The Positive Effect of Employee Pay on Executive Pay

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Abstract

Top executive pay at U.S. public firms increases by 2.3% when employee pay increases due to the 41% federal minimum wage hike in 2007. Exploiting establishment-level data, my main difference-in-differences design compares firms with larger shares of employees in states bound by the federal minimum wage ("treated") to firms with smaller shares ("control") over time. I find that the effect is more pronounced in minimum-wage-sensitive industries (i.e., leisure and retail) and in firms that are smaller or face a high probability of distress, consistent with executives being compensated for increased distress risk after the hike.

Key words: executive compensation, employee pay, minimum wages, pay inequality
JEL classifications: G34, J31, J38, M52

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1. Introduction

Attitudes towards income inequality could differ drastically across regions. Survey data suggests that, for the past two decades, many Europeans have been more income-inequality averse compared to Americans (as illustrated in Figure 1).[1] Academic studies document the same fact within the corporate world, where income inequality is commonly measured by the within-firm CEO-worker pay gap. For example, Kiatpongsan and Norton (2014) show that in European countries, the actual CEO-worker pay gap is closer to the publicly perceived "ideal" gap than in the United States. Consistent with these patterns, Dittmann, Schneider, and Zhu (2020) establish a link between European rank-and-file worker and CEO compensation levels and find that in Germany, workers demand a pay increase due to relative wealth concerns when the information about CEO pay is disclosed to them.[2]

In this paper, I investigate whether the link between the highest-paid (referred to as "top" hereafter) executive and worker pay exists in the United States. Inequality aversion in the United States is lower and, hence, public opinions do not constrain relative executive pay as much. Understanding the mechanism behind this link is important because the executive-employee 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 talent of CEOs (Gabaix and Landier (2008)), and strategic choice of peer group firms (Faulkender and Yang (2010)) among the plausible explanations of the high CEO-worker pay gap (evolution of the pay gap over time is illustrated in Figure 2). Despite no consensus among academics, regulators and the general public tend to believe that executives 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 executive 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) increases (e.g., Tonti (2022)).

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

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

[2] A CEO is typically the highest-paid executive within firms (Bloom, Ohlmacher, Tello-Trillo, and Wallskog (2021)).

[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 ratios exceeding 50. The text of the Act is available here: https://www.congress.gov/bill/117th-congress/senate-bill/794/.

executive 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 firm-level compensation. Thus, a MW hike provides a plausible shock to establish the economic link between employee and executive pay.

I use the introduction of a U.S. federal MW law signed on May 25, 2007 as a natural experiment. The Fair Minimum Wage Act of 2007 raised the federal MW from \$5.15 to \$5.85, \$6.55, and \$7.25 per hour each year from July 2007 through July 2009, representing a total of 41% increase in the federal MW. This shock has been used in the literature to analyze the effects of the MW policies on firm-level outcomes such as employment (Clemens and Wither (2019)) and investment (Gustafson and Kotter (2023)). In comparison, the first contribution of this paper is using the Fair Minimum Wage Act of 2007 to provide novel evidence that employee pay has spillover effects on top executive pay.

To establish the link between employee wages and top executive compensation, I use several empirical strategies. I start with a difference-in-differences (DD) approach that uses firms' headquarters location, following Gustafson and Kotter (2023). Since an employee has the legal right to receive the larger of state and federal MWs, the law impacts only the wages of employees in states where the initial state MW was lower than or equal to the federal MW (referred to as "bound states"). Therefore, the DD strategy compares changes in outcome variables in two dimensions: across firms headquartered in bound states (first difference) and before vs. after the MW hike (second difference). Using this specification, I show that total executive pay does not change significantly after the MW law – which is not surprising given the well-established finding that MW labor affected by the law is concentrated in the leisure, hospitality and retail trade industries (e.g., Gustafson and Kotter (2023), Dai and Qiu (2023), Chava, Oettl, and Singh (2023)). Hence, following Gustafson and Kotter (2023), I next add the third difference dimension of "affected" firms based on industries which are defined pre-treatment and fixed throughout the event window. Using the triple-differences (DDD), I establish a positive and statistically significant effect of the MW hike on total executive pay. The estimates suggest that post-MW law, compensation levels increased by 71.8% for top executives of affected vs. unaffected firms in bound vs. unbound states.

Having established these "baseline" results, I build upon the baseline DD methodology by changing the focus of the second difference to the distribution of *workers* instead of *headquarters* across states. To do so, for each firm-year, I construct the proportion of workers located in bound states ("%Bound" variable) using the establishment-level Your-economy Time Series (YTS) data. Thus, this "main" DD approach compares changes in executive pay across firms

with different shares of workers located in bound states (first difference) and before vs. after the MW hike (second difference).

Developing the main specification instead of using the baseline is the second contribution of this paper – it allows the firms' exposure to the MW shock to be continuous and hence more precisely measured. I interpret the "%Bound" variable as treatment intensity: the larger the share of workers in bound states, the more significant the effect of the MW change on the firm, ceteris paribus. Despite this methodological benefit, one concern is that the "%Bound" variable might change after the MW hike.[5] However, I show that the change in the "%Bound" coefficient estimate is not significant after the MW law. With this argument in mind and to take advantage of the improved precision of the estimates, I proceed using the main specification for the rest of the paper.

I establish a series of results for the effects of the MW law on top executive pay using the main specification. Similarly to the baseline, while I find no effect in the DD specification, perhaps as expected, I find a strong and positive effect in the DDD specification that adds the third "affected" industry dimension. I find that a 10-percentage point increase in the share of employees in bound states is followed by a 6.7% increase in top executive total pay for affected vs. unaffected firms post-MW law. This total pay increase is driven by both salary and incentive pay raises. The result is consistent with a common, permanent nature of salary changes (Murphy (2013)) and MW shocks. It is also consistent with incentive pay raises being awarded based on fairness considerations (Edmans, Gosling, and Jenter (2023)), aiming to directionally align executive pay changes to the employee pay increases driven by the MW shock.

I further examine other important presumptions of my analysis. I show that there are no differential trends in top executive pay for the periods pre-MW raise, suggesting that the parallel trends assumption holds, which is critical for the DDD validity. Moreover, the existing literature shows that MW laws raise worker wages, and the effect is most pronounced for smaller firms (Wursten and Reich (2023)) due to their lower wage levels and lower product market pricing power (Cooper, Luengo-Prado, and Parker (2020)). Following these results, my analysis focuses on the firms that are in the bottom two terciles of the distribution by the number of employees (referred to as "smaller" firms throughout the paper). Crucially, the smaller firms are publicly traded U.S. firms, with a median firm having $607.7 MM in total assets and employing 540 workers.

[5]The literature on worker migration around the MW shifts is nascent. See Minton and Wheaton (2022) for an overview of the literature. They find no significant evidence for a cumulative change in the net migration rate of near-minimum-wage workers over the three post-treatment years using the Fair Minimum Wage Act of 2007.

The third contribution of this paper is that it considers several competing explanations of the established top executive pay increase. The first mechanism posits that top executives receive compensation premia for *bearing increased risks* triggered by the MW hike. Indeed, it has been shown that MW raises result in a higher probability of bankruptcy and a greater risk of industry exit (Luca and Luca (2019), Aaronson, French, Sorkin, and To (2018), Alexandre, Bação, Cerejeira, Costa, and Portela (2022), Acar, Bossavie, and Makovec (2019), Chava et al. (2023)). Consistent with this mechanism, I show that top executives of firms more exposed to the bankruptcy risk ex ante – such as financially distressed (as measured by Altman (1968) Z-score and Ohlson (1980) O-score) and very small public firms – experience larger pay increases after the MW hike compared to their counterparts. Hence, my result adds to the literature linking distress risks to compensating wage differentials in competitive labor markets (e.g., Graham, Kim, Li, and Qiu (2023), Peters and Wagner (2014), Chang, Hayes, and Hillegeist (2016)).

Alternatively, the second mechanism predicts that top executives may enjoy positive spillovers from worker wage raises due to their strong *ability to bargain* (Murphy and Zabojnik (2004), Murphy and Zabojnik (2007), Frydman (2019)). Nash bargaining models posit that firm rents and the strength of executive bargaining power are crucial drivers of bargaining outcomes (e.g., Mortensen (2003)). Following the literature, I use profit per employee as a proxy for rents (e.g., Blanchflower, Oswald, and Sanfey (1996)) and the share of institutional ownership and board independence as proxies for external pressure from either shareholders (e.g., Hartzell and Starks (2003)) or the board (e.g., Hermalin and Weisbach (1998)), respectively. I show that there is no significant effect on either determinant of top executive bargaining outcomes after the MW shock – inconsistent with the second conjecture.

The third type of mechanisms operates through the effects of MW on *the rank-and-file worker labor market*. On the one hand, competitive labor market models predict a rise in prices and costs as a result of the MW raise (e.g., Neumark and Wascher (2008), Draca, Machin, and Van Reenen (2011), Aaronson et al. (2018), Harasztosi and Lindner (2019), Aaronson (2001)). Correspondingly, top executives may demand a compensation raise to adjust to a higher cost of living. However, I show that top executive pay raises are pronounced not only in nominal but also in real terms, which is inconsistent with this scenario. On the other hand, wage raises may result in improved productivity, leading to stronger firm performance through the efficiency wages channel (Yellen (1984), Akerlof and Yellen (1986)), allowing top executives to reap larger compensation benefits. In contrast to this scenario, my results show no evidence of firm performance boost after the MW law as measured by industry-adjusted ROA and annualized stock returns. Moreover, to enhance the power of my test, I develop an

alternative triple-differences identification strategy based on the variation in firm-level executive compensation setting dates relative to the MW law announcement date (May 25). I show that firms disclosing executive pay for fiscal year 2007 before May 25, 2007 award significantly larger compensation levels for fiscal year 2008 compared to 2007. This result is also inconsistent with the efficiency wages mechanism since firms are unlikely to be able to adjust productivity during this short time period. Taken together, my findings support neither the cost-of-living adjustment mechanism nor the efficiency wages mechanism as a driver of the worker-top executive pay link.

Last, the fourth group of mechanisms operates through the negative effects of MW raise on *the executive-employee pay gap*. One consequence of an increase in MW workers' wages may be that, according to the tournament model (Lazear and Rosen (1981)), other managers and top executives (including CEOs) also need to be paid more to maintain tournament incentives. Inconsistent with this explanation, I show that top executives of firms with lower top executive-employee pay ratios and, hence, lower tournament incentives before the MW shock receive larger pay raises compared to their counterparts.

The two other potential sub-mechanisms are based on the change in the level of pushback (due to a high executive-employee pay ratio) that top executives may receive when negotiating a pay raise after the MW shock. On the one hand, if shareholders become increasingly opposed to the executive-employee pay ratio as it grows (Pan et al. (2022), Chang, Dambra, Schonberger, and Suk (2023)), a reduction in the ratio coming from the MW raise would allow the top executives to get a pay raise easier. However, I show that pay raises are more pronounced for top executives of firms with a smaller number of blockholders, which proxies for weak blockholder influence. On the other hand, a wage raise for rank-and-file workers triggered by the MW increase should compensate for the perceived unfairness of the large executive-employee pay gap from the employee perspective, which is a concern for labor unions (e.g., Huang, Jiang, Lie, and Que (2017)). However, I show that top executives of firms in unionized industries receive smaller pay raises, contradicting the idea that a MW raise boosts employee morale through fairness concerns, simplifying a pay raise negotiation for top executives.

