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

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Abstract

Using an about 40% U.S. federal minimum wage hike as a natural experiment, I establish an about 2.6% spillover effect of worker wages on CEO pay in smaller and medium U.S. public firms by employment size. I exploit a triple-differences methodology based on the distribution of workers across states. After the hike, a 10% increase in employment share in states bound by federal minimum wage leads to an about 7.7% increase in CEO total pay for firms in minimum-wage-sensitive industries relative to other industries. The results are consistent with CEOs demanding a compensation raise following an exogenous employee pay increase due to fairness concerns and inconsistent with the efficiency wages mechanism or CEOs extracting rents due to strong bargaining power. The results are robust to controlling for firm profitability, observable firm characteristics (matched sample), and local economic conditions (sample of firms headquartered in counties along contiguous state borders).

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

JEL classification: G34, J31, J38, D63

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1. Introduction

Attitudes to income inequality differ drastically across regions: survey data suggests that European society is less tolerant of having high income inequality than American society. Figure 1 illustrates this well-established stylized fact: for the past two decades, the inequality aversion index is consistently higher for many European countries compared to the U.S.[1] Academic studies (e.g., Kiatpongsan and Norton (2014))[2] document that this fact also applies for the corporate world, where the income inequality is commonly measured by the within-firm CEO-worker pay gap. Consistent with these findings, Dittmann, Schneider, and Zhu (2020) establish a link between European rank-and-file worker and CEO compensation levels: they document that in Germany, workers demand a pay increase when the information about CEO pay is disclosed to them due to relative wealth concerns.

In this paper, I investigate whether the link between CEO and worker pay exists in the U.S., where inequality aversion is lower and, hence, public opinions do not constrain relative CEO pay as much. Understanding the mechanism behind this link is important because the CEO-worker pay gap affects corporate policies and firm value (Pan, Pikulina, Siegel, and Wang (2022)). 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 high pay gap (illustrated in Figure 2). However, despite no consensus among academics, regulators and the general public tend to believe that CEOs are significantly overpaid compared to rank-and-file workers. Politicians refer to the high CEO-worker pay gap as "a national disgrace"[3] and attempt to address it by curbing CEO pay through tax policies[4] and promoting binding say-on-pay votes. General public reactions to the issue include worker strikes (e.g., Leonhardt (2023)) and lobbying for minimum wage (MW, hereafter) hikes (e.g., Tonti (2022)).

While there is mixed evidence on whether strikes are associated with wage increases (Card (1990)), it is well-established that an increase in MW level is a strong instrument for raising wages for rank-and-file workers who are paid at or below MW (e.g., Lee (1999), Freeman (1996)) without any direct effect on CEO pay. Moreover, a MW hike does not depend on the performance of a particular firm, which alleviates the endogeneity concern about an omitted factor driving all within-firm compensation levels. Thus, a MW shock provides a convenient opportunity to estimate the causality between employee and CEO pay. To take advantage of this setting, I use the introduction of a U.S. federal MW law signed on May 25, 2007, as a natural experiment and provide novel evidence on

[1]Related, Osberg and Smeeding (2006) make the same point using actual and "ideal" Gini coefficients as a metric of inequality aversion.

[2]They show that in European countries, the actual CEO-worker pay gap is closer to the publicly perceived "ideal" gap than in the U.S.

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

[4]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/.

the positive spillover effects of employee pay on CEO pay. 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.

To establish causality between worker wages and CEO compensation, 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 wages of 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).[5] Hence, the third difference compares changes in CEO pay across affected versus unaffected firms. To summarize, the DDD strategy estimates changes in CEO compensation 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. Consistent with the existing literature, I show that the MW law indeed raised average worker wages, but only in smaller and medium-sized firms (referred to as "smaller" firms). These firms comprise the first (intuitively, small firms) and second (intuitively, medium firms) terciles of the U.S. public firms sample by the number of employees. In particular, a median smaller firm employs 537 workers.[6] I also show that for smaller firms, the parallel pre-trends assumption holds, which is crucial for the DDD validity (Olden and Møen (2022)). Moreover, intuitively, the billion-dollar wages of CEOs of large corporations should not be affected by low-skilled worker wage changes driven by the MW shock. For example, it is hard to imagine that Elon Musk would care about a two-dollar MW raise given to the janitors at Tesla Inc. Hence, I expect the link between worker and CEO pay, if it exists, to be pronounced within smaller firms.

Analyzing changes in smaller firm 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 7.7% increase in CEO total pay for affected vs. unaffected firms post-MW law. This total pay increase is driven by both salary and incentive pay raises. This result is consistent with the common permanent nature of salary (Murphy (2013)) and MW shocks, and with incentive pay raises being important for CEOs since they boost CEO reputation (Edmans, Gosling, and Jenter (2023)).[7]

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

[6] To compare, the Small Business Administration (SBA) for the U.S. and the World Bank for the OECD countries define small and medium enterprises (SMEs) as businesses with no more than 500 employees. Sources: https://advocacy.sba.gov/wp-content/uploads/2023/03/Frequently-Asked-Questions-About-Small-Business-March-2023-508c.pdf and https://documents1.worldbank.org/curated/en/819161468766822276/pdf/multi0page.pdf.

[7] Since incentive pay awards have to be approved by the board and presented to shareholders, increases in incentive

A key advantage of my analysis is that on top of establishing the positive spillover effect from worker wages to CEO pay, I evaluate its economic magnitude. This is important from the policy perspective since quantifying the semi-elasticity of the CEO pay with respect to MW is crucial for successfully targeting income inequality. I show that the CEO pay-MW semi-elasticity is equal to 0.064, translating into the 41% hike in federal MW leading to a 2.61% relative increase in pay for a median smaller firm CEO. In comparison, the corresponding relative increase in pay for a median smaller firm employee is over seven times larger and is equal to 12.81%.

However, to effectively adjust the economic policy using the MW hikes, it is also important to understand the underlying mechanism for the link between smaller firm employee and CEO pay. I consider several competing mechanisms for the CEO pay increase I find. The first mechanism is based on the efficiency wage hypothesis (Yellen (1984), Akerlof and Yellen (1986)). 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 firm performance boost after the MW law using standard performance measures. To enhance the power of my test, I develop an alternative triple-differences identification strategy based on the variation in firm-level CEO compensation setting dates in 2007 relative to the MW law announcement date (May 25). I show that smaller firms disclosing CEO pay after May 25 award significantly larger compensation levels during 2007 compared to smaller firms disclosing CEO pay before that date. This result is inconsistent with the efficiency wages hypothesis predictions since firms are unlikely to be able to adjust productivity during this short time period. Taken together, my findings do not support the efficiency wages mechanism driving the worker-CEO pay link.

An alternative mechanism is based on the bargaining hypothesis: one may argue that CEOs enjoy positive spillovers from worker wage raises due to their strong bargaining power (Murphy and Zabojnik (2004), Murphy and Zabojnik (2007), Frydman (2019)). 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 smaller firm CEOs increases more than that of high-rent smaller firm CEOs, and for both groups, pay increases are insignificant. This evidence is inconsistent with the CEO bargaining power story.

Finally, the third mechanism I consider is based on the fairness hypothesis, which suggests that worker wage level serves as a reference point for CEOs evaluating whether their compensation is awarded in a "fair" way (Edmans et al. (2023), Chaigneau, Edmans, and Gottlieb (2022)). The fairness story implies that after the MW hike drives this reference point upwards, CEOs demand a compensation raise out of fairness concerns. If this is the case, younger CEOs should receive a larger increase in pay than older CEOs following the MW shock because they should care more about being compensated fairly.[8] Indeed, younger CEOs face longer career horizons, and for them, current

pay improve CEO reputation in the executive labor market (Edmans et al. (2023)).

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

pay affects the present value of future pay more by signaling their ability in the executive labor market (Boschen and Smith (1995)) and improving outside options (Gibbons and Murphy (1992)) or affecting intertemporal risk-sharing (Edmans, Gabaix, Sadzik, and Sannikov (2012)). Consistent with this logic, I find that younger smaller firm CEOs experience an about 2.5 times larger increase in total pay after the MW law than older CEOs.

Moreover, my results survive a set of robustness checks, including 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 analyses alleviate the concerns that my results may suffer from the endogeneity resulting from firm observable characteristics or different economic conditions across regions driving the results.

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 workers compared to the high-wage workers (Lee (1999), Freeman (1996), Neumark, Schweitzer, and Wascher (2004), Gopalan, Hamilton, Kalda, and Sovich (2021)). DiNardo, Fortin, and Lemieux (1995) document heterogeneous effects based on gender, while others consider different country settings (e.g., Rinz and Voorheis (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 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. Other work connecting CEO compensation and worker wages largely focuses on the effects of the pay ratios on firm value and operating performance (Faleye, Reis, and Venkateswaran (2013), Mueller, Ouimet, and Simintzi (2017), Rouen (2020)) and documents correlations. One notable exception is De Vito and Gómez (2022), who document a causal effect on firm performance using a quasi-exogenous shock to within-firm pay inequality through labor regulation reform in Italy.

Finally, my results are consistent with the fairness hypothesis of executive compensation of Edmans et al. (2023) and Chaigneau et al. (2022). Given that, I hope that 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.[9]

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.

[9]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, Gabaix, and Jenter (2017) provide excellent overviews of the executive compensation literature.

2. Background

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.[10] 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.[11] Related, Harasztosi 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.[12] 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 3 presents the distribution of states classified as bound or unbound pre-introduction of the law.[13]

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

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

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

[13] Note that the effective MW changes in bound states happen only as a result of the law, while in unbound states, these changes may be driven by other policies (e.g., indexation to inflation). However, around the federal MW laws, the effective MW increases are much larger in magnitude in bound than unbound states. Therefore, the interpretation of this identification strategy is that any observed effects on the outcome variables come from the additional change in MW in bound versus unbound states (see Gustafson and Kotter (2022) for a more detailed discussion).

As of November 2023, federal MW constitutes $7.25 per hour, and the Fair Minimum Wage Act of 2007 remains 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.[14] 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 industries are most affected by the minimum wage law?

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 4 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 4a). This finding is consistent with Dai and Qiu (2022), Chava et al. (2019), and similar to Gustafson and Kotter (2022).[15] Hence, I define "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.[16] Hence, tipped workers are classified as below federal MW workers, despite being subject to lower cash MW level. Figure 4b shows that even excluding tipped workers (reflected in Figure 4c), Leisure and Hospitality and Retail Trade industries remain the most reliant on MW labor.

2.3. Does firm size matter for the minimum wage law effects?

MW hikes also have a differential impact on firms depending on their size. Intuitively, CEOs of large corporations are too wealthy to be affected in terms of pay by a two-dollar change in worker salaries coming from a MW shock. Often, these CEOs are completely ignorant of how much workers of their firms are paid. An illustrative example is Elon Musk's confession of being unaware of

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

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

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

Tesla workers being paid under-MW salary of $5 per hour in 2016 (Livin (2016)), which resulted in a lawsuit and eventually a $550,000 settlement (Slowey (2016)). In contrast, owners of smaller businesses are much more likely to be aware of MW changes and their effects on the firm. Some of the smaller firm CEOs are engaged in the MW policy considerations to the point of giving interviews to CNN (Elassar (2021)).

Consistent with this argument, it is well-documented that MW hikes significantly impact smaller firm worker wages. Wursten and Reich (2023) document a causal effect between U.S. minimum wage hikes and wages in smaller firms but not in larger firms. Del Carpio, Nguyen, and Wang (2012) show that Indonesian small firms are more responsive to minimum wage changes, and for them, average wage is more correlated with minimum wage changes. This evidence supports the crucial for identification assumption that in smaller firms, the MW shock is passed on to the average firm wages. In Section 5.1 I confirm that, indeed, the MW law actually raised employee wages in smaller firms and, hence, will focus on smaller firms from Section 5.1 onwards.

3. Data and sample construction

3.1. Data sources

This study combines multiple data sources. The primary CEO compensation source is the Capital IQ People Intelligence database due to its extensive coverage of over 2.4 million individuals worldwide of public and private companies including executives, board members, and investment professionals. Supplementary compensation datasets include MSCI (formerly GMI Ratings), Execucomp, Institutional Shareholder Services Incentive Lab (ISS, hereafter), and BoardEx. Establishment-level employment and state location come from the Your-economy Time Series (YTS, hereafter) dataset. The YTS is a yearly establishment-level database covering location, employment, and sales for public and private firms. S&P Compustat North America database (annual data file) is used to construct yearly firm-level control variables and access historical firm headquarters locations and industry codes. Stock returns come from the Center for Research on Security Prices database (CRSP, hereafter). Proxy statement filing dates are retrieved using the SEC Analytics Suite by WRDS.

