bound	Diff	t-stat	p-value
Total pay	2241.636	2472.384	-230.748	-.617	.537
Cash pay	856.434	973.931	-117.497	-.687	.492
Salary	470.074	471.003	-.929	-.031	.976
Bonus	330.427	429.276	-98.849	-.69	.491
Incentive pay	2237.95	2823.854	-585.904	-1.116	.265
Size	7.194	7.435	-.24	-1.66	.097
Profitability	.113	.127	-.014	-1.799	.072
ROA	.055	.061	-.007	-1.09	.276
Q	1.772	1.81	-.038	-.513	.608
M/B	1.77	1.807	-.038	-.502	.616
log(annual return)	.116	.117	-.001	-.057	.955
%Bound	4.98	54.648	-49.668	-28.618	0
Number of distinct firms	376	847	.	.	.

Table A6: The effects of the minimum wage law on CEO pay: Matched sample

The table displays the results of DDD regressions with the logarithm of total pay, 1+cash pay (i.e., 1+salary+bonus), 1+salary, 1+bonus, 1+incentive pay (i.e., 1+total pay-salary-bonus) as dependent variables. One is added to the CEO total pay component values to account for meaningful zero values. The sample is matched, includes periods $t \in [-2; 2]$, and is constructed from the main sample firms with covariate-vector nearest neighbor matching technique with replacement using the Mahalanobis weighting metric (Abadie and Imbens (2002)), with the bias-correcting estimator of Abadie et al. (2004) at $t = -1$ (for more description, see Section 6.1 and Table A5). The main sample includes all firms with positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year and firm fixed effects are included where indicated. Standard errors are clustered by firm. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Smaller firms, bottom 2/3 sample by # employees

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	1.131***	0.400	0.843***	-2.798**	4.971***
	(3.49)	(0.95)	(2.80)	(-2.56)	(3.55)
PostXAffected	-1.551***	-0.038	-0.566*	1.186	-1.683
	(-3.45)	(-0.17)	(-1.85)	(1.63)	(-1.14)
PostX%Bound	-0.055	-0.004	-0.066	0.006	-0.135
	(-0.61)	(-0.04)	(-0.96)	(0.02)	(-0.64)
AffectedX%Bound	1.214	-1.518***	-0.377	7.024***	21.967***
	(1.38)	(-2.64)	(-0.70)	(2.59)	(4.80)
Post	0.219	-0.265*	-0.024	0.208	-0.217
	(1.07)	(-1.85)	(-0.17)	(0.54)	(-0.58)
%Bound	-0.270	0.330	0.200	-0.443	-0.239
	(-0.76)	(1.49)	(0.95)	(-0.55)	(-0.31)
Constant	5.183***	6.129***	4.777***	7.385***	2.977*
	(9.00)	(12.10)	(7.55)	(4.68)	(1.71)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.57	.6	.51	.56	.7
N	5,650	3,408	3,936	3,796	3,408

Panel B: Larger firms, top 1/3 sample by # employees

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.123	0.563	0.477	-0.334	0.127
	(0.17)	(0.44)	(0.60)	(-0.10)	(0.05)
PostXAffected	0.332	-0.675	-0.728	-0.757	4.347**
	(0.50)	(-0.95)	(-1.62)	(-0.31)	(2.18)
PostX%Bound	-0.224	-0.174	-0.069	0.250	-0.702
	(-0.83)	(-0.80)	(-0.44)	(0.36)	(-1.37)
AffectedX%Bound	1.574	-2.118*	-2.392***	1.238	-9.682***
	(0.68)	(-1.77)	(-3.22)	(0.43)	(-3.30)
Post	-0.012	0.186	0.323**	-0.871	0.756
	(-0.05)	(0.87)	(2.23)	(-0.96)	(1.19)
%Bound	-0.429	-0.832*	-0.421	-3.769***	-1.516
	(-0.60)	(-1.80)	(-1.18)	(-2.66)	(-1.19)
Constant	5.820***	7.428***	5.405***	9.620**	2.627
	(3.37)	(4.75)	(5.13)	(2.25)	(0.67)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.5	.5	.45	.57	.63
N	2,820	1,922	2,149	2,012	1,922

Panel C: All firms

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.875**	0.684	0.854**	-1.014	2.114
	(2.28)	(1.36)	(2.56)	(-0.72)	(1.50)
PostXAffected	-0.976**	-0.091	-0.588*	0.120	0.838
	(-2.13)	(-0.28)	(-1.94)	(0.12)	(0.68)
PostX%Bound	-0.084	-0.036	-0.089	0.133	-0.181
	(-0.97)	(-0.45)	(-1.44)	(0.54)	(-0.93)
AffectedX%Bound	1.224*	-1.253	-0.757	3.488*	2.636
	(1.66)	(-1.52)	(-1.09)	(1.75)	(0.53)
Post	0.119	-0.136	0.094	-0.128	-0.120
	(0.76)	(-1.12)	(0.85)	(-0.30)	(-0.42)
%Bound	-0.220	0.172	0.126	-0.643	-0.235
	(-0.68)	(0.85)	(0.69)	(-0.96)	(-0.37)
Constant	5.360***	6.115***	4.851***	6.464***	3.356**
	(9.40)	(11.88)	(7.83)	(4.11)	(2.03)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.59	.59	.54	.54	.68
N	8,470	5,330	6,085	5,808	5,330