To summarize, my results are most consistent with the risk premium mechanism and inconsistent with the mechanisms operating through bargaining, cost-of-living adjustment, efficiency wages, tournament incentives, shareholder constraints, and employee morale. Also, important from a policy perspective, I show that the top executive pay-MW semi-elasticity is 0.057. This translates into the 41% hike in federal MW leading to a 2.3% relative increase in top executive pay.

My results survive a set of robustness checks, including using different samples – a matched

sample based on observable firm characteristics (firm size, ROA, profitability, Tobin's Q, and market-to-book) 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)). Additionally, excluding the 2008 - 2009 financial crisis years and controlling for state political ideology do not change the results in a significant way. Furthermore, I show that all the control variables (including profitability), as well as other observables such as leverage, capital expenditures, and R&D expenditures are not affected by the MW raise, while employment drops, consistent with competitive labor market models. These analyses alleviate the concerns that my results may suffer from the endogeneity resulting from firm observable characteristics, different economic conditions across regions, or several other omitted variables driving the results.

This paper contributes to the growing literature on the executive-employee pay disparity. Other work connecting executive (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. A 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. This paper provides novel evidence of positive spillover effects of employee pay on top executive pay using a large MW shock in the United States.

Second, this paper is related 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)). Relative to these papers, I document a positive impact of MW on *top executives'* pay.

2. Background

2.1. History of the minimum wage policy in the United States

As documented by the U.S. Department of Labor, any employee has the right to receive a maximum of state and federal MW.[6] 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 (Clemens and Wither (2019), Gustafson and Kotter (2023), Chava et al. (2023)), I call these states "bound".

Before the mid-1980s, the majority of the states were bound. Hence, federal MW laws enacted

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

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 (Gustafson and Kotter (2023)). 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. These laws raised the federal MW level in total by 26.9%, 21.2%, and 40.8%, respectively. In addition, many academics have emphasized the importance of using large-magnitude shocks for establishing causality (e.g., Card (2021)).[7] Hence, the federal MW law of May 25, 2007, which affects the federal MW level the most out of the three laws, is an appropriate choice to use as a natural experiment for estimating the spillover effects of employee pay on top executive 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.[8] The law consists 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 A.2. Figure 3 presents the distribution of states classified as bound or unbound pre-introduction of the law.[9]

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)

[7]Related, Harasztosi and Lindner (2019), Aaronson et al. (2018), and Sorkin (2015) highlight challenges coming from using small MW shocks for identification and estimation of their effects.

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

[9]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 (2023) for a more detailed discussion).

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 (2023), Chava et al. (2023), and similar to Gustafson and Kotter (2023).[10] 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 pronounced in the subsample of affected firms.

Importantly, the Current Population Survey data reflect employee wages without overtime pay, tips, or commissions.[11] 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?

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., Neumark et al. (2004), Lee (1999), Freeman (1996), 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 suggest state MW hikes leading to up to $2.50 wage raise for firms with a high share of MW labor (Gopalan et al. (2021)).

Importantly, MW hikes also have a differential impact on firms depending on their size. In particular, 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. They name lower wage levels and lower product

[10] Dai and Qiu (2023) and Chava et al. (2023) use Accommodation and Food Services and Retail Trade industries (NAICS2 codes 72 and 44-45). Gustafson and Kotter (2023) use the Entertainment, Retail, and Restaurants, Hotels, and Motels industries (FF48 codes 7, 42, and 43).

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

market pricing power (Cooper et al. (2020)) among the reasons driving the persistence of the MW raise effect in smaller firms. Similarly, Del Carpio, Nguyen, and Wang (2012) show that Indonesian smaller firms are more responsive to minimum wage changes, and for them, average wage is more correlated with minimum wage changes.[12]

Based on this evidence, the assumption that the MW hike is a significant shock to worker wages is plausibly satisfied for the smaller firms in my sample. Therefore, for the rest of the paper, I focus on smaller firms to estimate the link between worker wages and top executive pay. However, when interpreting the results of my analysis, one should keep in mind that the smaller firms are de facto a significant and representative sample of U.S. publicly traded firms.

Finally, focusing on smaller firms provides an extra identification benefit - particularly in the case of the baseline specification exploiting the distribution of firm headquarters across states. Gustafson and Kotter (2023) note that smaller firms are more likely to have the majority of workers located in the state of the firm's headquarters, reducing the potential measurement error in defining bound states. I discuss the research methodology of this study in detail in Section 4.1.

3. Data and sample construction

3.1. Data sources

This study combines multiple data sources. The primary executive 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) and Execucomp. To my knowledge, this is the most extensive combination of publicly available executive compensation datasets.[13] Establishment-level employment and state location come from the Your-economy Time Series (YTS, hereafter) dataset. The YTS

[12]Intuitively, top executives 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 top executives are completely ignorant of how much workers of their firms are paid. An illustrative example is Elon Musk's claim of being unaware of 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 top executives are engaged in the MW policy considerations to the point of giving interviews to CNN (Elassar (2021)).

[13]The two other available datasets are the Institutional Shareholder Services Incentive Lab (ISS, hereafter) and BoardEx - however, due to their lower coverage, they increase my sample by only seven firms. At the same time, using these two datasets would result in increased data heterogeneity due to differential measuring standards, so I decide to forgo them. The results are robust to including these seven firms in the sample.

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 characteristics, including 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. Blockholder holdings data are from Schwartz-Ziv and Volkova (2024). Industry unionization data come from the Union Membership and Coverage Database (Hirsch and Macpherson (2003), Hirsch, Macpherson, and Even (2024)). Table A.1 presents the definitions of the variables.

3.2. Sample construction

I start with the sample of all publicly traded corporations headquartered in the United States 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 (2023).[14] For example, period t includes all top executive 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 executive compensation after the MW shock is considered as such.

I further merge the Compustat sample to the executive compensation datasets. I define the highest-paid executive in the fiscal year per firm to be the top executive (often the CEO, as documented by Bloom et al. (2021)).[15] I start matching with Capital IQ by GVKEY and continue the matching procedure for unmatched observations with MSCI by ticker and Execucomp by GVKEY. This matching order is defined by the relative firm coverage presented in Figure IA.1a. I require all observations to have positive top executive total pay.[16] The distribution of the matched number of firms per period is given in Figure IA.1b. Table IA.1, Panel A further shows that over 94% of top executive pay observations come from Capital IQ.[17]

[14]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.

[15]The CEO indicator is sparsely populated for Capital IQ, which is the main source of executive compensation data in my study (see Table IA.1, Panel A). In particular, there are fewer than 2,000 CEOs out of over 2.4 million individuals covered by Capital IQ. Therefore, I focus on the highest-paid executives as a proxy for top management.

[16]For a detailed description of the construction of top executive pay datasets, see Section IA.1 in the Appendix.

[17]Additionally, to account for potential measurement differences across Capital IQ, MSCI, and Execucomp (see Table IA.1, Panel B), I include dataset fixed effects into the regression analysis discussed in Section 4.1.

Next, following Flynn and Ghent (2024), I match the Compustat-top executive 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 the 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 IA.1c presents the number of matched firms per period to YTS, CRSP, and post-additional filters. The resulting "all firms" sample consists of 1,683 firms per period with available top executive pay data, employment levels, and non-missing control variables for the five-period time horizon of $t \in [-2; 2]$, covering dates from July 24, 2005, to July 24, 2010, as shown in Figure IA.1d.

However, as discussed in Section 2.3, it is reasonable to expect ex-ante that the relationship between worker pay and top executive pay is pronounced most strongly in *smaller* firms. To define firm size, I split the main sample into terciles by the number of employees. Intuitively, 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 "main" sample of focus.[18] Additionally, in a robustness analysis, I consider a matched sample and a sample of firms headquartered in counties on the borders of contiguous states.

Table 1 presents summary statistics for firm characteristics and top executive pay variables for the main sample of U.S. public firms. A median firm in the sample has 540 employees.[19]

[18]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.

[19]To compare, the Small Business Administration 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.

During the sample period, 5.1% of workers are located in bound states, and a median firm has $607.7MM in total assets, an ROA of 0.016, a market-to-book ratio of 1.24, and pays the top executive $437.9K per year.

4. Methodology and Hypotheses

4.1. Methodology: Baseline specifications

To estimate the link between employee pay and top executive pay, I start by following two baseline specifications of Gustafson and Kotter (2023). The baseline DD specification allows to analyze changes in top executive pay from two angles: the first difference compares outcomes after vis-a-vis before the effective date of the law, while the second difference compares outcomes for firms in bound states vis-a-vis unbound states. In particular, the baseline DD model is given by Equation 1a:

$$\begin{aligned}\log(\text{Top executive pay}_{i,j,t}) = \beta_0(\text{Post}_t \times \text{Bound}_i) + \beta_1 \text{Post}_t + \beta_2 \text{Bound}_i \\ + \Gamma \text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \eta_d + \epsilon_{i,j,t},\end{aligned} \tag{1a}$$

where top executive pay is a top executive pay component (total pay, 1 + salary, 1 + bonus, 1 + cash pay, or 1 + incentive pay)[20] in firm i of industry j at period t; Post is a dummy variable equal to 1 for periods $t = 0, 1, 2$; Bound is a dummy variable equal to 1 for firms located in bound states at pre-treatment period $t = -2$; X is a vector of firm characteristics; δ are industry by year fixed effects (Gormley and Matsa (2014)); and γ are firm fixed effects. Additionally, η_d are the top executive compensation dataset fixed effects and account for potential variable measurement heterogeneity.

To extend the baseline DD model, the baseline DDD specification adds the industry-level interaction component ("Affected" variable) and is given by Equation 1b:

[20]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 executive compensation datasets, I assign the difference between total pay and cash pay to incentive pay. Section IA.1 discusses pay variables construction and definitions in detail. One is added to the top executive total pay component values to account for meaningful zero values.

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

where Affected is a dummy variable equal to 1 if a firm belongs to a NAICS2 71, 72, 44, or 45 industry; and all the other variables are the same as in Equation 1a.

The key advantage of the DDD specification is that it allows for estimating and comparing the effect of a MW increase shock on top executive pay in two dimensions: location and industry. This effect is represented by the coefficient β_0. More precisely, the interpretation of β_0 is the following: after the MW law, top executives receive $\beta_0 \times 100\%$ extra compensation at affected vs. unaffected firms located in bound vis-a-vis unbound states. A commonly acknowledged limitation of the DDD model is a potential link between the third-difference factor and the outcome variable. When interpreting the estimates of Equation 1b, one should keep in mind that the measure of exposure to MW labor ("Affected" variable) may be correlated with the top executive pay. Despite the "Affected" variable being defined at the pre-treatment period (at $t = -2$), this endogeneity concern may make the causal interpretation of the β_0 coefficient challenging. However, due to limited theoretical evidence on the differences in top executive pay setting patterns across affected and unaffected firms after MW shocks, investigating the existence of such a possible bias and its associated effects is left outside the scope of this paper.