3.2. Sample construction

I start with the sample of all publicly traded corporations headquartered in the U.S. with non-missing total assets. I define an event window for the MW increase as $t \in [-2; 2]$, keeping it wide enough to capture the effects of the MW law and narrow enough to exclude other potential confounding events. Importantly, the event window focuses on fiscal years rather than calendar years, following Gustafson and Kotter (2022).[17] For example, period t includes all CEO compensation filings from July 24, 2007, which is the effective date of the MW law, until July 24, 2008. I do this to ensure that any firm that wishes to update CEO compensation after the MW shock is considered as such.

I further merge the Compustat sample to the CEO 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.[18] I start matching with Capital IQ by GVKEY and continue the matching procedure for unmatched observations with MSCI by Ticker, Execucomp by GVKEY, ISS by CIK, and BoardEx by GVKEY using the BoardEx-CRSP-Compustat link table by WRDS.[19] This matching order is defined by the relative firm coverage presented in Figure A.1a. I require all observations to have positive CEO total pay.[20] The distribution of the matched number of firms per period is given in Figure A.1b.

Next, following Flynn and Ghent (2021), I match the Compustat-CEO pay sample to YTS firm×headquarters data 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. The YTS firm×headquarters data is constructed from the raw YTS data by aggregating establishment-level employment in bound and unbound states for each firm×headquarters (as per Figure 3).

I further clean the sample as follows. I match to control variables from Compustat and CRSP and winsorize all the current and lagged control variables at the 0.5% and 99.5% levels. Control variables follow the executive compensation literature and include lagged firm size, ROA, profitability, Tobin's Q, market-to-book, and log(1+annualized return). To further improve estimation precision, I keep only firms with non-missing observations for each period of the event window. I define industries at the NAICS3 level at $t = -2$ to ensure that firms do not change industries throughout the event window. Figure A.1c presents the number of matched firms per period to YTS, CRSP, and post-additional filters.

The resulting "main" sample consists of 1,690 firms per period with available CEO pay data, employment levels, and non-missing control variables[21] for the five-period time horizon of $t \in [-2; 2]$, covering dates from July 24, 2005, to July 24, 2010, as shown in Figure A.1d. In a robustness

[17] 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.

[18] Confirmed in 94.74% of cases where CEO is identified. 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 80.43% cases.

[19] 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.

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

[21] Table A.1 presents the definitions of the variables.

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 7).

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 28.5% of employees located in bound states. During the sample period, a median CEO has received $604.6K in total pay, with cash pay and incentive pay accounting for 78% and 22% of compensation, respectively. The median firm in the sample has $1.10B in total assets, an ROA of 0.029, and a market-to-book ratio of 1.30.

However, as discussed in Section 2.3, it is reasonable to expect ex-ante that the relationship between worker pay and CEO pay is pronounced most strongly in smaller firms vs. larger firms. To test this conjecture, I present the results for the analysis of the CEO-worker pay link based on three subsamples: smaller firms, larger firms, and all firms. To define smaller and larger firms, I follow a naive and intuitive approach and start by splitting the main sample into terciles by the number of employees. Each tercile corresponds to the "small", "medium", and "large" firm group. I further aggregate the bottom two terciles into a group of "smaller" firms, which is the sample of focus since the largest firms are unlikely to be affected.[22] 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 537 employees - to compare, the U.S. SBA and the World Bank for the OECD countries define SMEs as firms with at most 500 employees. For a smaller firm in my sample, 5.1% of workers are located in bound states. In contrast, a median larger firm has 8,615 employees, with 27.6% of them working in bound states. A median smaller firm has $606MM in total assets, an ROA of 0.016, a market-to-book ratio of 1.25, and pays the CEO $438.4K per year. In contrast, the total assets of a median larger firm constitute $4.7B, 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. Methodology and Hypotheses

4.1. 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 wage raise following the MW law becoming effective. Therefore, the first difference represents a continuous assignment

[22] Some may argue that an alternative approach is to consider only the lowest tercile as "smaller" firms. However, if anything, including medium-sized firms will bias me against finding the results while increasing the smaller subsample size and, hence, the power of the tests.

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 based on the share of MW workers in these industries (see Section 2.2 for a discussion). Equation 1 presents the DDD model:

$$\begin{aligned}\log(\text{CEO pay}_{i,j,t}) = {} & \beta_0(\text{Post}_t \times \text{Affected}_j \times \%\text{Bound}_i) \\ & + \beta_1(\text{Post}_t \times \text{Affected}_j) + \beta_2(\text{Post}_t \times \%\text{Bound}_i) + \beta_3(\text{Affected}_j \times \%\text{Bound}_i) \\ & + \beta_4\text{Post}_t + \beta_5\text{Affected}_j + \beta_6\%\text{Bound}_i + \Gamma\text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \epsilon_{i,j,t},\end{aligned} \quad (1)$$

where CEO pay is a CEO pay component[23] (total pay, 1 + salary, 1 + bonus, 1 + cash pay, or 1 + incentive pay)[24] in firm i of industry j at period t; Post is a dummy variable equal to 1 for periods $t = 0, 1, 2$, Affected is a dummy variable equal to 1 if a firm belongs to a NAICS2 71, 72, 44, or 45 industry,[25] %Bound is the share of firm workers employed at establishments located in bound states, X is a vector of firm characteristics, δ are industry by year fixed effects (Gormley and Matsa (2014)), and γ are firm fixed effects.

Importantly, the DDD strategy is valid if the parallel trends in ratios assumption (Olden and Møen (2022)) for CEO total pay with respect to the MW shock effect is satisfied. To test this assumption, I split the subsamples of smaller affected and unaffected firms into high-%Bound and low-%Bound firms based on the median %Bound values. I further create ratios of median CEO total pay of firms with high (high-%Bound) to low (low-%Bound) share of workers located in bound states. Figure A.2 illustrates that during the pre-treatment period ($t = -2$ and $t = -1$), this ratio for smaller affected firms largely co-moves with this ratio for smaller unaffected firms. However, the two ratios abruptly diverge when the MW law becomes effective ($t = 0$ onwards). This evidence supports the causal interpretation of my results (Olden and Møen (2022)).

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 effect is represented by 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. In the next section, I develop the hypotheses connecting employee and CEO pay and discuss how the estimate of β_0 allows to make conclusions about which hypothesis my results are most consistent with.

[23]Typically, incentive pay consists of stock and option awards, long-term incentive plan, and other compensation (as per Execucomp definition, which is the dataset commonly used in executive compensation literature). 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. Section A.1 discusses pay variables construction and definitions in detail.

[24]One is added to the CEO total pay component values to account for meaningful zero values.

[25]See Section 2.2 for a detailed discussion.

4.2. Hypotheses and mechanisms

Intuitively, CEO pay could be positively related, negatively related, or unrelated to employee pay. In this section, I cover each of these three scenarios and outline mechanisms that could be driving them. I further discuss empirical settings that allow to test each of these mechanisms.

To begin with, a positive relationship between worker pay and CEO pay is summarized in Hypothesis 1. If Hypothesis 1 is true, it will be reflected in a positive estimate of β_0. This result is consistent with three potential mechanisms.

Hypothesis 1 *When employees receive an exogenous wage raise, CEOs receive a pay raise as well.*

The first mechanism is driven by the efficiency wage hypothesis (Yellen (1984), Akerlof and Yellen (1986)). It predicts that an increase in wages will incentivize employees to work more efficiently. As a result, improved productivity will translate into better firm performance, allowing the CEOs to enjoy higher compensation. Therefore, if the efficiency wages story is true, I expect to see that firm performance goes up following the MW shock. Moreover, since standard firm performance measures can be noisy (Hermalin and Weisbach (2012)), I also test the efficiency wages story using an alternative identification strategy based on the variation in firm-level CEO compensation setting dates relative to the MW law announcement date during *only* the year 2007 (discussed in Section 6.1.2). Plausibly, wages do not affect productivity in such a short time frame of several months (Oi and Idson (1999)). Hence, if the efficiency wages story is true, I expect to find no effect on CEO pay in this alternative setting.

The second mechanism is based on the bargaining hypothesis (Murphy and Zabojnik (2004), Murphy and Zabojnik (2007), Frydman (2019)). It predicts that strong bargaining power resulting from increased competition in the CEO labor markets drives CEO ability to extract more rents from the firm. In turn, this rent extraction is reflected in larger CEO compensation. Empirically, firm rents are commonly proxied by profitability measures (e.g., Kim (2020)). Therefore, if the bargaining story is true, I expect to find that CEOs of high-rent firms receive larger pay increases after the MW shock compared to CEOs of low-rent firms.

The third mechanism is based on the "fairness" hypothesis (Edmans et al. (2023), Chaigneau et al. (2022)). It predicts that CEOs access the "fairness" levels of their compensation with respect to several reference points, one of which is worker pay.[26] As a result, when workers receive an exogenous pay raise, CEOs demand a corresponding pay raise for themselves out of fairness concerns. A "fair" reward would signal a CEO's reputation in the executive labor market and motivate her to put in effort. Hence, if the fairness story is true, I expect to find that younger CEOs receive larger pay raises than older CEOs. On the one hand, current CEO pay signals their ability in the executive labor market (Boschen and Smith (1995)), improving outside options (Gibbons and

[26] Other reference points include peer company CEO pay, CEO value added to the firm, and CEO last year's compensation (Edmans et al. (2023)).

Murphy (1992)), which matter more for younger CEOs since they have more time until retirement. On the other hand, since younger CEOs have more time until retirement, their optimal contract is driven by intertemporal risk-sharing and follows the "deferred reward principle" (Edmans et al. (2012)). Despite being fundamentally different and thus challenging to differentiate from each other, both stories predict that for younger CEOs, current pay affects the present value of future pay more, and therefore, they should care more about being compensated fairly.

Alternatively, a negative relationship between worker pay and CEO pay is summarized in Hypothesis 2. A negative estimate of β_0 is consistent with Hypothesis 2.

Hypothesis 2 *When employees receive an exogenous wage raise, CEO pay drops.*

A mechanism consistent with Hypothesis 2 is based on the idea (referred to as the "profitability" hypothesis) that increased worker wages may negatively affect firm profitability. Indeed, MW hikes are introduced by the government and are plausibly exogenous to the firms. Consequently, if a firm does not anticipate a MW raise, the shock may represent an unplanned expenditure in terms of firm payroll. As a result, an extra wage expense will reduce firm profit and, hence, the share that could be awarded to the CEO. Consistent with this hypothesis, existing research has shown a drop in firm profitability and revenues following MW hikes (e.g., Agarwal, Ayyagari, and Kosová (2023), Draca, Machin, and Van Reenen (2011), Harasztosi and Lindner (2019)). Therefore, if the profitability hypothesis is true, I expect to see a drop in firm profitability after the MW shock.

Finally, Hypothesis 3 summarizes an absence of a relationship between worker pay and CEO pay. An estimate of β_0 equivalent to zero is consistent with Hypothesis 3.

Hypothesis 3 *When employees receive an exogenous wage raise, CEO pay does not change.*

A mechanism consistent with Hypothesis 3 is based on the labor market segmentation theory (Reich, Gordon, and Edwards (1973)). Since CEOs and rank-and-file workers belong to different employee groups (executives vs. non-executives), they plausibly operate in different labor markets. Therefore, if the labor market segmentation story is true, I expect to find that a MW shock raising worker pay will not affect CEO pay in a meaningful way.

In this paper, I test Hypotheses 1, 2, 3 by estimating the coefficient β_0 and observing its sign and magnitude. Given that each of the three hypotheses may be driven by at least one potential mechanism, I further attempt to find additional evidence in favor of the leading hypothesis by testing these mechanisms. As discussed above, each mechanism results in at least one more firm-level outcome being affected (e.g., firm performance for efficiency wage hypothesis, profitability for profitability hypothesis, etc.). Therefore, I try to differentiate between these mechanisms using this convenient empirical setting and figure out which forces are most consistent with my results.

5. Main result: How does CEO pay react to changes in worker pay?

5.1. Did the minimum wage law raise worker wages?

My study relies on a crucial identification assumption that the MW law is an exogenous shock to worker wages. The fact that MW hikes are transmitted to worker wages is well-established in the existing literature. Several studies document a raise in the lower tail of the worker wage distribution following a state or federal MW shock (e.g., Fortin et al. (2021), Rinz and Voorheis (2018), Autor et al. (2016), Neumark et al. (2004), Lee (1999), Freeman (1996), DiNardo et al. (1995), and others). Other studies (e.g., Bailey, DiNardo, and Stuart (2021)) establish a positive effect of MW hikes on average wages for firms in states with many at- or below-MW level workers. Quantitative estimates (Gopalan et al. (2021)) suggest state MW hikes leading to up to $2.50 wage raise for workers with a high share of MW labor.