Table A7: The effects of the minimum wage law on CEO pay: Border counties sample

The table displays the results of DDD regressions with the logarithm of total pay, 1+cash pay (i.e., 1+salary+bonus), 1+salary, 1+bonus, 1+incentive pay (i.e., 1+total pay-salary-bonus) as dependent variables. One is added to the CEO total pay component values to account for meaningful zero values. In Panel A, the effects on cash and incentive pay components are not presented due to missing original data on these pay components in CEO disclosed pay (see Section A.1). The sample contains bound and unbound firms headquartered in counties located along the borders of contiguous states and is constructed from the main sample as described in Section 6.2. The main sample includes all firms with positive total pay observations and non-missing lagged control variable observations. Time horizon is $t \in [-2; 2]$. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade), and drops out of estimation results since it is collinear with firm fixed effects. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. A firm is larger if it belongs to the top tercile of the sample by the number of employees at $t = -1$. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry-year, firm, and state border fixed effects are included where indicated. Standard errors are clustered by firm. T-statistics is presented in parentheses, and ***, **, * correspond to significance at 1%, 5%, and 10%, respectively.

Panel A: Smaller firms, bottom 2/3 sample by # employees

	Total pay
	(1) log(total pay)
PostXAffectedX%Bound	0.764* (1.80)
PostXAffected	-0.275 (-0.60)
PostX%Bound	-0.114 (-0.74)
AffectedX%Bound	6.507*** (4.66)
Post	0.323 (1.26)
%Bound	-0.112 (-0.22)
Constant	4.508*** (5.95)
Controls	Yes
Industry-Year FE	Yes
Firm FE	Yes
State border FE	Yes
Adj-R-squared	.54
N	2,755

Panel B: Larger firms, top 1/3 sample by # employees

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	2.059	0.564	-0.343	4.916	-8.621***
	(1.47)	(0.45)	(-0.41)	(0.82)	(-3.53)
PostXAffected	2.666**	0.359	0.370	-5.073	8.097***
	(2.02)	(0.37)	(0.66)	(-1.06)	(3.04)
PostX%Bound	-0.155	-0.375	0.025	-0.461	-0.749
	(-0.32)	(-1.38)	(0.13)	(-0.42)	(-1.51)
AffectedX%Bound	1.039	-8.556***	-5.385***	-7.991	15.710
	(0.28)	(-4.92)	(-3.62)	(-0.93)	(1.37)
Post	-0.719**	0.048	-0.057	0.888	-0.355
	(-2.33)	(0.11)	(-0.13)	(0.99)	(-0.44)
%Bound	1.115	-0.762	-0.497	-3.394	0.253
	(1.18)	(-1.18)	(-0.88)	(-1.53)	(0.18)
Constant	4.524	8.842***	7.220***	-1.066	8.392*
	(1.53)	(3.76)	(5.19)	(-0.18)	(1.90)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
State border FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.44	.58	.56	.54	.67
N	1,375	932	1,054	997	932

Panel C: All firms

	Total pay	Cash pay			Incentive pay
	(1) log(total pay)	(2) log(1+cash pay)	(3) log(1+salary)	(4) log(1+bonus)	(5) log(1+incentive pay)
PostXAffectedX%Bound	0.867	0.651	0.257	-0.540	-0.750
	(1.20)	(0.95)	(0.67)	(-0.31)	(-0.21)
PostXAffected	-0.029	0.068	0.124	-0.778	2.203
	(-0.04)	(0.14)	(0.43)	(-0.32)	(1.02)
PostX%Bound	0.024	-0.160	-0.040	-0.787*	-0.463
	(0.16)	(-1.05)	(-0.30)	(-1.85)	(-1.35)
AffectedX%Bound	-0.801	-2.312	-1.688	0.814	-5.668
	(-0.24)	(-1.38)	(-1.50)	(0.22)	(-0.74)
Post	0.057	0.151	0.120	1.043**	0.459
	(0.28)	(0.82)	(0.75)	(2.34)	(1.02)
%Bound	0.179	0.250	0.005	-0.418	2.199*
	(0.37)	(0.76)	(0.02)	(-0.40)	(1.74)
Constant	4.086***	5.719***	5.137***	3.813	1.060
	(4.89)	(7.74)	(7.25)	(1.62)	(0.59)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
State border FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.59	.61	.55	.48	.69
N	4,130	2,599	2,967	2,888	2,599