4.2. Methodology: Main specifications

I further build upon the baseline specifications and improve the precision of the location component by replacing the discrete "Bound" variable with the continuous "%Bound" variable representing the firm-headquarter-level annual 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, this new second difference (%Bound) represents a continuous assignment to treatment at the firm-period level: a larger employment share in bound states represents a larger treatment dose. The main DD and DDD specifications are hence given by Equations 2a and 2b, respectively:

$$\begin{aligned}\log(\text{Top executive pay}_{i,j,t}) = \beta_0(\text{Post}_t \times \%\text{Bound}_{i,t}) + \beta_1\text{Post}_t + \beta_2\%\text{Bound}_{i,t} \\ + \Gamma\text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \eta_d + \epsilon_{i,j,t},\end{aligned} \tag{2a}$$

where %Bound is the share of firm workers employed at establishments located in bound states; and all the other variables are the same as in Equation 1a; and

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

where the variables are the same as in Equations 1b and 2a.

Similarly to the baseline DDD specification, in the main DDD specification, the coefficient of interest is β_0. The interpretation of β_0 is as follows: a ten percentage point increase in the share of employees located in bound states results in $10 \times \beta_0\%$ extra compensation post-MW law for top executives of affected vis-a-vis unaffected firms. Besides, similarly to the baseline case, one should be cautious when interpreting the main DDD specification estimates in terms of causality vs. correlation.

A key assumption of both DD and DDD main specifications is the exogeneity of the %Bound variable with respect to the MW law passage. If workers migrate across states over time after the MW shock, one can argue that main specifications may suffer from a bad controls problem. The existing literature on the patterns of worker migration due to MW changes is limited – Minton and Wheaton (2022) provide an overview and show no evidence of a significant effect of the Fair Minimum Wage Act of 2007 on the cumulative net migration of near-minimum-wage workers over the three post-treatment periods. Thus, to address this concern, after establishing the main results, I show that, in a regression of $\%\text{Bound}_{i,t} = \beta_0\text{Post}_t + \Gamma\text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \eta_d + \epsilon_{i,j,t}$, the β_0 coefficient is statistically insignificant with an economically negligible magnitude. This result supports the accuracy of the main DD and DDD specifications through the statistically and economically insignificant changes in the %Bound variable after the MW shock.

Additionally and importantly, the main DDD identification strategy of Equation 2b is valid if the parallel trends assumption for total top executive pay with respect to the MW shock effect is satisfied. To test this assumption, I estimate the temporal pattern in total pay for affected

firms compared to the patterns for unaffected firms with the same share of workers in bound states (following, e.g., Babenko, Bennett, and Sen (2021)). Figure A.1 illustrates that during the pre-treatment period ($t = -2$ and $t = -1$), there is no apparent trend for the total top executive pay. However, there is an increase in pay when the MW law becomes effective ($t = 1$ onwards). This evidence further supports the credibility of my results.

Due to the advantages discussed above and having shown evidence in support of the crucial assumptions, after establishing the baseline results, I use the main DDD specification of Equation 2b for the rest of the paper. In the next section, I develop the hypotheses connecting employee and top executive pay and discuss how the estimate of β_0 allows me to make conclusions about which hypothesis my results are most consistent with.

4.3. Hypotheses and mechanisms

Intuitively, top executive 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 the testing of each of these mechanisms.

To begin with, a positive relationship between worker pay and top executive pay is summarized in Hypothesis 1. If Hypothesis 1 is true, it will be reflected in a positive and statistically significant estimate of β_0. This result is consistent with several potential mechanisms, which I split into four groups.

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

4.3.1. Risk premium mechanism

The first mechanism proposes that a MW hike triggers an increase in distress risks, leading top executives to demand compensation raises. Prior studies have shown that MW hikes result in an increased probability of bankruptcy and a greater likelihood of firms exiting the industry (Luca and Luca (2019), Aaronson et al. (2018), Alexandre et al. (2022), Acar et al. (2019)). Moreover, this scenario is consistent with the literature documenting compensating wage premia for distress risks for employees in general (Graham et al. (2023)) and executives in particular (Chang et al. (2016), Peters and Wagner (2014)).

If this conjecture is true, top executives of more vulnerable firms ex ante will receive larger pay increases following the MW law. Empirically, I define vulnerable firms as financially distressed

and very small firms. While justifying the first category is straightforward, the second category is supported by prior evidence that MW hikes exacerbate exit rates among smaller firms (Chava et al. (2023)). Following the literature, I use Z-score (Altman (1968)) and O-score (Ohlson (1980)) as proxies for financial distress risk.

4.3.2. Bargaining mechanism

The second mechanism is based on the bargaining hypothesis (Murphy and Zabojnik (2004), Murphy and Zabojnik (2007), Frydman (2019)). It predicts that strong bargaining ability resulting from increased competition in the executive labor markets drives executives' ability to extract more rents from the firm. In turn, this rent extraction is reflected in larger executive compensation.

Standard Nash bargaining models represent wages to be determined as a function of firm rents and bargaining power (e.g., Mortensen (2003)). Therefore, if the *bargaining* scenario is true, the MW shock will be followed by an increase in either rents or the top executive's bargaining power, allowing executives to enjoy a larger compensation package.

Empirically, bargaining models often use profits per worker as a measure of rents (e.g., Blanchflower et al. (1996)), while executive bargaining power is commonly proxied by levels of external pressure (or monitoring) that the executive is exposed to. The extent of this pressure can be measured by the level of institutional ownership (e.g., Hartzell and Starks (2003)) or by the degree of board independence (e.g., Hermalin and Weisbach (1998)). Hence, an increase in the profits per worker, and/or a reduction in the share of institutional ownership, and/or a reduction in board independence after the MW shock will be consistent with the bargaining mechanism.

4.3.3. Labor mechanisms: cost-of-living adjustment vs. efficiency wages

The third group of mechanisms is based on the labor economics effects of the MW raise. It has been well-established in the MW literature that a MW raise represents a positive shock to aggregate demand, which affects multiple firm-level and economic outcomes. Competitive labor market models predict that an aggregate demand shock will pass through to lower employment due to increased costs and to higher prices (e.g., Neumark and Wascher (2008), Draca et al. (2011), Aaronson et al. (2018), Harasztosi and Lindner (2019), Aaronson (2001)). As a result, top executives may be getting a pay raise after the MW shock to accommodate this adjustment in the cost of living. If this *cost-of-living adjustment* scenario is true, then the results should

disappear after bringing top executive pay to real terms.

Alternatively, according to the efficiency wages model (Yellen (1984), Akerlof and Yellen (1986)), improved productivity through larger employment and larger wages will translate into better firm performance, allowing the top executives to enjoy higher compensation. Therefore, if the *efficiency wages* scenario 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 mechanism using an alternative identification strategy based on the variation in firm-level executive compensation setting dates relative to the MW law announcement date. Plausibly, wages do not affect productivity in such a short time frame of several months (Oi and Idson (1999)). Hence, if the *efficiency wages* mechanism is true, I expect to find no effect on top executive pay in this alternative setting.

Since the cost-of-living adjustment scenario and efficiency wages scenario have opposing predictions on employment level after the MW shock, it is easy to differentiate between them.

4.3.4. Executive-employee pay ratio mechanisms: tournament incentives vs. shareholder constraints vs. employee morale

The fourth group of mechanisms is driven by the executive-employee pay ratio effects of the MW hike. On the one hand, the tournament incentives model predicts that wider pay gaps incentivize workers to input more effort to achieve promotion (Lazear and Rosen (1981)). Therefore, if employees get an exogenous wage raise through a MW shock, other higher-ranked non-executive and executive employees should get a pay raise to keep within-firm competition unchanged. Hence, if the *tournament incentives* scenario is true, top executives of firms with higher top executive-employee pay ratios pre-shock should receive larger compensation raises since tournament incentives are strongest in such firms.

On the other hand, it is well-established that shareholders care about executive-employee pay ratios and often disapprove of too high ratios through equity markets (Pan et al. (2022)) or say-on-pay votes (Chang et al. (2023)). The board often wants to avoid controversy with the shareholders (Edmans et al. (2023)), and therefore, the board may tolerate a high pay ratio only to some threshold. As workers receive a wage raise through the MW hike, the denominator of the executive-employee pay ratio increases (e.g., Harasztosi and Lindner (2019), Boeri (2012)), and the ratio itself drops. In other words, the constraint on the highest tolerable top executive pay level gets relaxed, and top executives are able to demand a pay raise without receiving pushback from the shareholders (and hence the board). Therefore, if the *shareholder constraints* scenario is true, top executives of firms with higher top executive-employee pay ratios pre-shock

should receive larger pay raises. Moreover, the results should be more pronounced for firms with a larger percentage of blockholder holdings (strongest shareholder influence).

Furthermore, the pushback to high executive compensation levels may come not only from the shareholders but also from labor unions (e.g., Huang et al. (2017)). If employees believe that it is unfair that the top executive is getting paid much more than them, it may motivate them to join a union. However, if employees receive an exogenous pay raise, they become less unhappy about the large top executive-worker pay differential, and the top executive may be able to negotiate a pay raise. Therefore, if the *employee morale* scenario is true, top executives of firms in unionized industries should receive larger pay increases after the MW shock.

As a result, the tournament incentives mechanism, shareholder constraints mechanism, and employee morale mechanism are consistent with the scenario that a MW hike affects the top executive-worker pay ratio negatively, which in turn allows top executives to negotiate a pay raise.

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

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

A mechanism consistent with Hypothesis 2 is based on the idea 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 may reduce firm profit and, hence, the share that could be awarded to the top executive. Consistent with this hypothesis, existing research has shown a drop in firm profitability and revenues following MW hikes (e.g., Agarwal, Ayyagari, and Kosová (2024), Draca et al. (2011), Harasztosi and Lindner (2019)). Therefore, if the *profitability* mechanism is true, I expect to see a drop in firm profitability after the MW shock.

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

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

A mechanism consistent with Hypothesis 3 is based on the labor market segmentation theory (Reich, Gordon, and Edwards (1973)). Since top executives and rank-and-file workers belong

to different employee groups (executives vs. non-executives), they may plausibly operate in different labor markets. Therefore, if the *labor market segmentation* mechanism is true, I expect to find that a MW shock raising worker pay will not affect top executive pay in a meaningful way.

In this paper, I test Hypotheses 1, 2, and 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 wages scenario, profitability for profitability scenario, 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 top executive pay react to changes in worker pay?

5.1. Establishing the effect of the minimum wage law on top executive pay

I now turn to the central question of interest of this paper: how does top executive pay change as a result of worker wage raise triggered by the MW law? To answer this question, I estimate the baseline and main DD and DDD specifications and investigate the values and magnitudes of the coefficient β_0. My prior is that the effect will be pronounced only in the DDD specification since the DD specification is dominated by industries with minimal exposure to MW labor, as discussed in Section 2.2.