To confirm that MW hikes affect wages in my sample and empirical setting, I estimate a variation of Equation 1 using average worker wage as a dependent variable. Since firm wages are not publicly available, I use three different proxies for average wages. The most precise proxy is based on the total labor expense (Compustat variable *xlr*) divided by the number of employees. However, total labor expenses are available only for about a quarter of the sample because firms are not legally obliged to disclose them. For robustness, I also use selling, general, and administrative expenses (SG&A, Compustat variable *xsga*) per employee and operating expenses (Compustat variable *xopr*) per employee as two other proxies of worker wages. This is because the affected firms, as a subset of non-manufacturing firms, disclose wages in SG&A expenses part of the income statement[27]; and all firms have to disclose wages in some part of the operating expenses section of the income statement.[28] A positive estimate of β_0 is consistent with the average worker wage increasing after the MW shock.

Table 3 presents the estimation results of Equation 1. Panel A shows that for smaller firms, the value of β_0 is estimated to range from 0.78 to 4.10, 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 7.8% to 41%. In contrast, the results are insignificant for larger firms (Panel B) and are weaker for all firms (Panel C). This is consistent with the expectation that MW hikes affect wages most in smaller firms (see Section 2.3 for a detailed discussion).

This evidence additionally confirms that the assumption that the MW hike is an exogenous shock to worker wages is satisfied for the smaller firms in my sample. The validity of this assumption allows me to draw causal conclusions from my analysis. Therefore, for the rest of the paper, I am going to focus on smaller firms to estimate the link between worker wages and CEO pay.

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

[28] Using SG&A per employee and operating expenses per employee allows to increase the sample size and, hence, statistical test power. However, it should be acknowledged that these two proxies are likely noisy and less precise than the total labor expense per employee because they include a variety of other expenses on top of worker wages.

5.2. Quantifying the effect of the minimum wage law on CEO pay of smaller firms

Having established that the MW law is a legitimate exogenous shock raising worker wages, I turn to the central question of interest of this paper: how does CEO pay change as a result of worker wage raise? To answer this question, I estimate Equation 1 and investigate the value and magnitude of the coefficient β_0.

Table 4, Panel A shows that for the sample of smaller firms, β_0 is positive, statistically significant, and equal to 0.77 (column 1). The economic magnitude of this estimate can be interpreted in the following way: a 10-percentage point increase in the share of employees located in bound states results in a 7.7% increase in CEO total pay for affected vs. unaffected firms, following a positive MW shock. Given the positive estimate of β_0, this evidence supports Hypothesis 1 and is inconsistent with Hypotheses 2 and 3.

Moreover, the total pay increase is transmitted through salary (column 3) and incentive pay (column 5) components of CEO compensation. The corresponding estimates of β_0 are also positive, statistically significant, and equal to 0.61 (column 3) and 5.31 (column 5). These estimates can be translated into economic magnitudes of 6.1% (53.1%) increase in salary (incentive pay) for affected vs. unaffected firms after the MW hike. The channel of transmission of CEO total pay raises through salary is particularly intuitive in my empirical setting since I exploit a MW shock as a source of identification. Both MW shocks and executive salary (Murphy (2013)) have the same persistent nature since they represent a fixed component of pay for different labor groups: MW shocks affect rank-and-file employees, while executive salary shocks affect executives. Therefore, if worker wage raises are spilled over to CEO pay raises, it makes sense to expect the "persistence" characteristic to be spilled over as well.

Additionally, the channel of transmission of CEO total pay raises through incentive pay is consistent with the prediction of the fairness hypothesis (Edmans et al. (2023)), which is one of the potential mechanisms consistent with my results.[29] According to the fairness hypothesis, CEOs particularly care about their incentive pay levels since they provide ex-post recognition incentives. This is because incentive pay raises boost CEO reputation as they are presented to shareholders and have to be approved by the board. This reputational effect outweighs the CEO extra happiness coming from the pure monetary award effect of a pay raise (Edmans et al. (2023)). Therefore, if a CEO observes a rise in worker wages, which updates the reference point for their own pay, they will request an increase in pay to be issued through incentive pay.

In contrast, Panel B of Table 3 shows that for the sample of larger firms, β_0 is statistically insignificant. This implies that, in larger firms, CEO pay does not react to changes in worker pay in a meaningful way. This evidence is consistent with MW shocks affecting wages mostly in smaller firms (as discussed in Sections 2.3 and 5.1). Moreover, Panel C of Table 3 further shows that the result of CEO pay increase is less pronounced for the sample of all firms: the value of β_0 estimate in

[29] See Section 4.2 for a detailed discussion of hypotheses and corresponding potential mechanisms supporting them.

column 1 is lower than in Panel A (0.588 vs. 0.766) and less statistically significant. This finding is intuitive because the sample of all firms includes both smaller firms (that drive the CEO pay increase result) and larger firms (which do not experience a significant change in CEO pay).

Overall, my findings act as evidence in favor of Hypothesis 1: following a MW shock which raises worker pay, CEOs receive a boost in their own pay. This result is pronounced for a subsample of smaller firms, which are plausibly most affected by the MW shock.

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

I next turn to quantifying the CEO total pay spillovers from the worker wages driven by the MW law. The empirical design of this study allows to assess how the magnitude of these spillovers changes with a change in the share of workers exposed to MW shocks through their location. However, it is also interesting to know how much a one-dollar increase in MW affects CEO total pay in smaller affected vs. unaffected firms. The answer is important not only from the perspective of maximizing shareholder value of the firm, but also from the perspective of policymakers targeting income and corporate inequality.

To measure the intensity of spillover effects of MW hikes to CEO pay, I exploit an important institutional detail underlying the introduction of the MW law. As discussed in Section 2, the law was enacted on May 25, 2007, and became effective on July 24, 2007. However, the implementation of the MW hikes was not uniform: the law comprised three consequent hike stages (see Table 1). Each hike represented an increase in MW equal to 13.6% (event #1), 12.0% (event #2), or 10.7% (event #3). This time-series variation provides a convenient opportunity to estimate the semi-elasticity of CEO total pay with respect to MW. In turn, this semi-elasticity can then be used to estimate the aggregate effect of the 41% federal MW hike on a median smaller firm CEO pay, which will be equal to a dollar amount of $\beta_0 \times 41\% \times$ median smaller firm CEO pay.

To do that, I estimate a triple-differences model (e.g., Gustafson and Kotter (2022), Chava et al. (2019)) given by Equation 2:

$$\begin{aligned}\log(\text{CEO pay}_{i,j,t}) = {} & \beta_0(\%\Delta\text{MW}_t \times \text{Affected}_j \times \%\text{Bound}_i) + \beta_1(\%\Delta\text{MW}_t \times \text{Affected}_j) \\ & + \beta_2(\%\Delta\text{MW}_t \times \%\text{Bound}_i) + \beta_3(\text{Affected}_j \times \%\text{Bound}_i) \\ & + \beta_4\%\Delta\text{MW}_t + \beta_5\text{Affected}_j + \beta_6\%\text{Bound}_i + \Gamma\text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \epsilon_{i,j,t},\end{aligned} \tag{2}$$

where CEO pay is a CEO pay component (total pay, 1 + salary, 1 + bonus, 1 + cash pay, or 1 + incentive pay) in firm i of industry j at period t; Affected is a dummy variable equal to 1 if a firm belongs to a NAICS2 71, 72, 44, or 45 industry, %Bound is the share of firm workers employed at establishments located in bound states, X is a vector of firm characteristics, δ are industry by year fixed effects (Gormley and Matsa (2014)), and γ are firm fixed effects.The key difference of Equation 2 with Equation 1 is the variable "%Δ MW". This is a continuous variable equal to the percentage increase in MW for the relevant time period. For example, %Δ MW is equal to 13.6 for $t = 0$.[30] The estimate of the coefficient β_0 represents the semi-elasticity of CEO pay to MW.

The results of the estimation of Equation 2 on the sample of smaller firms are presented in Table 5, Panel A. In column (1), which uses CEO total pay as a dependent variable, β_0 is equal to 0.064 and is statistically significant. The economic magnitude of this coefficient is that a 10-percentage point increase in MW for affected vs. unaffected firms with the same employment share in bound states leads to a 0.64% increase in CEO total pay. Consistent with the main results, the estimates of β_0 in columns (3) and (5) are also positive and statistically significant, representing that MW hikes affect CEO total pay through the channels of increased salary and incentive pay.

The estimated value of semi-elasticity of CEO total pay to MW changes, which is equal to 0.064, implies that a 40.78% total increase in federal MW triggered by the Fair Minimum Wage Act of 2007 results in a \$11,441 ($0.064 \times 40.78\% \times \$438,363$) increase in total pay for the CEO of a median smaller firm in my sample. This effect corresponds to a 2.61% (\$11,441 divided by \$438,363 times 100%) relative increase in smaller firm CEO total pay.

Since MW policy targets primarily rank-and-file workers and not CEOs, it is interesting to compare the economic magnitudes of CEO pay raises in response to the 41% MW hike to the corresponding worker pay raises. The ex-ante expectation is that worker pay raises are significantly larger than CEO pay spillovers. To find the semi-elasticity of worker wages to MW, I estimate a variation of Equation 2 on the sample of smaller firms, using the three proxies for worker wages (labor expense per employee, SG&A expense per employee, and operating expense per employee, as per the analysis of Section 5.1) as dependent variables.

Panel B of Table 5 shows that in smaller firms, the semi-elasticity of worker pay with respect to MW ranges from 0.037 to 0.314, depending on the worker wage proxies used. This evidence translates into a \$6,752 ($0.314 \times 40.78\% \times \$52,726$) absolute, or 12.81% (\$6,752 divided by \$52,726 times 100%) relative, increase in pay for a median smaller firm worker following the Fair Minimum Wage Act of 2007 becoming effective, if disclosed labor expense per employee is used as a proxy for wages (column (1)).[31] Indeed, the estimate of β_0 is much larger in column (1) of Panel B than in column (1) of Panel A, implying that worker wages react to a one-dollar MW hike much stronger than CEO pay does.[32] In the next section, I turn to the discussion of potential mechanisms that may be driving the established and quantified CEO pay raises in response to the worker wage hikes resulting from the MW hike.

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

[31] If the SG&A (column (2)) and the operating expense per employee (column(3)) proxy for wages instead, the corresponding economic magnitude of the median smaller firm employee pay increase is \$1,447 (1.5%) and \$5,623 (1.6%), respectively. However, these estimates are likely too small since both SG&A and operating expenses per employee are noisier proxies for worker wages, as mentioned in Section 5.1.

[32] This evidence, on top of serving its primary purpose of quantifying the semi-elasticity values, serves as a "sanity check" for my results.

6. Mechanism: Why does CEO pay increase after the MW law?

Knowing that CEOs of smaller firms observe positive spillovers to their pay coming from worker wage raises driven by the MW law, the next intuitive question to ask is: Why does this happen? In this section, I consider each of the three potential mechanisms - efficiency wages, bargaining, and fairness - that are consistent with my results.[33] In particular, I attempt to understand which of these mechanisms and to what extent are at work.

6.1. Efficiency wage hypothesis

As discussed in Section 4.2, I test the efficiency wage hypothesis in two ways presented in Sections 6.1.1 and 6.1.2.

6.1.1. Do affected firms perform relatively better after the minimum wage law?

In this section, I start with a naive and intuitive approach and examine how firm performance changes around the MW law in smaller firms. To do that, I estimate a variation of Equation 1 using two proxies for firm performance, namely industry-adjusted ROA (Hermalin and Weisbach (2012)) and the logarithm of 1 plus annualized return, as dependent variables.[34] A positive estimate of β_0 is consistent with the efficiency wages story since it implies that firm performance improves after the MW shock, consistent with higher wages boosting worker productivity.

Table 6, Panel A presents the estimation results. The estimate for β_0 is statistically insignificant regardless of the firm performance proxy used (i.e., in both columns (1) and (2)). Moreover, when industry-adjusted ROA is used as a proxy (column (1)), the value of β_0 is estimated to be near zero (0.056). These results illustrate that there is no meaningful difference in performance between smaller affected vs. unaffected firms after the MW hike, which is inconsistent with the efficiency wage mechanism predictions.

6.1.2. New identification: variation in firm CEO pay setting dates

To further enhance the power of the efficiency wage mechanism test, I adopt a second, more sophisticated approach. This approach relies on exploiting the variation in firm-level CEO compensation setting dates relative to the MW law announcement date.

I develop a new identification strategy motivated by the stylized fact that U.S. public firms typically set and negotiate CEO compensation levels one time per year (Bebchuk, Cremers, and Peyer (2011), Dow and Raposo (2005)). Therefore, during 2007, U.S. public firms in my sample could be divided into two groups: those that disclosed CEO pay before ("control", hereafter) and after ("treated", hereafter) May 25. The date of May 25 is crucial in the context of the MW law

[33]See Section 4.2 for a detailed discussion of the mechanisms.

[34]One is added to the annualized return to account for meaningful zero values.

because this is the date when the law was enacted and, hence, introduced to the public. Hence, assuming that the MW law enactment was not anticipated, firms that disclosed CEO pay before May 25 were at a relative disadvantage: they did not have a chance to renegotiate it and had to stick with it for the ongoing fiscal year. In contrast, firms that had not yet disclosed their 2007 CEO pay levels by May 25 were able to incorporate the new information about the MW hike into their executive compensation setting practices.

For the purposes of my study, it is interesting to see whether there is a difference in CEO compensation levels for control and treated firms during 2007. Given the main result established in Section 5, the expectation is that treated firms will adjust their ex-ante CEO pay levels upwards as they learn about the MW hike. In contrast, control firms are not able to adjust their CEO pay levels even if it is optimal for them to do so since their negotiation time was prior to May 25. To explore this expectation, I develop a triple-differences model summarized in Equation 3:

$$\begin{aligned}\log(\text{CEO pay}_{i,j,t}) = {} & \beta_0(\text{Proxy date after May } 25_i \times \text{Affected}_j \times \%\text{Bound}_i) \\ & + \beta_1(\text{Proxy date after May } 25_i \times \text{Affected}_j) \\ & + \beta_2(\text{Proxy date after May } 25_i \times \%\text{Bound}_i) + \beta_3(\text{Affected}_j \times \%\text{Bound}_i) \\ & + \beta_4\text{Proxy date after May } 25_i + \beta_5\text{Affected}_j + \beta_6\%\text{Bound}_i \\ & + \Gamma\text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \epsilon_{i,j,t},\end{aligned} \tag{3}$$

where CEO pay is a CEO pay component (total pay, 1 + salary, 1 + bonus, 1 + cash pay, or 1 + incentive pay) in firm i of industry j at period t; Affected is a dummy variable equal to 1 if a firm belongs to a NAICS2 71, 72, 44, or 45 industry, %Bound is the share of firm workers employed at establishments located in bound states, X is a vector of firm characteristics, δ are industry by year fixed effects (Gormley and Matsa (2014)), and γ are firm fixed effects. The time horizon of the Equation 3 corresponds to the year 2007 only. The key difference between Equation 3 and Equation 1 is the variable "Proxy date after May 25". This is a dummy variable equal to 1 for treated firms and 0 for control firms. This definition follows the existing literature that treats the CEO pay negotiation date as the date of filing the DEF-14A form, where firms are required by the SEC to disclose executive compensation levels (Bebchuk et al. (2011)).

Crucially, this methodology is particularly fruitful for disentangling the efficiency wages mechanism from the other potential mechanisms at work. Since I consider CEO pay changes during the calendar year 2007 only, the observed effects (if any) *cannot* be driven by changes in firm productivity since firms plausibly do not adjust productivity within such a short time frame. Therefore, in this setting, a positive estimate of β_0 is inconsistent with the efficiency wages story.

Panel B of Table 6 presents the estimation results of Equation 3 on the sample of smaller firms. Column (1) shows that β_0 is positive, statistically significant, and equal to 3.241. This estimate is also economically significant: a 10-percentage point increase in the share of employees located in bound states leads to a 32.41% increase in CEO total pay levels for treated vs. control, affected vs. unaffected firms after a MW hike. This evidence goes against the efficiency wages mechanism.

As a result, taking together the evidence established in Sections 6.1.1 and 6.1.2, I conclude that the efficiency wages hypothesis is unlikely to be driving the CEO pay raises observed after the MW shock in smaller firms.

6.2. Bargaining hypothesis

In this section, I test the bargaining mechanism by analyzing the magnitudes of the total pay spillover effects coming from the MW hike in two cases: for smaller firm CEOs with high and low bargaining power. To do that, I estimate Equation 1 separately on the subsamples of high-rent and low-rent firms. I follow the literature and use standard productivity measures, industry-adjusted ROA and profitability (e.g., Kim (2020)), as proxies for firm rents to be shared. In particular, I split the smaller firm sample into two parts based on the sample median of average rents-to-be-shared measure at the pre-treatment period and compare the values of β_0 estimates for both subsamples. An estimate of β_0 that is larger for high-rent firms than for low-rent firms is consistent with the bargaining mechanism since it implies that CEOs of high-rent firms who have stronger bargaining power faced more significant total pay raises after the MW hike raised worker wages.

Table 7 presents the estimation results, with columns (1) and (2) corresponding to the industry-adjusted ROA proxy for firm rents to be shared and columns (3) and (4) - to the profitability proxy. In both settings, β_0 estimate is much higher for low-rent firms than for high-rent firms. In particular, when industry-adjusted ROA is used as a proxy, the β_0 estimate is weakly statistically significant for low-rent firms and insignificant for high-rent firms. Moreover, when profitability is used as a proxy, the magnitude of β_0 for high-rent firms is economically insignificant (0.04), while for low-rent firms, the magnitude is much larger and over 1 (1.374). Taken together, this evidence suggests that the empirical patterns of β_0 coefficient estimates are inconsistent with the bargaining story: the difference in CEO bargaining power is unlikely to drive my result of CEO pay increases after the MW hike.

6.3. Fairness hypothesis

In this section, I assess the plausibility of the fairness mechanism driving my main results by testing the heterogeneity in total pay CEO spillovers following the MW hike based on CEO age. To do that, I estimate Equation 1 on the subsample of younger and older CEOs. In particular, I split the subsample of smaller firms into two parts by median CEO age at $t = -1$. At that time, "younger" CEOs are between 32 and 57 years old (with a mean of 49 years old), while "older" CEOs are between 58 and 96 years old (with a mean of 65 years old). An estimate of β_0 that is larger for younger CEOs is consistent with the fairness mechanism: it illustrates that younger CEOs who care relatively more about fair compensation levels (either due to career concerns (Gibbons and Murphy (1992)) or intertemporal risk-sharing (Edmans et al. (2012))) face larger total pay raises after worker wage reference point is shifted upwards due to the MW hike.

Table 8 presents the estimation results. Indeed, the β_0 estimate is more statistically and economically significant for younger CEOs of smaller firms (column (1)) than for older CEOs (column (2)). The economic magnitude of the effect is that a 10% increase in the share of workers located in bound states leads to a 10% increase in total pay level for younger CEOs of smaller affected vs. unaffected firms after the MW hike. This effect is over 2.5 times smaller for older CEOs, equal to only 3.92%. These estimates serve as evidence in favor of the fairness story. Indeed, since being compensated fairly matters more for younger CEOs, when the "fair" compensation reference point of worker wages becomes higher, younger CEOs require an over 2.5 times larger corresponding "fair" pay increase for themselves compared to older CEOs. I conclude that it is likely that the fairness mechanism is driving the result of higher CEO pay after the MW hike in smaller firms.

7. Robustness

In the above analysis, I consider the three potential mechanisms based on the efficiency wages (Section 6.1), CEO bargaining power (Section 6.2), and fairness theory (Section 6.3), that may potentially explain the positive spillover effects from worker wages to CEO total pay after the MW law. I show evidence that is most consistent with the fairness story and largely inconsistent with the two other stories.

This analysis relies on several assumptions. One assumption is that firm profitability, which is directly correlated with worker wages and CEO pay, does not change differentially around the MW hike for affected and unaffected firms with the same employment share in bound states. The second assumption expands the first one, stating that affected and unaffected firms with the same employment share in bound states are not systematically different with respect to observable firm characteristics (including profitability). This assumption represents randomized treatment, which is crucial for the validity of the triple-differences analysis. The third assumption claims that differential state-level economic conditions are not responsible for driving my triple-differences results. This assumption is important since my identification strategy relies on the state-level variation in firm exposure to the MW hike. Violation of any of these assumptions gives rise to doubts about endogeneity driving the results. Therefore, in this section, I address the robustness of the established results to each of these critical assumptions.

7.1. Profitability hypothesis

First, playing the Devil's Advocate, one may argue that a government policy such as MW change may affect many potential firm-level outcomes in addition to worker wages. If this is the case, my results would suffer from endogeneity concerns. In my methodology, I attempt to control for a wide range of such outcomes. In particular, I control for lagged firm size, ROA, profitability, Tobin's Q, market-to-book, and log(1+annualized return); I include industry by year fixed effects to control for time-varying industry shocks (Gormley and Matsa (2014)), and firm fixed effects to control for unobserved firm heterogeneity.

Nevertheless, for robustness purposes, in this section, I consider the effect of the MW hike on firm profitability. Profitability is a natural and important outcome variable of interest because the alternative scenario based on Hypothesis 2 claims that MW hikes, being an unexpected payroll expense for the firm, may affect firm profitability *negatively*, leading to a corresponding *drop* in CEO pay levels. Therefore, if my established results of CEO pay *raises* after the MW hike are accurate, I expect to find no significant adverse effect on firm profitability around the MW law enactment date. To test the "profitability" hypothesis, I estimate a variation of Equation 1 using firm profitability as a dependent variable. An estimate of β_0 that is statistically significant and negative will undermine the plausibility of my results, while all the other estimates of β_0 will imply that my results are robust and unlikely to suffer from endogeneity concerns.

Table A.2 illustrates the estimation results, with each column corresponding to one of the subsamples used throughout this paper. Column 1 summarizes baseline results for smaller firms; column 2 reflects the sample of smaller firms that filed their proxy statements in 2007 only (efficiency wages mechanism); columns 3 and 4 contain the results for the sample of high-rent and low-rent smaller firms, respectively (bargaining mechanism); and columns 5 and 6 illustrate the results for the sample of smaller firm younger and older CEOs, respectively (fairness mechanism). Table A.2 shows that, almost always, the estimates of β_0 are either near zero (columns 1 - 3)) or positive (columns 4 and 5).[35] Only in column 6, which corresponds to the sample of older smaller firm CEOs used to implement the fairness hypothesis test, the estimate of β_0 is negative and significant at 10% level - however, this result is weak both in terms of the magnitude and statistical significance. Overall, the evidence presented in Table A.2 enhances the plausibility of my results that following a MW hike that boosts worker wages, CEOs observe compensation raises for themselves.

7.2. Matched sample

An alternative concern is based on the fact that my identification strategy relies on comparing CEO pay outcomes for firms based on their industry (affected vs. unaffected). The idea is that the MW hike will largely impact affected firms that comprise restaurants, hotels, grocery stores, and other businesses majorly relying on MW labor. In contrast, the MW hike will not directly impact unaffected firms that cover all other industries with a small (if any) share of MW workers.[36]

Based on this argument, one may worry that firms belonging to different business segments may be fundamentally different with respect to observable firm characteristics. If this is the case,

[35]Investigation of the reasons why firm profitability increases for low-rent smaller firms and younger smaller firm CEOs is outside of the scope of this paper and is left for future research.

[36]See Section 2.2 for a detailed discussion.

the causality between the worker wage raise and the CEO pay raise through the MW hike will be confounded. Indeed, if affected and unaffected firms are not comparable with respect to observable characteristics, these differences in characteristics may be responsible for driving the results. In the baseline specification of Equation 1, I include control variables, industry by year fixed effects, and firm fixed effects to take care of this concern in a straightforward way.

Moreover, to further mitigate this concern, I construct a matched sample out of the main sample of firms. To do that, I first classify the firm-years into "bound" or "unbound" to make sure that firms are comparable with respect to the exposure to the MW hike effects intensity. A firm is bound if it has more than the sample median of 15.35% employees located in bound states at $t = -1$; and unbound otherwise.[37] I then match each bound firm with one unbound firm 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), and I require an exact match on NAICS2 industry code (making sure that (un)affected firms are matched with (un)affected firms).

The resulting matched sample consists of 1,654 firms per period. Table A.3 shows that matched bound and unbound firms do not significantly differ on the pre-treatment CEO pay levels and firm characteristics. I further estimate Equation 1 using CEO total pay as a dependent variable on the matched sample of smaller, larger, and all firms. If my results are robust, I expect to find a positive and significant estimate of β_0 for smaller firms, an insignificant estimate for larger firms, and a weakly (if at all) positive and significant estimate for all firms. Columns 1 - 3 of Table A.4 illustrate that these patterns hold. This evidence shows that differences in observable firm characteristics between firms exposed to treatment and not exposed to treatment are unlikely to drive my results.

7.3. Bordering counties sample

Another potential concern is based on the fact that my identification strategy exploits the variation in the location of workers across bound and unbound states (%Bound variable in Equation 1). The idea is that the MW hike will largely and primarily affect firms with a high share of employees in bound states compared to firms with a low employment share in bound states. This is because, in bound states, the federal MW level is binding and, hence, determines worker wage level.[38]

Therefore, one may worry that the MW policy may be regulated to impact firms differentially across states. If this is the case, the causality between worker wages and CEO pay levels through the MW hike will be questionable due to the resulting endogeneity problem. To address this concern, I need to show that state-level shocks are unlikely to be responsible for my results. I do that by

[37] 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.