Starting with the baseline specification, I estimate Equations 1a (DD) and 1b (DDD) and present the results in Table 2. Column (1) shows that within the DD specification, the estimate of β_0 is statistically and economically insignificant, with the coefficient value equal to -0.074. Further confirming the expected patterns, column (2) shows that within the triple-differences model, the value of β_0 is positive and statistically significant. The economic magnitude of this estimate suggests that total top executive pay increases by 71.8% for affected vs. unaffected firms in bound vs. unbound states following a positive MW shock. Moreover, the total pay increase is transmitted through cash pay (column (3)) and incentive pay (column (6)) compensation components. The corresponding estimates of β_0 are also positive and statistically significant. These estimates can be translated into economic magnitudes of a 43% (248.8%) increase in cash pay (incentive pay) for affected vs. unaffected firms in bound vs. unbound states after the MW hike.

I next continue with the main specification, estimating 2a (DD) and 2b (DDD). Table 3 summarizes the results, which are similar to the baseline: there is no effect within the difference-in-differences specification (column (1)), but a strong, positive, statistically significant effect is pronounced within the DDD model (column (2)). The economic magnitude of β_0 is such that a 10-percentage point increase in the share of employees located in bound states results in a 6.73% increase in total top executive pay for affected vs. unaffected firms after the MW hike. Notably, these estimates are quantitatively similar to the estimates of Table 2. Additionally, the total pay increase is driven by the salary (column (4)) and incentive pay (column (6)) increases. This result is also in line with the baseline findings. The corresponding estimates of β_0 can be interpreted as reflecting 6.41% (49.92%) increases in salary (incentive pay) for affected vs. unaffected firms post-MW hike after a 10-percentage point increase in the employment share in bound states.

The channel of transmission of total top executive pay raises through salary, which is part of cash pay, 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 top executive pay raises, it makes sense to expect the "persistence" characteristic to be spilled over as well.

Additionally, the channel of transmission of top executive total pay raises through incentive pay is consistent with the findings of Edmans et al. (2023). Using a comprehensive survey of FTSE All-Share directors and institutional investors in UK equities, they show that incentive pay awards play an important role in reflecting fairness considerations with respect to top executive pay-setting practices. One such reflection is the alignment of the positive direction of the change in incentive pay for top executives with the positive direction of the change in worker pay triggered by the MW hike.

Finally, column (7) of Table 3 addresses the concern that %Bound variable may change over time, potentially giving rise to endogeneity issues. In a regression with %Bound on the left-hand side, the timing coefficient estimate ("Post") is statistically insignificant and economically around zero. This evidence alleviates the concern of %Bound variable being a part of the bad controls problem and supports the main specification, as discussed in Section 4.2.

5.2. Quantifying the semi-elasticity of top executive pay with respect to minimum wages

Having presented evidence in favor of Hypothesis 1 in the previous section, I next turn to quantifying the positive total top executive 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 total top executive pay in affected vs. unaffected firms. The answer to this question 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 top executive 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 A.2). 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 top executive 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 median firm's top executive pay, which will be equal to a dollar amount of $\beta_0 \times 41\% \times$ median firm's top executive pay.

To do that, I estimate a triple-differences model (e.g., Gustafson and Kotter (2023), Chava et al. (2023)) given by Equation 3:

$$\begin{aligned}\log(\text{Top executive pay}_{i,j,t}) = {} & \beta_0(\%\Delta\text{MW}_t \times \%\text{Bound}_{i,t} \times \text{Affected}_j) \\ & + \beta_1(\%\Delta\text{MW}_t \times \%\text{Bound}_{i,t}) + \beta_2(\%\Delta\text{MW}_t \times \text{Affected}_j) \\ & + \beta_3(\%\text{Bound}_{i,t} \times \text{Affected}_j) + \beta_4\%\Delta\text{MW}_t + \beta_5\%\text{Bound}_{i,t} \\ & + \beta_6\text{Affected}_j + \Gamma\text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \epsilon_{i,j,t},\end{aligned} \tag{3}$$

where all the variables except %ΔMW are the same as in the main DDD specification of Equation 2b. The key difference between the two methodologies is that, in this specification, %ΔMW is a continuous variable equal to the percentage increase in MW for the relevant time period. For example, %ΔMW is equal to 13.6 for $t = 0$, and all the other values of %ΔMW are summarized in Table A.2, Column "Δ Min Wage %." As a result, the estimate of the coefficient β_0 represents the semi-elasticity of top executive pay to MW.

The results of the estimation of Equation 3 are presented in Table 4. In column (1), which

uses total top executive pay as a dependent variable, β_0 is equal to 0.057 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 0.57% increase in top executive 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 top executive total pay through the channels of increased salary and incentive pay.

The estimated value of semi-elasticity of top executive total pay to MW changes, which is equal to 0.057, implies that a 40.78% total increase in federal MW triggered by the Fair Minimum Wage Act of 2007 results in a \$10,179 ($0.057 \times 40.78\% \times \$437,908$) increase in total pay for the top executive of a median firm in my sample. This effect corresponds to a 2.32% ($(\$10,179/\$437,908)\times 100\%$) relative increase in top executive total pay.

In the next section, I turn to the discussion of potential mechanisms that may be driving the established and quantified top executive pay raises in response to the worker wage hikes resulting from the MW hike.

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

Knowing that top executives 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 four groups of potential mechanisms that are consistent with my results. In particular, I attempt to understand which of these mechanisms and to what extent are at work. This list of mechanisms is not exhaustive, but covers the most plausible and intuitive explanations of the observed top executive pay raises around the MW shock to the best of my knowledge.

6.1. Risk premium mechanism

I start by exploring whether the pay raises after the MW shock are higher for top executives of the most unstable, vulnerable firms ex ante. If the top executives demand a premium for being exposed to extra risk driven by the MW level raise, then the observed pay increases should be highest for top executives of financially distressed and very small firms.

To test this mechanism, I first compare the estimates for β_0 from Equation 2b for financially distressed firms (with Altman (1968) Z-score less than 1.85 or Ohlson (1980) O-score larger than 0.5) to financially stable firms (with Z-score greater than 3 or O-score smaller than 0.5). Columns (1) - (4) of Table 5 present the results: the economic magnitudes of the coefficients

imply that a 10% increase in the share of workers in bound states leads to a 19.92% to 21.36% (magnitude depending on the choice of proxy) increase in top executive pay level for distressed firms, with the coefficient estimate being statistically significant. In contrast, for financially stable firms, the estimate is both statistically and economically insignificant.

I supplement this analysis by looking at very small U.S. public firms, defined as the lowest tercile of the sample (following the logic similar to the original sample construction). Columns (5) and (6) show that for very small firms, the β_0 estimate from Equation 2b is positive and statistically significant. The economic magnitude translates into a 10% increase in the share of workers in bound states leading to a 14.55% increase in top executive pay levels for very small affected vs. unaffected firms after the hike. However, for the top two terciles of the sample, the coefficient value is statistically and economically insignificant.

To summarize, both tests present evidence in favor of top executives of financially distressed firms and very small public firms being compensated more than top executives of their counterparts. This is consistent with the risk premium scenario: I conclude that it is likely that the compensation for risk mechanism plays an important role in driving the results. My result bridges an important gap connecting two prominent streams of research: one linking MW raises to increased distress risks (Luca and Luca (2019), Aaronson et al. (2018), Alexandre et al. (2022), Acar et al. (2019), Chava et al. (2023)), and the other linking distress risks to compensation premia (Chang et al. (2016), Graham et al. (2023), Peters and Wagner (2014)).

6.2. Bargaining mechanism

In this section, I analyze the impact of the MW hike on the ability of top executives to bargain for wages. Since standard Nash bargaining models define firm rents and the strength of the bargaining power as two key determinants of the ability to bargain, I test whether any of these two factors change after the MW shock. In particular, I estimate Equation 2b using the dependent variables of profit per employee as a measure of firm rents and two proxies for top executive bargaining power: share of institutional ownership and board independence ratio. Higher firm rents represent a potentially larger compensation pool to be distributed, while a lower share of institutional ownership and lower board independence represent lower levels of external pressure. Each of these outcomes increases the bargaining ability of the top executive, which is consistent with the mechanism.

On the one hand, column (1) of Table 6 shows that, in the specification with the profit per employee as the dependent variable, the estimate of β_0 is positive but statistically insignificant. This implies that firm rents do not significantly increase after the MW shock. On the other

hand, column (2) shows the same pattern for the specification with the logarithm of the share of institutional ownership as the dependent variable. Finally, column (3) shows that the board independence ratio decreases after the MW hike, however, the β_0 coefficient estimate is only weakly statistically significant (at the 10% level). Taken together, columns (2) and (3) illustrate no meaningful changes to the strength of the top executive bargaining power after the MW shock. Overall, the empirical patterns of β_0 coefficient estimates presented in Table 6 are not consistent with the bargaining mechanism - the evidence suggests that changes in bargaining ability are unlikely to drive my result of top executive pay increases after the MW hike.

6.3. Labor mechanisms: cost-of-living adjustment vs. efficiency wages

In this section, I consider two mechanisms driven by the effects of the MW shock on labor market outcomes. The labor literature differentiates between the two fundamental views on labor market structure: competitive labor markets and monopsony. As a result, the differences driving these views give rise to the cost-of-living adjustment and efficiency wages mechanisms, respectively, that may explain the result of top executive pay raises after the MW hike.

6.3.1. Cost-of-living adjustment mechanism

If the MW shock is passed on to consumers through higher prices (consistent with the competitive labor market view), top executives may request a compensation raise to adapt to the increased cost of living in bound states. If so, adjusting the nominal top executive pay levels for the cost-of-living changes should keep the real top executive pay level constant before and after the MW shock. In terms of the model, this implies that the triple-difference coefficient β_0 will be insignificant if real top executive pay is used as a dependent variable in Equation 2b. I obtain the real top executive compensation level by normalizing the nominal top executive compensation level by the corresponding yearly regional Consumer Price Index level for all urban consumers (CPI-U) from the Bureau of Labor Statistics data.

Column (1) of Table 7 presents the estimation results of Equation 2b: β_0 is statistically significant and equal to 0.67. The economic magnitude of the effect is that, following the MW shock, a 10% increase in the share of workers located in bound states leads to a 6.7% increase in realized total top executive pay for affected vs. unaffected firms. This estimate is just marginally lower than the main estimate of Table 3. This evidence goes against the cost-of-living adjustment scenario: I conclude that it is unlikely that top executives receive pay raises after the MW shock to offset the faster growth rate of the cost of living in bound states.

6.3.2. Efficiency wages mechanism: 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. To do that, I estimate a variation of Equation 2b 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. A positive estimate of β_0 is consistent with the efficiency wages scenario since it implies that firm performance improves after the MW shock, consistent with higher wages boosting worker productivity.

Table 7, Columns (2) and (3) present the estimation results. The estimates for β_0 are statistically insignificant regardless of the firm performance proxy used. Moreover, when industry-adjusted ROA is used as a proxy (column (2)), the value of β_0 is estimated to be near zero. These results illustrate that there is no meaningful difference in performance between affected vs. unaffected firms after the MW hike, which is inconsistent with the efficiency wages mechanism predictions.

6.3.3. Efficiency wages mechanism: Variation in firm executive pay-setting dates

To further enhance the power of the efficiency wages mechanism test, I adopt a second, more sophisticated approach. This approach relies on exploiting the variation in firm-level executive 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 executive compensation levels one time per year (Bebchuk, Cremers, and Peyer (2011), Dow and Raposo (2005)). Therefore, the U.S. public firms in my sample could be divided into two groups within one fiscal year: those that disclosed top executive pay from December 15, 2006 to May 25, 2007 ("pre-period", hereafter) and others. Figure 6 summarizes this timeline: both dates of December 15, 2006, and May 25, 2007, are vital for the identification strategy.

In particular, starting from December 15, 2006, firms had to adhere to new filing requirements amended by the SEC when disclosing executive compensation data.[21] Therefore, limiting the sample to firms that disclose top executive compensation levels after this date allows to exclude the differential filing standard effects. Furthermore, the date of May 25, 2007, is crucial in the context of the MW law because this is the date when the law was enacted and, hence,

[21]In July 2006, the SEC introduced new rules that completely changed the reporting standards for executive and director compensation levels. The press release from the SEC website is available here: https://www.sec.gov/news/press/2006/2006-219.htm.

introduced to the public. Hence, assuming that the MW law enactment was not anticipated, firms disclosing top executive pay in the pre-period were at a relative disadvantage: they did not have a chance to renegotiate top executive pay and had to keep it for the ongoing fiscal year 2007. However, in FY 2008 (December 15, 2007, to May 25, 2008; "post-period", hereafter), these firms were finally able to incorporate the new information about the MW hike into their top executive compensation-setting practices.

Therefore, for the purposes of my study, it is interesting to see whether there is a difference in top executive compensation levels for firms from December 15, 2006, to May 25, 2007, and December 15, 2007, to May 25, 2008. To explore this conjecture, I develop a triple-differences model summarized in Equation 4:

$$\begin{aligned}\log(\text{Top executive pay}_{i,j,t}) = {} & \beta_0(\text{FY 2008}_t \times \%\text{Bound}_{i,t} \times \text{Affected}_j) \\ & + \beta_1(\text{FY 2008}_t \times \%\text{Bound}_{i,t}) + \beta_2(\text{FY 2008}_t \times \text{Affected}_j) \\ & + \beta_3(\%\text{Bound}_{i,t} \times \text{Affected}_j) + \beta_4\text{FY 2008}_t + \beta_5\%\text{Bound}_{i,t} \\ & + \beta_6\text{Affected}_j + \Gamma\text{X}_{i,t-1} + \mu_j + \gamma_i + \epsilon_{i,j,t},\end{aligned} \tag{4}$$

where all the variables except FY 2008 and μ are the same as in the main DDD specification of Equation 2b. FY 2008 is a dummy variable equal to 1 for the post-period and 0 for the pre-period. This definition follows the existing literature that treats the executive 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)). μ are industry fixed effects - since the time horizon of the Equation 4 corresponds to the union of two periods: December 15, 2006, to May 25, 2007, and December 15, 2007, to May 25, 2008. Moreover, the sample of firms is limited to those with filing dates from December to May to ensure that firms are compared within the same industries during different fiscal years.

Crucially, this methodology is particularly fruitful for disentangling the efficiency wages mechanism from the other potential mechanisms at work. Since I consider top executive pay changes during the several calendar months only, the observed effects (if any) are unlikely to 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 scenario.

Column (4) of Table 7 presents the estimation results of Equation 4. The estimate of β_0 is positive, statistically significant, and equal to 5.572. This estimate is also economically significant: a 10-percentage point increase in the share of employees located in bound states

leads to a 55.72% increase in top executive total pay levels for affected vs. unaffected firms post- vs. pre-MW shock. As discussed above, this evidence goes against the efficiency wages mechanism.

As a result, taking together the evidence established in Sections 6.3.2 and 6.3.3, I conclude that the efficiency wages mechanism is unlikely to be driving the top executive pay raises observed after the MW shock.

6.4. Executive-employee pay ratio mechanisms: tournament incentives vs. shareholder constraints vs. employee morale

In this section, I analyze the three mechanisms operating through the executive-employee pay ratio changes due to the MW shock. Each mechanism is initially driven by the fact that a MW hike increases worker wages and hence reduces the executive-employee pay ratio (before the executive pay adjustment, if any).

6.4.1. Tournament incentives mechanism

To start with, one possibility is that the MW shock exogenously raises worker wages, which triggers a raise for all other higher-ranked workers, including the top executives, through a tournament incentives model. According to the model, high top executive-employee pay ratios present strong grounds for within-firm tournaments. Hence, if this scenario is true, I expect the estimate of β_0 from Equation 2b to be larger for firms with high top executive-employee pay ratios.

To test this mechanism, I construct the top executive-employee pay ratio at the firm-year level since firms were not obliged to disclose it during the time horizon of my study.[22] In particular, I follow the labor-finance literature (e.g., Faleye et al. (2013)) and proxy for average employee pay by $(xlr - \textit{Total top executive pay})/\#\textit{employees}$, where *xlr* is total staff expense from Compustat, and $\#\textit{employees}$ is the number of workers in all establishments of a firm from YTS. I define top executive-employee pay ratio as $\textit{Total top executive pay}/\textit{Average employee pay}$.

To overcome the issue of missing values and self-selection into disclosing *xlr*, I further follow the *xlr* imputation procedure developed by Donangelo (2016). I first define industries based

[22] In September 2013, the SEC meeting resulted in a majority vote for approval of Section 953(b) of the Dodd-Frank Act, which required all public firms filing annual reports with the SEC to disclose their CEO-to-median employee pay ratios. The final version of the rule was adopted in August 2015, and each firm was obliged to provide data on the CEO-employee pay ratio starting in its first full fiscal year beginning on or after January 1, 2017. However, in the time horizon surrounding the Fair Minimum Wage Act of 2007, the rule was not yet proposed by the SEC: the event window I use in this paper covers years from 2005 to 2011 (See Figure 5).

on FF-17 codes, using SIC2 where FF-17 code is unavailable. Next, I separate firms into firm size buckets every year and create imputation groups based on industry and size buckets (Hartman-Glaser, Lustig, and Xiaolan (2019)). I use five size buckets for imputation to ensure a balanced firm distribution among the groups based on the relative distribution of firms by FF-17 industries in the main sample. This procedure results in the definition of average employee pay given in Equation 5:

$$\text{Average employee pay} = \begin{cases} \text{(xlr - total top executive pay)/\# employees, } \textit{if xlr is non-missing} \\ \text{average ((xlr - total top executive pay)/\# employees),} \\ \qquad\qquad \text{by industry and firm size bucket, } \textit{if xlr is missing} \end{cases} \tag{5}$$

I keep only firms with non-missing observations for each $t \in [-2; 2]$ period. Table IA.2 shows the descriptive statistics for the top executive-employee pay ratio and its components for the sample: based on the imputation procedure in Panel A and based on the original *xlr* variable in Panel B.[23] The distribution of the top executive-employee pay ratio is similar before and after the imputation, indicating that the imputation procedure provides a reasonable approximation of the pay ratio values while increasing the sample size by over two times.

Table 8, columns (1) and (2) present the results of estimating Equation 2b for firms with low (below median) and high (above median) top executive-employee pay ratios before the MW shock. The estimate of the β_0 coefficient is statistically and economically significant and equal to 1.179 for firms with low pre-shock pay ratios, while it is insignificant for firms with high pay ratios. This evidence goes against the tournament incentives scenario since top executives of firms with lower tournament incentives received higher compensation raises after the MW shock. I conclude that tournament incentives are unlikely to be at play when explaining the top executive pay raises.

6.4.2. Shareholder constraints mechanism

Alternatively, a MW raise may translate into an increase in the median wage level, in turn reducing the top executive-employee pay ratio. This reduction could give an opportunity for top

[23]By definition of Equation 5, average employee pay is the average pay of all employees except for the top executive, including non-highest-paid executives. Moreover, *xlr* includes incentive compensation, other benefit plans, payroll taxes, pension costs, and profit sharing (when included in staff expense by the company) on top of employee salaries and wages. These two facts imply that the statistics for average employee pay are higher than median worker pay numbers cited in the media, which do not include non-highest-paid executive pay and other labor expenses apart from employee wages (Faleye et al. (2013)).

executives to bargain for pay raises for themselves until the ratio reaches the toleration limit for the shareholders. If so, I expect that the β_0 coefficient estimate from Equation 2b would be larger for top executives of firms with the highest degree of shareholder constraints, namely, with the highest number of blockholders.

Columns (3) and (4) of Table 8 show that for firms with a large (above median) number of blockholders, the estimate of β_0 is smaller and less statistically significant than for firms with a small (below median) number of blockholders. This evidence is inconsistent with the shareholder constraints scenario since the fewer the number of blockholders, the less constrained top executives are in negotiating their pay with the board. I conclude that shareholder constraints through high top executive-employee pay ratios are unlikely to be driving the top executive pay raises result around the MW hike.

6.4.3. Employee morale mechanism

Last but not least, it is possible that a MW raise reduces the relative tension between top executives and workers due to a reduction in the large pay inequality between them. If so, the MW raise provides a convenient setting for top executives to lock in a pay raise for themselves when workers are less unhappy with the executive-employee pay ratio due to their own wage raises. Therefore, if this scenario is true, I expect that the β_0 estimate from Equation 2b would be larger for firms in highly unionized industries, where the tension between top executives and workers over the pay levels is stronger ex-ante.

Table 8, columns (5) and (6) show the estimation results of Equation 2b for the firms in highly (above median) unionized and low (below median) unionized industries. In contrast to the predictions of the employee morale scenario, top executives of firms in highly unionized industries receive relatively smaller compensation raises after the MW shock. This is reflected in a smaller magnitude of β_0 and its statistical significance. Therefore, I conclude that employee morale is unlikely to be a key driver in the observed top executive pay raises after the MW shock.

7. Robustness

In the above analysis, I consider the four groups of mechanisms that may explain the positive spillover effects from worker wages to total top executive compensation after the MW law. I show evidence that is most consistent with the risk premium mechanism and largely inconsistent with the three other groups of mechanisms.

This analysis relies on several assumptions. One assumption is that variables (for example, firm profitability), which are directly correlated with worker wages and executive pay, do 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. Do firm outcomes change around the minimum wage law?

First, one may argue that a government policy such as a MW change may affect many potential firm-level outcomes in addition to worker wages. In the methodology of this study, I attempt to control for a wide range of such outcomes. In particular, control variables of Equation 2b include lagged firm size, ROA, profitability, Tobin's Q, market-to-book, and log(1+annualized return). I also include industry by year fixed effects to control for time-varying industry shocks (Gormley and Matsa (2014)), firm fixed effects to control for unobserved firm heterogeneity, and compensation dataset fixed effects to control for potential variable measurement heterogeneity. Nevertheless, for robustness purposes, I additionally consider the effect of the MW hike on all the control variables of the main DDD specification (Panel A) and several other observable firm characteristics (Panel B) in Table A.3.