[38] See Section 2.1 for a detailed discussion.

applying the border analysis method used in the literature (e.g., Holmes (1998), Dube et al. (2010), Gustafson and Kotter (2022), etc.). This method relies on the claim that firms headquartered on opposing sides of the state border but very close to each other (in neighboring counties) 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. First, 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 "bordering counties" sample of 804 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. If my results are robust, the coefficient β_0 will be positive and significant and more pronounced for smaller firms than for larger and all firms. Columns 4 - 6 of Table A.4 show that these patterns hold. This evidence hints that differences in state-level economic conditions are unlikely to drive my results.

8. Conclusion

Using a sample of smaller and medium-sized U.S. public firms matched to granular establishment-level location and employment data, I document a positive spillover effect of worker wages on CEO compensation levels. Additionally, I evaluate the role of efficiency wages, CEO bargaining power, and CEO fairness concerns as potential mechanisms at force.

Large U.S. public firm CEO-worker pay gaps throughout the past two decades reflect that American society is relatively tolerant of income and corporate inequality. Despite that, I establish positive causality between CEO and worker pay in the U.S. I document that a 41% increase in the federal MW level mandated by the Fair Minimum Wage Act of 2007 results in a 2.6% increase in CEO compensation levels during the three consequent fiscal years. Using a triple-differences specification, I estimate that a 10% increase in the share of workers located in states bound by federal MW levels before the hike leads to a 7.7% increase in CEO pay for firms in MW-sensitive industries vs. other industries.

I interpret my results as most consistent with the fairness hypothesis of executive compensation (Edmans et al. (2023)). According to the hypothesis, CEOs treat worker wage level as a reference point for their compensation and require a "fair" pay raise for themselves following an exogenous worker wage raise. Indeed, I find that younger CEOs who should have stronger fairness concerns due to longer career horizons face over 2.5 times larger total pay raises than older CEOs.

I do not find evidence consistent with either efficiency wages or CEO bargaining power driving the link between CEO and worker pay. In particular, firm performance does not significantly improve after the MW hike, and CEOs of high-rent firms who could plausibly extract these rents due to their strong bargaining power do not face larger pay increases compared to low-rent firm CEOs. Moreover, using an alternative triple-difference identification strategy, I show that firms that have not yet filed proxy statements before the MW hike disclose significantly larger CEO pay levels in the first year after the hike compared to the rest of the firms. Since significant changes in firm productivity cannot manifest within a short time frame of one year, this result serves as additional evidence inconsistent with the efficiency wages story.

Future research is necessary to understand the connection between worker pay and CEO pay in more detail. Does the spillover effect extend to the other members of the C-suite, or is it pronounced for CEOs only? How do firms adjust employment and production in response to the federal MW hike? Is CEO motivation affected by the fair pay raise, as predicted by the fairness hypothesis? These are just a few examples of open questions to be addressed within the fruitful area of worker and CEO pay link.

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Tables and figures

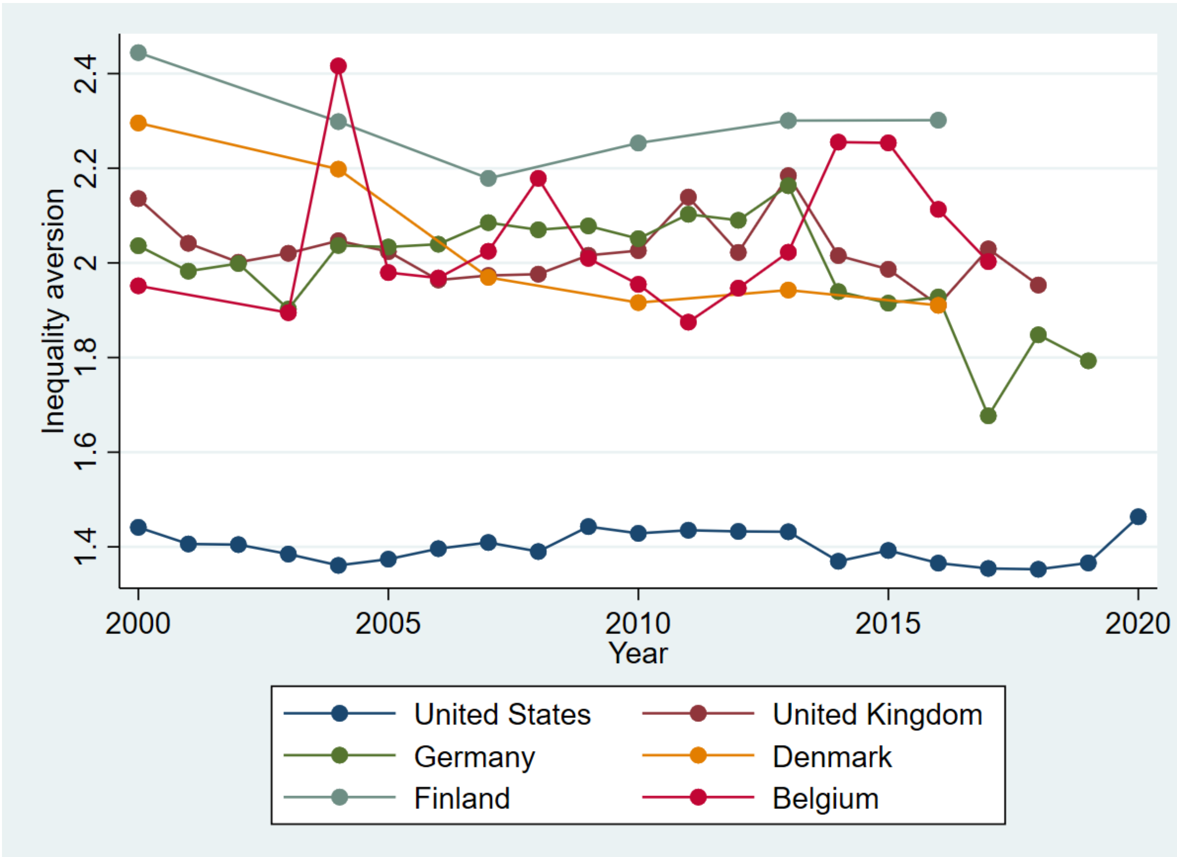


Figure 1: Inequality aversion index, by country

The figure illustrates the trends in the inequality aversion parameter ϵ over time for the U.S. and several European countries. The estimate values are created by Kot and Paradowski (2022b). The inequality aversion parameter estimates data comes from Kot and Paradowski (2022a).

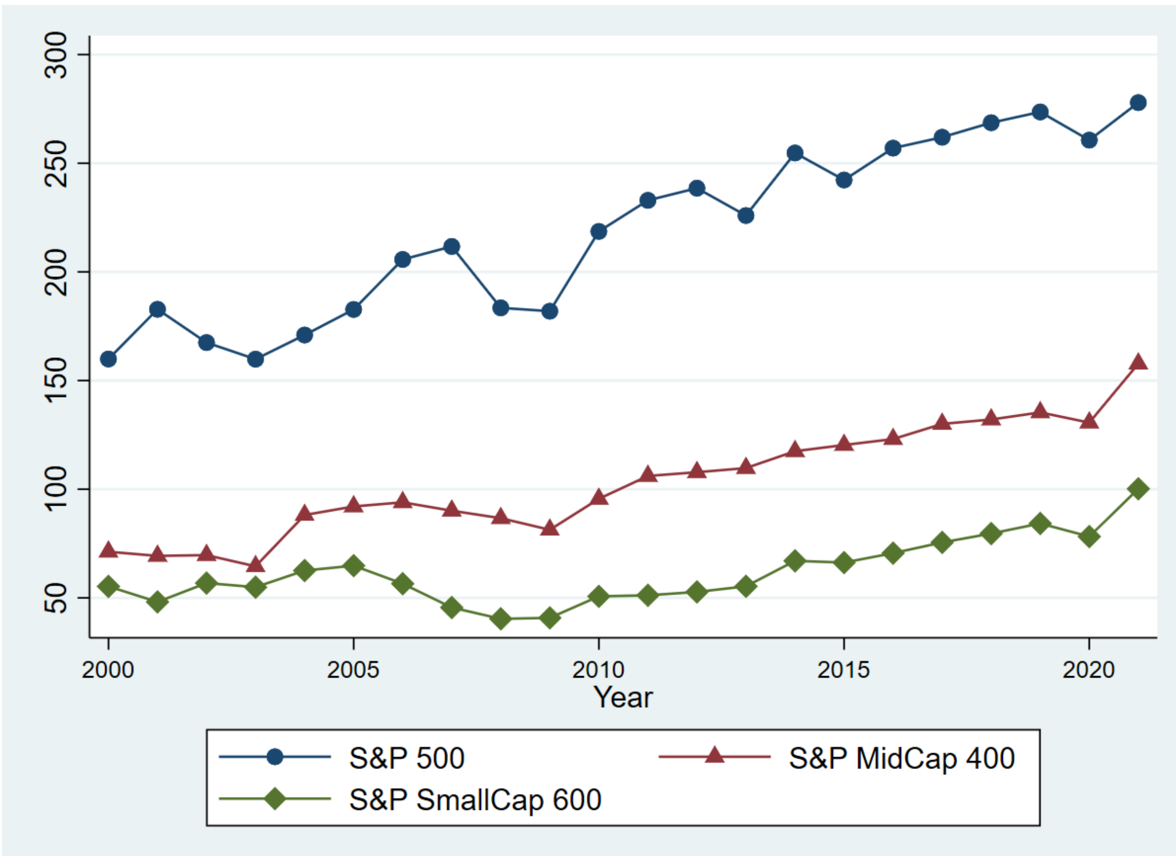


Figure 2: Evolution of the CEO-employee pay ratio, by firm size

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.

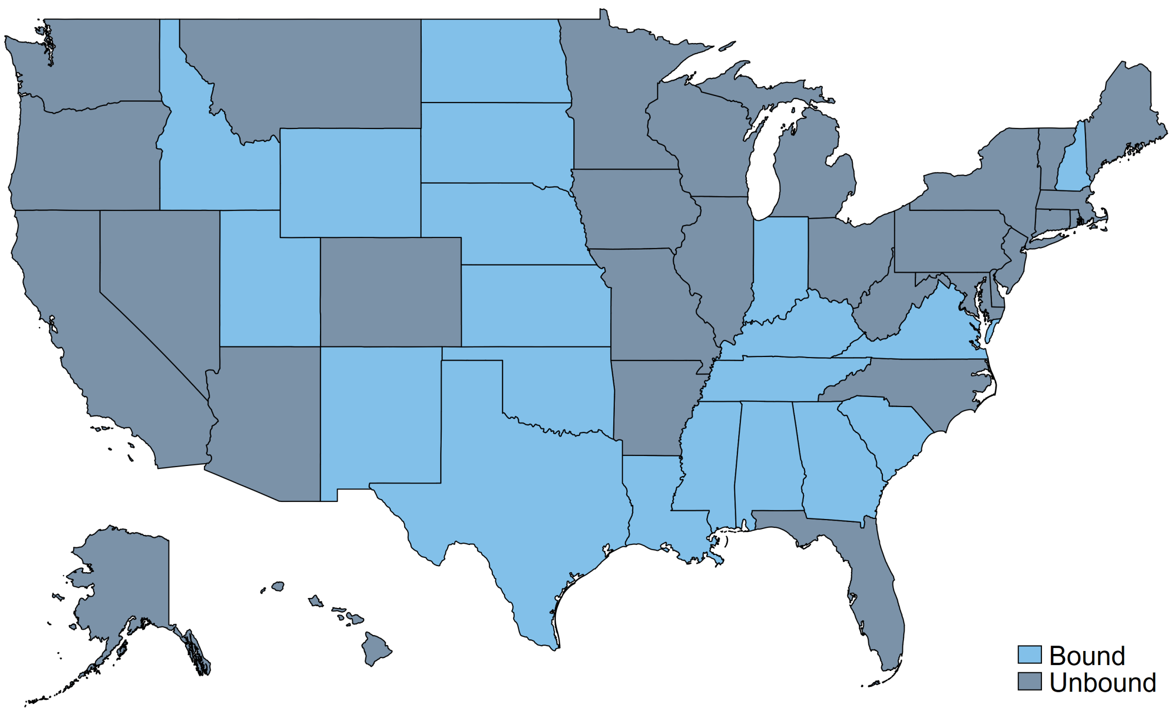
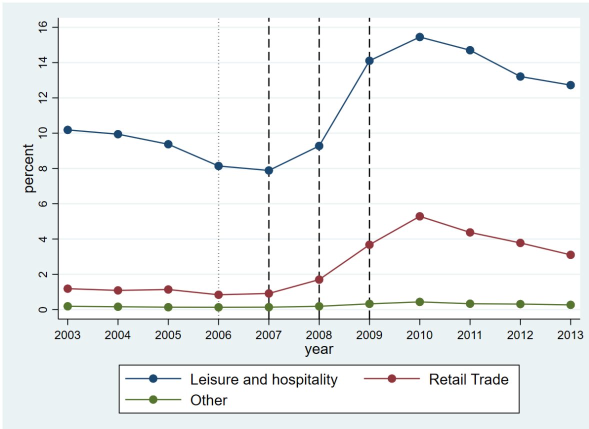
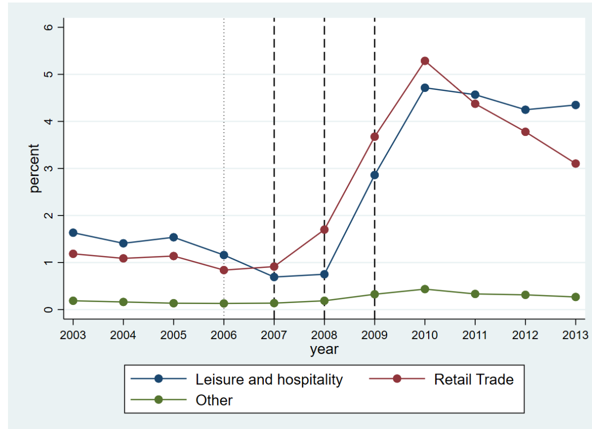


Figure 3: Map of bound and unbound states

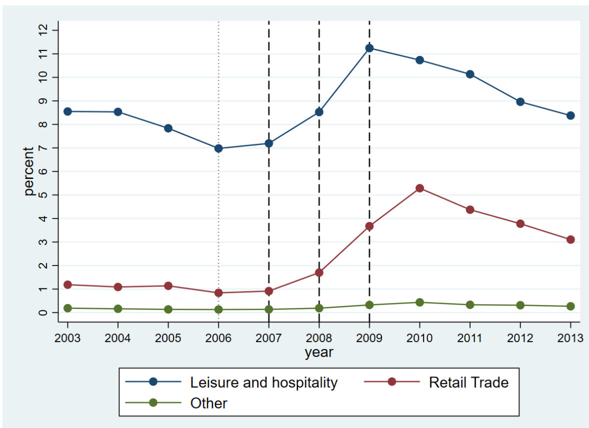
The figure illustrates the distribution of bound and unbound states defined pre-introduction of the law (Gustafson and Kotter (2022)). A state is bound if the state minimum wage is lower than the federal minimum wage. A state is unbound if the opposite holds. Since an employee receives the largest of 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 4: 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).

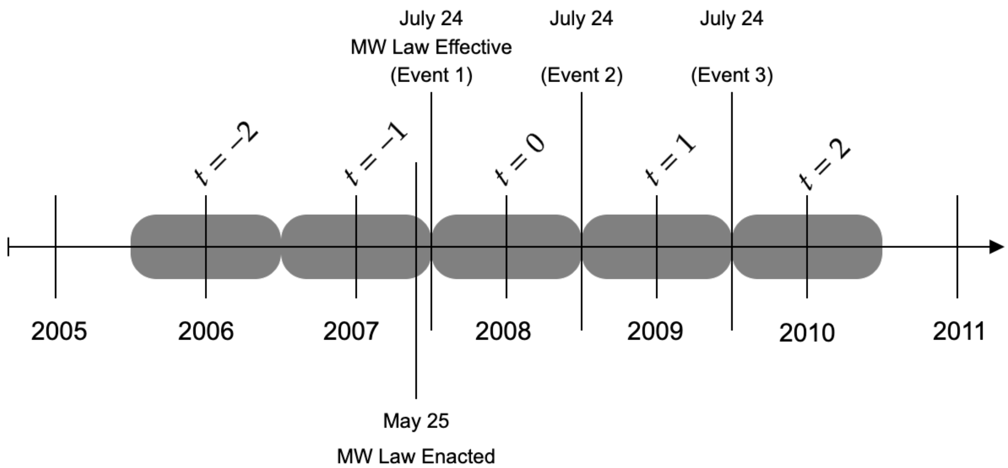


Figure 5: Timeline

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

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

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 A.1. 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
Total pay ('000)	1,964.032	604.562	4,139.117	52.050	8,466.318	8,450
Salary ('000)	442.774	350.000	321.702	106.756	1,040.769	6,115
Bonus ('000)	327.395	0.000	1,231.311	0.000	1,521.100	5,830
Cash pay ('000)	824.651	468.269	1,417.134	131.250	2,550.000	5,294
Incentive pay ('000)	1,926.392	391.029	4,349.935	0.000	9,045.228	5,294
Affected	0.051	0.000	0.221	0.000	1.000	8,450
%Bound	0.285	0.154	0.331	0.000	1.000	8,450
# employees	7,303.896	1,315.000	25,066.130	48.000	31,528.000	8,450
Labor expense per employee ('000)	125.851	56.321	329.922	18.683	392.536	2,177
SG&A expense per employee ('000)	918.063	87.703	12,276.067	13.248	833.955	7,103
Operating expense per employee ('000)	3,838.351	352.287	57,000.642	74.774	2,892.339	8,450
Total assets (in MM)	13,922.908	1,096.397	96,262.771	42.396	34,417.000	8,450
Size	7.070	7.001	1.987	3.747	10.446	8,449
M/B	1.770	1.299	1.295	0.856	4.212	8,449
ROA	0.016	0.029	0.168	-0.246	0.181	8,449
Profitability	0.078	0.093	0.171	-0.147	0.271	8,449
Q	1.775	1.306	1.296	0.866	4.214	8,449
log(1+annual return)	-0.050	0.018	0.538	-1.001	0.672	8,450

Panel B: Smaller firms

	Mean	Median	Std	5th	95th	N
Total pay ('000)	1,025.576	438.363	2,230.131	41.032	3,804.744	5,640
Salary ('000)	336.547	285.000	224.257	96.923	750.000	3,961
Bonus ('000)	154.490	0.000	485.972	0.000	734.902	3,809
Cash pay ('000)	524.997	373.846	623.600	114.833	1,396.568	3,393
Incentive pay ('000)	868.364	183.700	2,439.469	0.000	3,886.960	3,393
Affected	0.027	0.000	0.161	0.000	0.000	5,640
%Bound	0.268	0.051	0.364	0.000	1.000	5,640
# employees	895.363	537.000	1,327.318	29.000	2,568.500	5,640
Labor expense per employee ('000)	122.251	52.726	362.747	23.459	414.447	1,678
SG&A expense per employee ('000)	1,240.976	95.913	14,455.660	22.173	1,169.310	4,731
Operating expense per employee ('000)	5,261.930	344.732	69,119.481	86.053	3,560.633	5,640
Total assets (in MM)	1,892.991	605.554	4,373.589	31.354	7,775.067	5,640
Size	6.305	6.406	1.652	3.445	8.961	5,639
M/B	1.814	1.247	1.432	0.827	4.739	5,639
ROA	-0.004	0.016	0.195	-0.344	0.195	5,639
Profitability	0.050	0.066	0.194	-0.257	0.264	5,639
Q	1.819	1.254	1.432	0.836	4.739	5,639
log(1+annual return)	-0.068	-0.005	0.564	-1.072	0.717	5,640

Panel C: Larger firms

	Mean	Median	Std	5th	95th	N
Total pay ('000)	3,847.623	1,711.590	6,019.088	122.500	15,249.886	2,810
Salary ('000)	638.115	553.868	377.434	180.385	1,285.000	2,154
Bonus ('000)	653.271	0.000	1,940.920	0.000	2,900.000	2,021
Cash pay ('000)	1,359.488	824.615	2,110.406	221.192	4,070.000	1,901
Incentive pay ('000)	3,814.812	1,517.874	6,043.289	0.000	14,710.966	1,901
Affected	0.101	0.000	0.302	0.000	1.000	2,810
%Bound	0.320	0.276	0.247	0.009	0.874	2,810
# employees	20,166.574	8,615.000	40,476.527	3,120.000	69,637.000	2,810
Labor expense per employee ('000)	137.958	78.292	179.757	7.503	372.663	499
SG&A expense per employee ('000)	274.007	72.736	5,823.114	3.917	410.319	2,372
Operating expense per employee ('000)	981.062	365.576	13,027.218	34.049	1,869.544	2,810
Total assets (in MM)	38,068.364	4,719.477	164,194.874	413.119	119,764.000	2,810
Size	8.604	8.459	1.696	6.024	11.693	2,810
M/B	1.681	1.386	0.959	0.907	3.428	2,810
ROA	0.055	0.053	0.080	-0.069	0.171	2,810
Profitability	0.133	0.126	0.089	0.013	0.280	2,810
Q	1.687	1.393	0.960	0.914	3.442	2,810
log(1+annual return)	-0.013	0.054	0.481	-0.853	0.585	2,810

Table 3: Did the minimum wage law raise worker 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 $t \in [-2; 2]$. 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

	Labor expense	SG&A expense	Operating expense
	(1) log(XLR per emp.)	(2) log(SG&A per emp.)	(3) log(XOPR per emp.)
PostXAffectedX%Bound	4.099***	0.488*	0.519*
	(8.18)	(1.89)	(1.99)
PostXAffected	0.053	-0.231	-0.181
	(0.21)	(-0.60)	(-0.44)
PostX%Bound	0.062***	0.050**	-0.003
	(7.77)	(2.40)	(-0.06)
AffectedX%Bound	-18.167***	-3.672***	-4.030***
	(-32.71)	(-3.41)	(-4.06)
Post	-0.020***	0.143	0.065**
	(-3.69)	(1.58)	(2.46)
%Bound	0.156**	-0.061	0.417**
	(2.65)	(-0.32)	(2.18)
Constant	4.211***	4.299***	4.710***
	(12.04)	(9.36)	(16.61)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.9	.93	.91
N	1,410	2,745	5,640

Panel B: Larger firms

	Labor expense	SG&A expense	Operating expense
	(1)	(2)	(3)
	log(XLR per emp.)	log(SG&A per emp.)	log(XOPR per emp.)
PostXAffectedX%Bound	-0.249	0.322	0.031
	(-0.45)	(1.43)	(0.16)
PostXAffected	0.084	-0.097	-0.038
	(0.38)	(-0.98)	(-0.54)
PostX%Bound	-0.051	-0.089	-0.014
	(-1.08)	(-0.96)	(-0.23)
AffectedX%Bound	-3.405***	-0.568	-0.408
	(-4.90)	(-1.21)	(-0.87)
Post	-0.009	0.070	0.017
	(-0.08)	(1.40)	(0.51)
%Bound	0.590**	0.289	0.126
	(2.55)	(1.20)	(0.68)
Constant	4.113***	3.434***	4.607***
	(3.35)	(5.55)	(8.29)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.96	.98	.96
N	700	1,370	2,810

Panel C: All firms

	Labor expense	SG&A expense	Operating expense
	(1) log(XLR per emp.)	(2) log(SG&A per emp.)	(3) log(XOPR per emp.)
PostXAffectedX%Bound	0.777	0.642***	0.447**
	(0.58)	(2.89)	(2.33)
PostXAffected	0.409***	-0.351**	-0.258
	(2.86)	(-2.50)	(-1.64)
PostX%Bound	0.039***	0.034	0.004
	(4.26)	(1.56)	(0.12)
AffectedX%Bound	-13.397***	-2.520**	-3.139***
	(-30.31)	(-2.49)	(-3.25)
Post	0.046	0.109**	0.050***
	(0.86)	(2.19)	(2.69)
%Bound	0.335**	-0.052	0.344**
	(2.54)	(-0.31)	(2.34)
Constant	4.532***	4.354***	4.746***
	(12.99)	(9.21)	(17.83)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.93	.95	.92
N	2,110	4,115	8,450

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

	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	0.766***	0.532	0.609**	-0.124	5.310***
	(3.84)	(1.24)	(2.43)	(-0.13)	(5.04)
PostXAffected	-1.223**	-0.327	-0.525	-0.210	-2.481*
	(-2.34)	(-1.52)	(-1.29)	(-0.19)	(-1.75)
PostX%Bound	-0.103**	0.076	0.067	-0.088	-0.276
	(-2.43)	(1.19)	(1.16)	(-0.50)	(-1.02)
AffectedX%Bound	0.275	-2.081***	0.626*	-1.571	10.244*
	(0.47)	(-2.97)	(1.87)	(-0.20)	(1.77)
Post	0.247	-0.051	0.048	-0.135	0.429
	(1.63)	(-0.42)	(0.60)	(-0.48)	(1.01)
%Bound	0.251	-0.100	-0.114	-0.081	0.383
	(1.25)	(-0.29)	(-0.35)	(-0.11)	(0.72)
Constant	4.450***	5.736***	4.969***	6.471***	1.369
	(9.31)	(12.73)	(12.33)	(4.42)	(1.13)
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	.41	.36	.42	.57
N	5,640	3,393	3,961	3,809	3,393