Within the set of control variables, one natural outcome variable of interest is profitability - this is 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 the top executive pay levels. Therefore, if my established results of the top executive 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. Table A.3, Panel A shows the estimation results of Equation 2b using profitability (columns (1) and (2)) and size, ROA, Tobin's Q, M/B ratio, and log(1+annualized return) as dependent variables (columns (3) - (12)). In both specifications (without and with control variables), the estimates are statistically

and economically insignificant. This implies that my results are robust and unlikely to suffer from endogeneity concerns - in particular, they go against the profitability scenario.

Moreover, in Panel B of Table A.3, I further consider the evolution of employment, leverage, capital expenditures, and R&D expenditures around the MW shock. None of the estimates, except in the specifications with employment as the dependent variable (columns (1) and (2)), are statistically significant. The estimates for employment are negative and statistically significant, translating into a decline in employment after the MW shock, consistent with competitive labor market models. Moreover, the coefficient estimates for capital expenditures (columns (5) and (6)) replicate the result of Gustafson and Kotter (2023).

Overall, the evidence presented in Table A.3 enhances the plausibility of my results that, following a MW hike that boosts worker wages, top executives also enjoy compensation raises.

7.2. Do differences in observable firm characteristics affect the results?

An alternative concern is based on the fact that my identification strategy relies on comparing top executive pay outcomes for firms based on their industry (affected vs. unaffected). The idea is that the MW hike will largely impact affected firms, which comprise restaurants, hotels, grocery stores, and other businesses that rely primarily 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.

Based on this argument, one may worry that firms belonging to different business segments may be fundamentally differing with respect to observable firm characteristics. If this is the case, the link between the worker wage raise and the top executive pay raise through the MW hike will be confounded. Indeed, if affected and unaffected firms are not comparable with respect to the observable characteristics, these differences in characteristics may be responsible for driving the results. In the baseline specification of Equation 2b, I include control variables, industry by year fixed effects, firm fixed effects, and compensation dataset fixed effects to take care of this concern in a straightforward way.

To further mitigate this concern, I construct a matched sample. I first classify the firm-years into "bound" or "unbound" to make sure that firms are comparable with respect to the intensity of exposure to the MW hike effects. A firm is bound if it has more than the sample median of 15.40% employees located in bound states at $t = -1$; and unbound otherwise. 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 for matching and to ensure that a firm does not switch from bound to unbound or vice versa during the time horizon. 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 (matching (un)affected firms with (un)affected firms).

The resulting sample consists of 1,648 firms per period. In line with the rest of the paper, I shift the focus of the analysis to smaller U.S. public firms (defined as bottom two sample terciles by employment at $t = -1$). Table A.4 shows that matched bound and unbound firms do not significantly differ on the pre-treatment top executive pay levels and firm characteristics. I further estimate Equation 2b on the matched sample, using top executive total pay as the dependent variable. If my results are robust, I expect to find a positive and significant estimate of β_0. Column (1) of Table A.5 illustrates that these patterns hold. This evidence shows that differences in observable firm characteristics between firms exposed to treatment and those not exposed to treatment are unlikely to drive my results.

7.3. Do changes in local economic conditions affect the results?

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 2b). 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.

Therefore, one may worry that the MW policy may be regulated to impact firms differentially across states. To address this concern, I show that state-level shocks are unlikely to be responsible for my results. I start with applying the border analysis method used in the literature (e.g., Pence (2006), Holmes (1998), Dube et al. (2010), Gustafson and Kotter (2023)). This method relies on the claim that firms headquartered on opposing sides of the state border but very close to each other geographically (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. Next, I leave only firms

headquartered in the counties located on the borders of state pairs and obtain a sample which consists of 802 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. Finally, consistent with the rest of the paper, I focus on the smaller firms, defined as bottom two sample terciles by employment at $t = -1$.

I next estimate Equation 2b on the bordering counties sample. If my results are robust, the coefficient β_0 will be positive and significant. Column (2) of Table A.5 shows that this pattern holds.

To further refute the concern about differences in state-level economic conditions potentially driving my results, I estimate Equation 2b on the main sample but excluding the 2008-2009 years of the global financial crisis (following, e.g., Gustafson and Kotter (2023)). Column (3) of Table A.5 reflects the positive and significant estimate of the coefficient β_0, consistent with the main results.

Finally, one may argue that based on the map in Figure 3, bound states are likely to lean Republican and follow a conservative political ideology. To alleviate the concern that the political environment may be responsible for my results, I estimate Equation 2b on the main sample controlling for the share of employees located in states won by the Republican party in the most recent presidential election cycle (given by the "%Republican" variable). Column (4) of Table A.5 confirms that the sign and magnitude of the coefficient β_0 do not change significantly compared to the main results, further alleviating the concern about differential state-level shocks as a confounding factor.

8. Conclusion

Using a sample of U.S. public firms matched to granular establishment-level location and employment data, I document a positive spillover effect of worker wages on top executive compensation levels, driven by the MW hike. Additionally, I evaluate the role of compensation for risk, bargaining, cost-of-living adjustment, efficiency wages, tournament incentives, shareholder constraints, and employee morale 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 a positive link between top executive and worker pay in the United States. I document that a 41% increase in the federal MW level mandated by the Fair Minimum Wage Act of

2007 results in a relative 2.32% increase in top executive compensation levels during the three consequent fiscal years. Using the standard baseline triple-difference specification, I estimate that, after the MW law, top executive pay increases by 71.8% for firms in MW-sensitive industries vs. other industries in states bound vs. unbound by federal MW levels. Further building upon the baseline methodology through the main specification based on the variation in the share of workers in bound states across time and industries, I establish a similar robust effect. In particular, I show that, after the MW law, a 10% increase in the share of workers in bound states leads to a 6.73% total top executive pay increase for firms in MW-sensitive industries vs other industries.

I interpret my results as most consistent with the compensation for risk mechanism. I find that the pay increases are more pronounced at ex ante financially distressed and very small public firms, where top executives are more exposed to the risk of bankruptcy. This evidence bridges the gap between existing studies that separately link MW hikes to increased distress risks and distress risks to compensation levels. In contrast, I do not find evidence consistent with the bargaining, cost-of-living adjustment, efficiency wages, tournament incentives, shareholder constraints, or employee morale mechanisms driving the link between top executive and worker pay. In particular, after the MW hike, there is no significant change in either firm rents (proxied by profits per employee) or top executive bargaining power strength (proxied by share of institutional ownership and board independence); the pay increases are pronounced in real terms as well as in nominal terms; and firm performance does not significantly improve. 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 top executive pay levels after the hike. Since significant changes in firm productivity are unlikely to manifest within a short time frame, this result serves as additional evidence inconsistent with the efficiency wages scenario. Furthermore, there is no stronger effect for firms with increased attention to executive-employee pay inequality, such as firms with high top executive-employee pay ratios, a large number of blockholders, and in industries with high unionization.

Future research is necessary to understand the connection between worker pay and top executive pay in more detail. Does the spillover effect extend to the other members of the C-suite, or is it pronounced only for the highest-paid executives? How do firms adjust production in response to the federal MW hike? These are just a few examples of open questions to be addressed within the fruitful area of worker and top executive pay link.

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Tables and figures

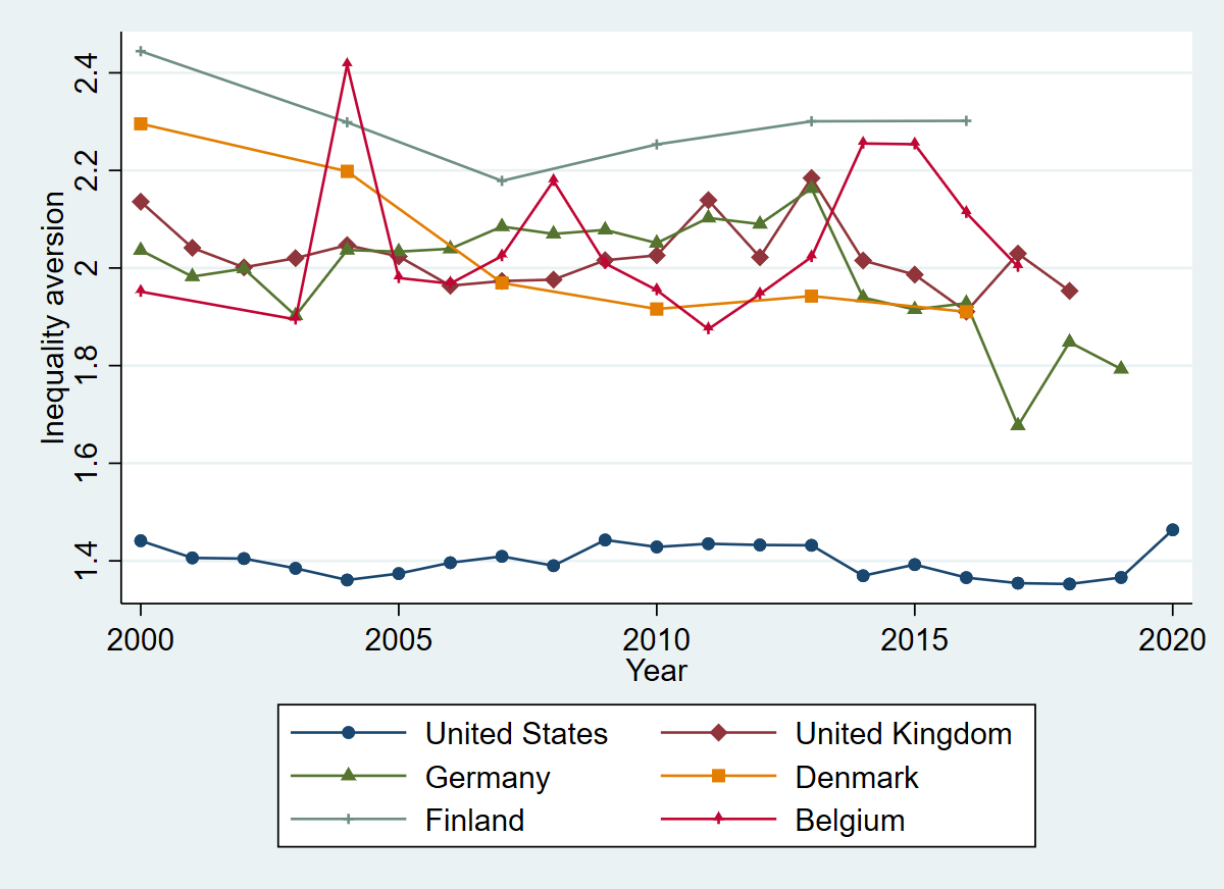


Figure 1: Inequality aversion index by country

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).