Panel B: Larger 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	0.756	0.817	0.642	-0.299	1.480
	(1.64)	(1.17)	(1.33)	(-0.16)	(0.69)
PostXAffected	0.118	-0.573*	-0.480**	-1.673*	3.763***
	(0.49)	(-1.84)	(-2.45)	(-1.88)	(4.06)
PostX%Bound	-0.139	-0.132	-0.016	0.501	-0.693*
	(-0.84)	(-0.78)	(-0.15)	(1.00)	(-1.74)
AffectedX%Bound	1.428	-1.054	-1.715	2.028	-7.334
	(0.49)	(-1.00)	(-1.62)	(1.29)	(-1.53)
Post	-0.175	-0.041	-0.002	-0.118	0.743*
	(-0.92)	(-0.28)	(-0.02)	(-0.22)	(1.68)
%Bound	0.296	-0.672**	-0.336	-3.122**	-1.207
	(0.57)	(-1.99)	(-1.34)	(-2.38)	(-1.36)
Constant	6.517***	7.376***	5.784***	7.401**	3.725
	(4.84)	(5.06)	(6.01)	(2.10)	(1.48)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.4	.36	.27	.48	.54
N	2,810	1,901	2,154	2,021	1,901

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	0.588**	0.378	0.467	-0.498	2.163*
	(2.46)	(0.81)	(1.49)	(-0.38)	(1.72)
PostXAffected	-0.554	-0.111	-0.407	-0.344	0.751
	(-1.08)	(-0.40)	(-1.29)	(-0.55)	(0.43)
PostX%Bound	-0.099**	0.017	0.037	-0.098	-0.443*
	(-2.17)	(0.36)	(0.79)	(-0.53)	(-1.72)
AffectedX%Bound	0.830	-0.455	0.146	1.707	-0.641
	(0.78)	(-0.51)	(0.39)	(0.70)	(-0.10)
Post	0.048	-0.046	0.020	-0.046	0.351
	(0.44)	(-0.44)	(0.32)	(-0.16)	(1.01)
%Bound	0.253	-0.080	-0.057	-0.281	0.287
	(1.36)	(-0.30)	(-0.22)	(-0.40)	(0.65)
Constant	4.838***	5.863***	5.101***	5.886***	1.951*
	(10.89)	(13.09)	(13.63)	(3.48)	(1.80)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.53	.49	.44	.44	.62
N	8,450	5,294	6,115	5,830	5,294

Table 5: Semi-elasticity of CEO pay and worker pay with respect to minimum wages

The table displays the results of DDD regressions with logarithm of CEO pay (Panel A) and worker pay (Panel B) as dependent variables. The coefficient on the DDD interaction represents the semi-elasticity of compensation with respect to minimum wages. Panel A shows the specifications 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 of Panel A is main and includes all smaller firms that have positive total pay observations and non-missing lagged control variable observations. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. Panel B shows the specifications 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. The sample of Panel B is described in the caption of Table 3. 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. 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. Dependent variable: log(CEO pay)

	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)
%Δ MW X Affected X %Bound	0.064***	0.042	0.051***	0.004	0.405***
	(4.73)	(1.54)	(2.79)	(0.06)	(5.10)
%Δ MW X Affected	-0.084**	-0.017	-0.037	-0.011	-0.178
	(-2.05)	(-0.93)	(-1.17)	(-0.16)	(-1.55)
%Δ MW X %Bound	-0.008**	0.005	0.005	-0.011	-0.022
	(-2.08)	(1.02)	(1.07)	(-0.78)	(-1.01)
Affected X %Bound	0.251	-2.131***	0.588*	-1.603	9.906*
	(0.41)	(-3.00)	(1.73)	(-0.21)	(1.73)
%Δ MW	0.013	-0.006	0.001	-0.013	0.044
	(1.22)	(-0.70)	(0.12)	(-0.70)	(1.47)
%Bound	0.247	-0.094	-0.110	-0.069	0.379
	(1.23)	(-0.27)	(-0.34)	(-0.09)	(0.72)
Constant	4.504***	5.738***	4.988***	6.468***	1.317
	(9.63)	(12.85)	(12.38)	(4.40)	(1.10)
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	.41	.36	.42	.57
N	5,640	3,393	3,961	3,809	3,393

Panel B: Smaller firms. Dependent variable: log(worker pay)

	Labor expense	SG&A expense	Operating expense
	(1) log(XLR per emp.)	(2) log(SG&A per emp.)	(3) log(XOPR per emp.)
%Δ MW X Affected X %Bound	0.314*** (10.44)	0.037* (1.85)	0.040** (2.02)
%Δ MW X Affected	0.007 (0.49)	-0.016 (-0.58)	-0.010 (-0.36)
%Δ MWX%Bound	0.004*** (7.29)	0.003** (2.04)	-0.000 (-0.13)
Affected X %Bound	-17.729*** (-42.71)	-3.662*** (-3.39)	-3.997*** (-4.02)
%Δ MW	-0.003*** (-3.62)	0.009 (1.43)	0.004** (2.25)
%Bound	0.157** (2.65)	-0.058 (-0.30)	0.418** (2.19)
Constant	4.204*** (12.16)	4.319*** (9.41)	4.718*** (16.62)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Adj-R-squared	.9	.93	.91
N	1,410	2,745	5,640

Table 6: Mechanism: Efficiency wage hypothesis

The table displays the results of testing the efficiency wage hypothesis as a potential mechanism explaining the main results of Table 4. Panel A reflects the DDD regression estimation results of the original methodology summarized in a variation of Equation 1 with firm industry-adjusted ROA and logarithm of 1+annualized return as dependent variables. In Panel A, the time horizon is $t \in [-2; 2]$. Panel B reflects the DDD regression estimation results of the new methodology summarized in Equation 3 based on the variation in firm CEO pay setting dates (see Section 6.1.2). In Panel B, the time horizon is the year 2007. The sample is main and includes all smaller firms that have positive total pay observations and non-missing lagged control variable observations. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Proxy date after May 25, 2007 is an indicator equal to 1 for firms that disclosed CEO pay for 2007 after May 25. 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, industry, 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. Original methodology

	(1) Industry-adjusted ROA	(2) log(1 + annualized return)
PostXAffectedX%Bound	0.056	0.194
	(0.81)	(0.53)
PostXAffected	0.047	0.004
	(1.07)	(0.02)
PostX%Bound	-0.001	0.076*
	(-0.06)	(1.82)
AffectedX%Bound	-0.004	-1.094***
	(-0.08)	(-2.74)
Post	-0.018*	0.114
	(-1.74)	(1.50)
%Bound	0.003	-0.077
	(0.18)	(-0.81)
Constant	0.174***	2.974***
	(2.75)	(12.53)
Controls	Yes	Yes
Industry-Year FE	Yes	Yes
Firm FE	Yes	Yes
Adj-R-squared	.6	.45
N	5,635	5,640

Panel B: Smaller firms. New methodology: variation in proxy filing dates

	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)
Proxy date after May 25, 2007XAffectedX%Bound	3.241***	2.883***	-0.238	6.418	2.770
	(2.86)	(4.75)	(-0.25)	(1.17)	(0.50)
Proxy date after May 25, 2007XAffected	-1.734*	-0.916***	-0.082	-1.605	-1.428
	(-1.79)	(-3.46)	(-0.20)	(-0.43)	(-0.35)
Proxy date after May 25, 2007X%Bound	-0.113	0.083	0.297	-1.253	-0.029
	(-0.19)	(0.23)	(1.11)	(-1.13)	(-0.02)
AffectedX%Bound	-0.099	-0.041	1.451	0.034	1.033
	(-0.18)	(-0.11)	(1.10)	(0.03)	(0.93)
Proxy date after May 25, 2007	0.287	-0.107	-0.031	0.238	-0.648
	(0.85)	(-0.65)	(-0.24)	(0.37)	(-1.28)
%Bound	-0.201*	-0.204	-0.196**	-0.477**	-0.149
	(-1.79)	(-1.63)	(-2.24)	(-2.30)	(-0.73)
Constant	3.059***	4.603***	4.524***	1.861***	0.175
	(15.50)	(22.72)	(28.14)	(3.99)	(0.71)
Controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.24	0.21	0.18	0.08	0.40
N	1,280	620	832	747	620

Table 7: Mechanism: Bargaining hypothesis

The table displays the results of testing the bargaining hypothesis as a potential mechanism explaining the main results of Table 4. It shows the estimates 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 smaller firms that have positive total pay observations and non-missing lagged control variable observations. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. Time horizon is $t \in [-2; 2]$. The sample is split by firm industry-adjusted ROA (columns 1 and 2) and by firm profitability (columns 3 and 4) into subsamples of high-rent firms (i.e., belonging to the top half of the sample at pre-treatment periods) and low-rent firms (i.e., belonging to the bottom half of the sample at pre-treatment periods). 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.

Smaller firms. Dependent variable: log(CEO total pay)

	Industry-adjusted ROA		Profitability	
	(1) High-rent	(2) Low-rent	(3) High-rent	(4) Low-rent
PostXAffectedX%Bound	0.513	1.251*	0.040	1.374
	(1.45)	(2.00)	(0.07)	(1.16)
PostXAffected	0.749	-2.632***	-0.798**	-1.823***
	(1.33)	(-5.72)	(-2.33)	(-3.74)
PostX%Bound	-0.066	-0.141	-0.000	-0.095**
	(-0.54)	(-1.47)	(-0.00)	(-2.32)
AffectedX%Bound	4.050***	0.362	2.115**	1.807**
	(3.20)	(0.66)	(2.02)	(2.59)
Post	-0.054	0.468**	0.077	0.399
	(-0.24)	(2.19)	(0.49)	(1.43)
%Bound	0.269	0.310	0.234	0.576*
	(1.14)	(0.85)	(0.81)	(1.72)
Constant	5.074***	3.182***	5.166***	3.415***
	(9.07)	(5.68)	(9.57)	(4.83)
Controls	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Adj-R-squared	.46	.43	.47	.4
N	2,815	2,820	2,815	2,820

Table 8: Mechanism: Fairness hypothesis

The table displays the results of testing the fairness hypothesis as a potential mechanism explaining the main results of Table 4. It shows the estimates of DDD regressions with the logarithm of CEO total pay as a dependent variable. The sample is main and includes all smaller firms that have positive total pay observations and non-missing lagged control variable observations. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. Time horizon is $t \in [-2; 2]$. The sample is split by CEO age into subsamples of younger CEOs (i.e., belonging to the bottom half of the sample at pre-treatment periods) and older CEOs (i.e., belonging to the top half of the sample at pre-treatment periods). 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.

Smaller firms

	Younger CEOs	Older CEOs
	(1) log(total pay)	(2) log(total pay)
PostXAffectedX%Bound	1.000***	0.392*
	(8.59)	(1.97)
PostXAffected	-1.735***	-1.574*
	(-13.11)	(-1.84)
PostX%Bound	-0.010	-0.104
	(-0.11)	(-1.02)
AffectedX%Bound	0.438	-0.992
	(0.37)	(-0.38)
Post	-0.129	0.574**
	(-1.01)	(2.53)
%Bound	0.206	-0.076
	(0.62)	(-0.18)
Constant	5.222***	3.640***
	(7.52)	(6.49)
Controls	Yes	Yes
Industry-Year FE	Yes	Yes
Firm FE	Yes	Yes
Adj-R-squared	.45	.46
N	2,865	2,555

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

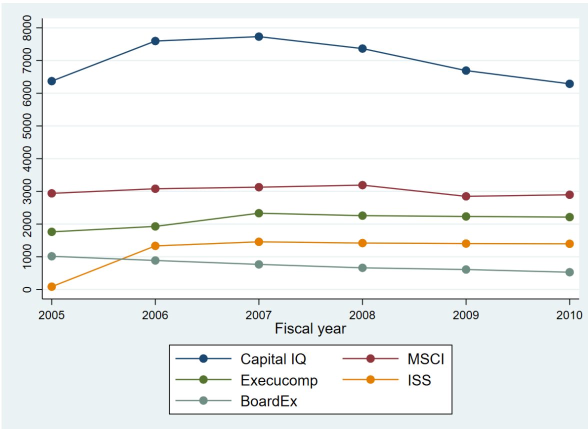
1. I start with defining **total pay** = *totalComp*, **bonus** = *bonus* and **salary** = *salary*.
2. N/A

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

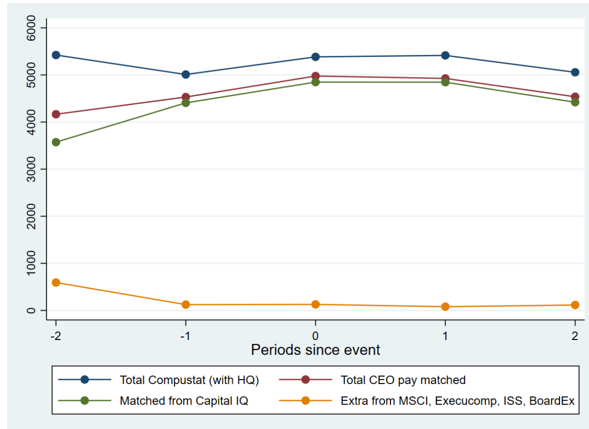
A.1.5. BoardEx

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.