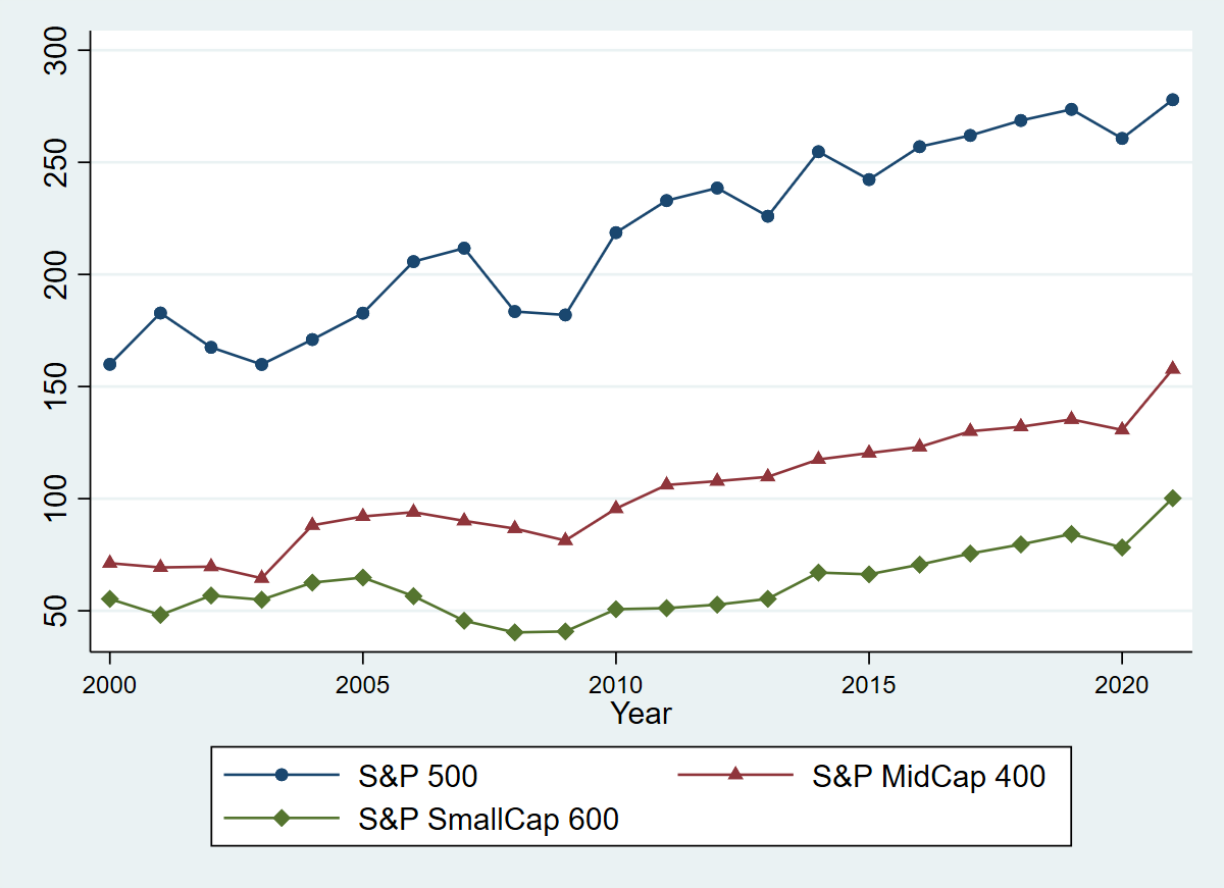


Figure 2: Evolution of the CEO-employee pay ratio by firm size

The figure illustrates the time series trends in the CEO-employee pay ratio by firm size. Due to the lack of data on median employee pay by firm size, the CEO-employee 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.

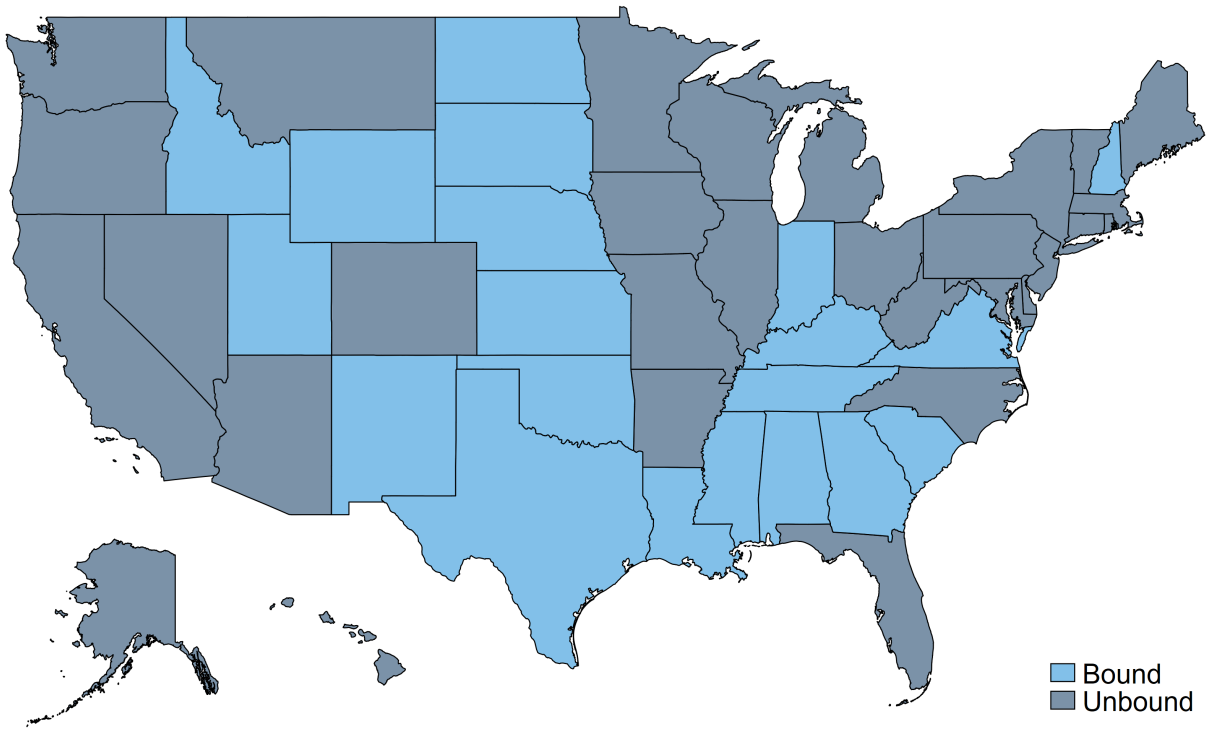
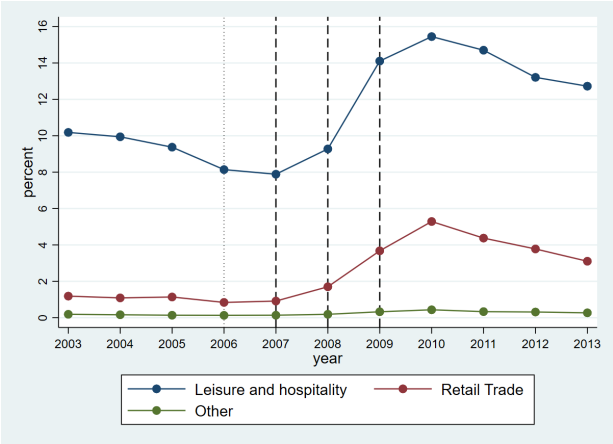
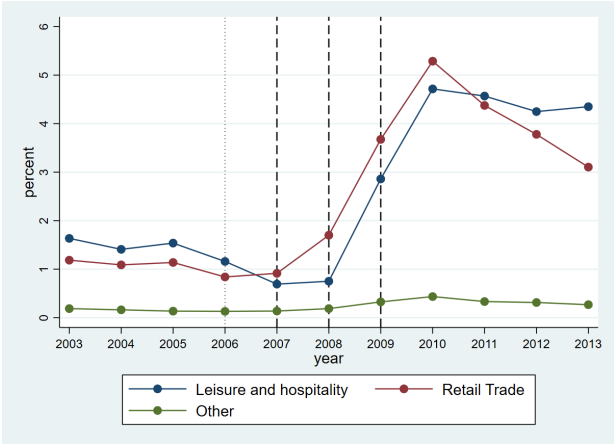


Figure 3: Map of bound and unbound states

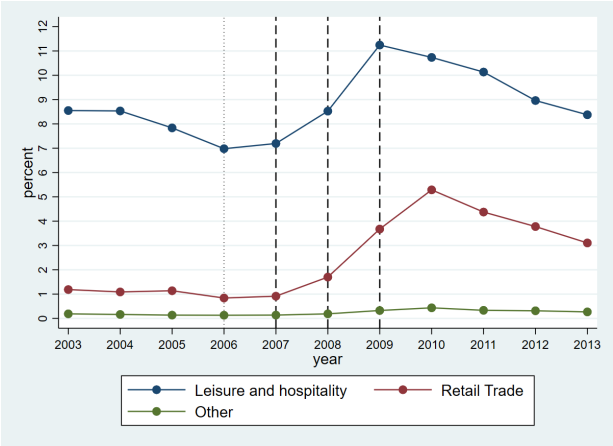
The figure illustrates the distribution of bound and unbound states defined pre-introduction of the law (Clemens and Wither (2019), Gustafson and Kotter (2023), Chava et al. (2023)). A state is bound if the state minimum wage is lower than the federal minimum wage. A state is unbound if the opposite 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 A.2).

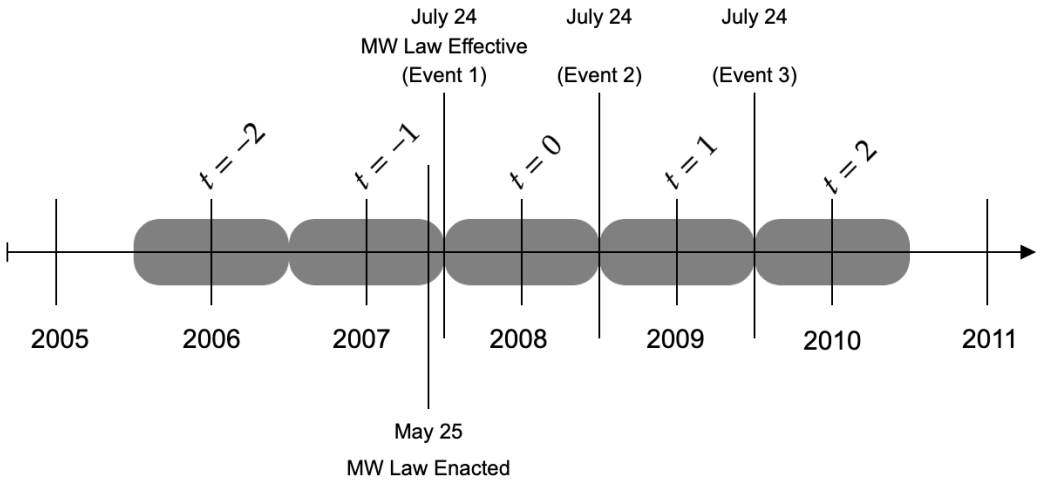


Figure 5: Timeline

The figure illustrates the timeline of the events associated with the MW law. Details on the events are provided in Table A.2.