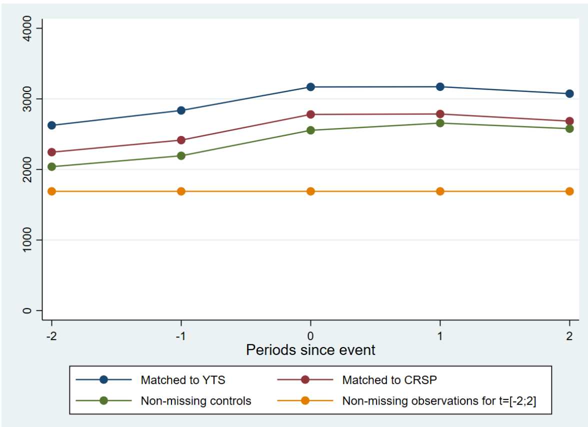
A.2. Tables and figures



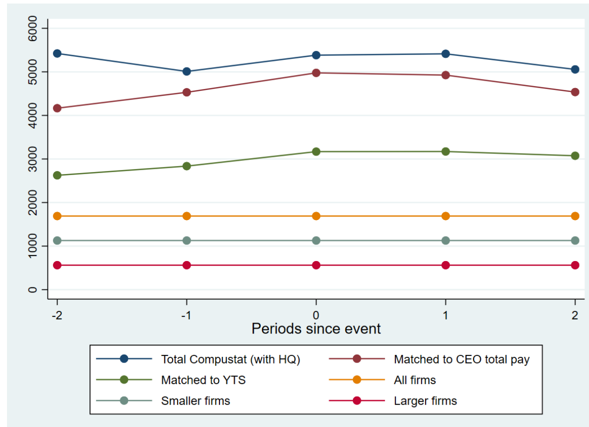
(a) Step 1: CEO pay coverage



(b) Step 2: Match to CEO pay data



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



(d) Summary: Sample

Figure A.1: 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).

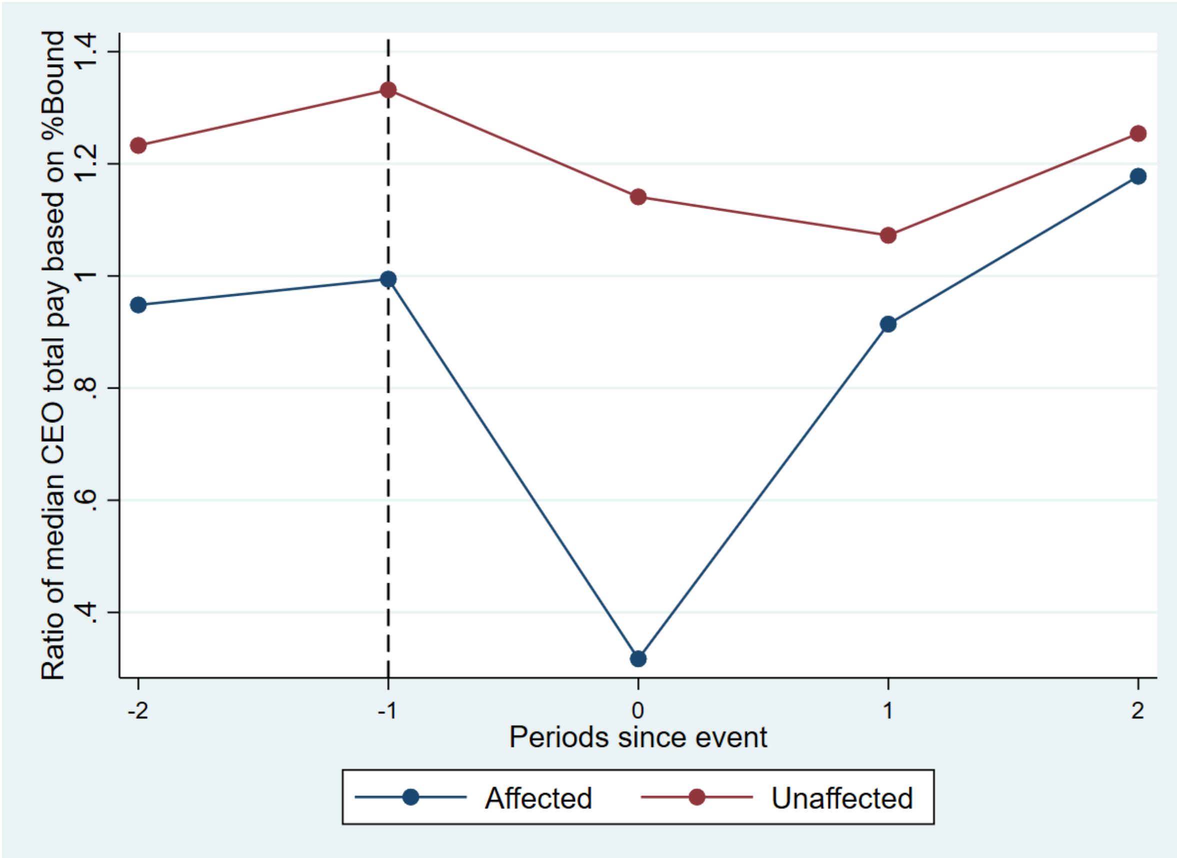


Figure A.2: DDD identification: Parallel pre-trends in smaller firms

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. 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)).

Table A.1: Variable definitions

The table shows definitions of all variables used throughout the analysis and the corresponding data sources.

Variable	**Definition**	**Source**
Total pay, Salary, Bonus	See Appendix	CEO pay datasets
Cash pay	Salary + Bonus	CEO pay datasets
Incentive pay	Total pay - Salary - Bonus	CEO pay datasets
Post	1 if t $\geq$ 0	YTS
Affected	1 if firm belongs to NAICS2 industries 71, 72, 44, 45 (Leisure and Hospitality, Retail Trade)	BLS
%Bound	% of employees located in bound states establishments	YTS
%Δ MW	percentage change in MW	See Table 1
Proxy date after May 25, 2007	1 if a firm filed DEF-14A in 2007 before May 25	SEC Analytics Suite by WRDS
XLR	Total labor expense, *xlr*	Compustat
XOPR	Operating expense, *xopr*	Compustat
SG&A	Selling, general and administrative expense, *xsga*	Compustat
Size	$log(at)$	Compustat
M/B	$(dltt + dlc + csho * \|prcc_f\|)/at$	Compustat
ROA	$ni/at[n-1]$	Compustat
Profitability	$(oibdp/at)$	Compustat
Q	$(at + (prcc_f * csho) - ceq - txdb)/at$	Compustat
log(1+annualized return)	$log(1 +$ annualized $[(1 + ret) * (1 + dlret) - 1])$, with standard returns data cleaning	CRSP
Smaller/Larger firm	1 if firm belongs to the bottom two/top terciles of the sample at $t = -1$ by # employees	
Younger/Older CEO	1 if firm belongs to the bottom/top half of the sample at $t = -1$ by CEO age	
High-rent/Low-rent	1 if firm belongs to the bottom/top half of the sample at $t \in [-2; -1]$ by average industry-adj. ROA or profitability	

Table A.2: The effects of the minimum wage law on firm profitability

The table displays the results of testing the profitability hypothesis as a potential mechanism that would undermine the main results of Table 4. It shows the estimates of DDD regressions with firm profitability as a dependent variable. In column 1, the sample is main and includes all smaller firms that have positive total pay observations and non-missing lagged control variable observations. A firm is smaller if it belongs to the bottom two terciles of the sample by the number of employees at $t = -1$. Time horizon is $t \in [-2; 2]$. In column 2, the sample and time horizon are restricted to the sample of Table 6, Panel B. In columns 3 and 4, the sample and time horizon are the same as in Table 7. In columns 5 and 6, the sample and time horizon are the same as in Table 8. 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, industry, 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.

Smaller firms. Dependent variable: firm profitability

	Baseline	Efficiency wages	Bargaining		Fairness	
	(1) All	(2) All in 2007	(3) High-rent	(4) Low-rent	(5) Younger CEOs	(6) Older CEOs
PostXAffectedX%Bound	-0.026	-0.097	-0.033	0.085**	0.115***	-0.068*
	(-0.55)	(-0.65)	(-1.31)	(2.04)	(4.90)	(-1.98)
PostXAffected	0.034	0.075	0.027	-0.057	-0.016	0.004
	(0.71)	(1.09)	(1.15)	(-0.90)	(-0.71)	(0.07)
PostX%Bound	0.010	0.044**	0.000	0.015	0.019*	0.008
	(1.57)	(2.38)	(0.00)	(1.33)	(1.95)	(0.75)
AffectedX%Bound	0.053	0.093	0.155**	-0.023	0.194**	0.241*
	(0.75)	(0.90)	(2.26)	(-0.44)	(2.21)	(1.72)
Post	-0.015	-0.024***	-0.027*	0.003	-0.020	0.004
	(-1.64)	(-3.78)	(-1.69)	(0.19)	(-0.97)	(0.43)
%Bound	0.022	-0.013	0.028	0.016	0.002	0.059**
	(1.66)	(-0.89)	(1.19)	(0.46)	(0.19)	(2.22)
Constant	0.081	-0.016	0.108*	0.054	0.077	0.052
	(1.31)	(-0.89)	(1.72)	(0.57)	(1.03)	(0.76)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	No	Yes	Yes	Yes	Yes
Industry FE	No	Yes	No	No	No	No
Firm FE	Yes	No	Yes	Yes	Yes	Yes
Adj-R-squared	.74	.78	.56	.7	.76	.71
N	5,639	1,279	2,815	2,820	2,865	2,555

Table A.3: Matched sample identification: CEO pay and firm characteristics at $t = -1$

This table presents the results of a 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 15.35% (sample median at $t = -1$) employees in bound states. Every bound firm is matched to one unbound firm, with an exact match required on industry (presented by NAICS2 codes). Matching covariates include firm size, profitability, ROA, Q, and M/B.

	Unbound	Bound	Diff	t-stat	p-value
Total pay	2241.727	2499.261	-257.534	-.678	.498
Cash pay	859.168	985.694	-126.526	-.731	.465
Salary	471.597	474.868	-3.272	-.107	.915
Bonus	334.124	436.838	-102.714	-.706	.48
Incentive pay	2242.968	2844.381	-601.414	-1.132	.258
Size	7.203	7.446	-.243	-1.662	.097
Profitability	.113	.127	-.013	-1.734	.083
ROA	.054	.061	-.007	-1.133	.258
Q	1.776	1.807	-.031	-.415	.678
M/B	1.774	1.804	-.03	-.405	.685
log(annual return)	.115	.119	-.004	-.154	.877
%Bound	.05	.544	-.494	-28.253	0
Number of distinct firms	367	827	.	.	.

Table A.4: The effects of the minimum wage law on CEO pay: Matched sample and Bordering counties sample

The table displays the estimates of DDD regressions with the logarithm of CEO total pay as a dependent variable. In columns 1 - 3, the sample is matched by observable firm characteristics (see Section 7.2 and Table A.3). In columns 4 - 6, the sample is constructed using the border analysis method (see Section 7.3). 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 $t = -1$. 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. 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.

Smaller firms. Dependent variable: log(CEO total pay)

	Matched sample			Bordering counties sample		
	(1) Smaller firms	(2) Larger firms	(3) All firms	(4) Smaller firms	(5) Larger firms	(6) All firms
PostXAffectedX%Bound	1.222***	0.198	0.995**	0.774*	2.109	0.869
	(3.17)	(0.27)	(2.49)	(1.81)	(1.50)	(1.20)
PostXAffected	-1.571***	0.330	-0.974**	-0.243	2.669**	0.014
	(-3.47)	(0.49)	(-2.11)	(-0.54)	(2.01)	(0.02)
PostX%Bound	-0.091	-0.142	-0.105	-0.140	-0.208	0.012
	(-1.02)	(-0.52)	(-1.21)	(-0.90)	(-0.42)	(0.08)
AffectedX%Bound	1.301	1.381	1.223*	6.244***	1.015	-0.943
	(1.41)	(0.62)	(1.65)	(4.36)	(0.27)	(-0.28)
Post	0.201	-0.076	0.100	0.284	-0.694**	0.016
	(1.00)	(-0.29)	(0.64)	(1.14)	(-2.22)	(0.08)
%Bound	-0.280	-0.132	-0.187	0.043	1.155	0.324
	(-0.76)	(-0.18)	(-0.55)	(0.08)	(1.19)	(0.64)
Constant	5.418***	5.778***	5.510***	3.713***	3.996	3.483***
	(9.22)	(3.35)	(9.38)	(5.23)	(1.30)	(4.29)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State border FE	No	No	No	Yes	Yes	Yes
Adj-R-squared	.57	.5	.59	.53	.44	.58
N	5,515	2,755	8,270	2,680	1,340	4,020