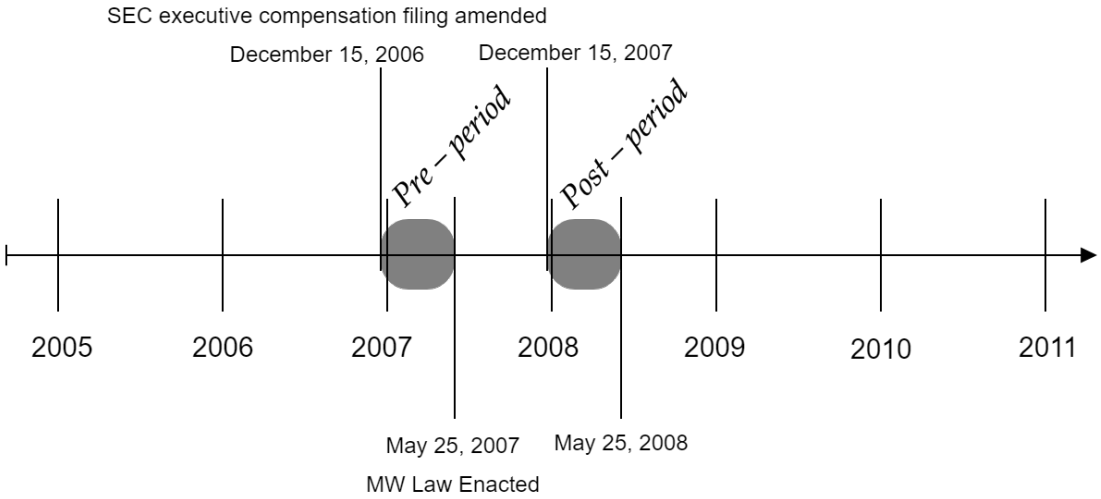


Figure 6: Timeline: Efficiency wages test

The figure illustrates the timeline associated with the sophisticated test of the efficiency wages mechanism. The identification strategy is based on the variation in the firm executive pay setting dates and discussed in Section 6.3.3.

Table 1: Summary statistics

The table displays summary statistics for the main sample of smaller firms (bottom two terciles by employment at $t = -1$) headquartered in the U.S. from $t = -2$ to $t = 2$ discussed in Section 3.2 of the text. The variables are defined in Table A.1.

	Mean	Median	Std	5th	95th	N
Total pay (′000)	1,024.486	437.908	2,232.043	41.051	3,780.785	5,610
Salary (′000)	336.366	285.000	224.469	96.961	750.000	3,940
Bonus (′000)	154.359	0.000	486.687	0.000	734.902	3,790
Cash pay (′000)	524.694	373.331	624.431	114.833	1,396.568	3,376
Incentive pay (′000)	868.424	182.392	2,443.518	0.000	3,886.960	3,376
Affected	2.674	0.000	16.133	0.000	0.000	5,610
Bound	0.246	0.000	0.431	0.000	1.000	5,610
%Bound	26.727	5.115	36.378	0.000	100.000	5,610
# employees	898.761	540.000	1,329.914	29.000	2,571.000	5,610
Total assets (in MM)	1,900.923	607.713	4,383.614	31.754	7,805.101	5,610
Size	6.314	6.410	1.648	3.458	8.963	5,609
M/B	1.808	1.243	1.429	0.827	4.725	5,609
ROA	-0.002	0.016	0.191	-0.335	0.192	5,609
Profitability	0.052	0.067	0.190	-0.242	0.264	5,609
Q	1.812	1.248	1.429	0.835	4.725	5,609
log(1+annual return)	-0.068	-0.005	0.563	-1.071	0.711	5,610

Table 2: Baseline: The effects of the minimum wage law on top executive pay

The table presents estimates from firm-level baseline regressions defined in Equations 1a (column (1)) and 1b (columns (2)-(6)), following Gustafson and Kotter (2023). The regressions examine top executive 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 influenced by the minimum wage law. The sample is outlined in Table 1. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). Bound is an indicator equal to 1 for firms located in bound (where federal MW is greater than or equal to state MW) states at pre-treatment period $t = -2$. Affected is an indicator equal to 1 for firms belonging to NAICS2 industries of 71, 72 (Leisure and Hospitality), 44, 45 (Retail Trade). Bound X Affected and Affected components drop out of estimation results since they are collinear with fixed effects. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at $t = -2$. Industry by year, firm, and top executive pay dataset 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.

	Total pay		Cash pay			Incentive pay
	(1)	(2)	(3)	(4)	(5)	(6)
	log(total pay)	log(total pay)	log(1+cash pay)	log(1+salary)	log(1+bonus)	log(1+incentive pay)
Post × Bound × Affected		0.718**	0.430**	0.298	0.578	2.488**
		(2.09)	(2.39)	(0.79)	(0.47)	(2.37)
Post × Bound	-0.074	-0.086*	0.111*	0.093**	0.097	-0.125
	(-1.60)	(-1.91)	(1.92)	(2.09)	(0.70)	(-0.74)
Post × Affected		-1.005	-0.307	-0.454	-0.423	-1.206
		(-1.67)	(-1.13)	(-1.14)	(-0.41)	(-0.63)
Post	0.164	0.202	-0.058	0.040	-0.190	0.384
	(1.15)	(1.38)	(-0.41)	(0.46)	(-0.66)	(1.06)
Bound	-0.125	-0.117	0.256	0.058	0.836	-0.075
	(-0.33)	(-0.31)	(0.44)	(0.37)	(0.69)	(-0.04)
Constant	4.573***	4.544***	5.633***	5.001***	5.966***	1.523
	(10.41)	(10.39)	(10.97)	(11.71)	(4.16)	(1.32)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Dataset FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.47	.47	.43	.37	.43	.61
N	5,610	5,610	3,376	3,940	3,790	3,376

Table 3: Main result: The effects of the minimum wage law on top executive pay

The table presents estimates from firm-level main regressions defined in Equations 2a (column (1)) and 2b (columns (2)-(6)). The regressions examine top executive 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 influenced by the minimum wage law. Additionally, for identification purposes, column (7) estimates the evolution of the %Bound variable before and after the minimum wage law. The sample is outlined in Table 1. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry by year, firm, and top executive pay dataset 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.

	Total pay		Cash pay			Incentive pay	Identification
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	log(1+total pay)	log(1+total pay)	log(1+cash pay)	log(1+salary)	log(1+bonus)	log(1+incentive pay)	%Bound
Post × %Bound × Affected		0.673***	0.600	0.641**	-0.083	4.992***	
		(4.21)	(1.29)	(2.53)	(-0.09)	(5.66)	
Post × %Bound	-0.077	-0.088*	0.083	0.059	-0.077	-0.174	
	(-1.64)	(-1.90)	(1.26)	(1.00)	(-0.44)	(-0.72)	
Post × Affected		-0.983*	-0.403**	-0.539	-0.263	-2.132	
		(-1.92)	(-2.02)	(-1.51)	(-0.24)	(-1.35)	
%Bound × Affected		0.365	-2.254***	0.626*	-2.227	9.552*	
		(0.57)	(-2.90)	(1.84)	(-0.28)	(1.77)	
Post	0.168	0.205	-0.058	0.043	-0.158	0.402	-0.004
	(1.18)	(1.41)	(-0.42)	(0.49)	(-0.55)	(1.10)	(-0.63)
%Bound	0.290	0.256	-0.057	-0.091	-0.061	0.199	
	(1.55)	(1.33)	(-0.16)	(-0.28)	(-0.08)	(0.37)	
Constant	4.470***	4.470***	5.720***	5.061***	6.137***	1.519	0.214***
	(10.12)	(10.09)	(12.05)	(11.74)	(4.29)	(1.43)	(6.13)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dataset FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Adj-R-squared	.47	.47	.43	.37	.43	.62	.97
N	5,610	5,610	3,376	3,940	3,790	3,376	5,610

Table 4: Semi-elasticity of top executive pay with respect to minimum wages

The table presents estimates based on a variation of firm-level main regressions defined in Equation 3, examining top executive 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 influenced by the minimum wage law. The sample is outlined in Table 1. %Δ MW is the percentage change in MW level and is defined in Table A.2. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry by year, firm, and top executive pay dataset 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.

	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)
%ΔMW × %Bound × Affected	0.057***	0.047	0.054***	0.007	0.379***
	(4.68)	(1.57)	(2.94)	(0.10)	(5.62)
%ΔMW × %Bound	-0.007	0.006	0.004	-0.010	-0.013
	(-1.64)	(1.09)	(0.90)	(-0.74)	(-0.67)
%ΔMW × Affected	-0.065	-0.022	-0.038	-0.016	-0.154
	(-1.59)	(-1.34)	(-1.34)	(-0.21)	(-1.23)
%Bound × Affected	0.346	-2.309***	0.591*	-2.260	9.229*
	(0.51)	(-2.93)	(1.72)	(-0.28)	(1.74)
%ΔMW	0.010	-0.006	0.001	-0.014	0.039
	(1.03)	(-0.60)	(0.16)	(-0.70)	(1.50)
%Bound	0.253	-0.051	-0.087	-0.047	0.194
	(1.31)	(-0.14)	(-0.26)	(-0.06)	(0.36)
Constant	4.516***	5.717***	5.075***	6.124***	1.483
	(10.35)	(12.19)	(11.81)	(4.25)	(1.41)
Controls	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Dataset FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.47	.43	.37	.43	.62
N	5,610	3,376	3,940	3,790	3,376

Table 5: Compensation-for-risk mechanism

The table presents estimates from firm-level main regressions defined in Equation 2b, testing the risk premium mechanism as a potential driver of the top executive pay increases established in Table 3. The mechanism discussed in Section 4.3.1 of the text is defined in Section 6.1 of the text. The sample is outlined in Table 1. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry by year, firm, and top executive pay dataset 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.

Dependent variable: log(top executive total pay)

	Distressed		Stable		Very small	Other small
	(1)	(2)	(3)	(4)	(5)	(6)
	Z-score $<$ 1.85	O-score $>$ 0.5	Z-score $>$ 3	O-score $\leq$ 0.5	Bottom 1/3 by size	Top 2/3 by size
Post × %Bound × Affected	2.136***	1.992***	0.558	-0.806	1.455**	-0.299
	(5.32)	(2.96)	(1.18)	(-0.73)	(2.50)	(-0.20)
Post × %Bound	-0.216	0.216	-0.040	-0.054	-0.237*	-0.076
	(-0.57)	(0.45)	(-0.59)	(-0.47)	(-1.80)	(-1.21)
Post × Affected	-1.017	-1.366***	-0.874	-0.514	-1.730**	-0.842
	(-1.68)	(-3.59)	(-0.97)	(-1.60)	(-2.65)	(-1.38)
%Bound × Affected	1.544*	1.753	0.922	8.078***	-1.312***	5.155**
	(1.71)	(0.89)	(0.29)	(5.93)	(-2.76)	(2.56)
Post	0.584	0.746*	0.159	0.296	0.119	0.354
	(1.02)	(1.85)	(1.07)	(1.57)	(0.84)	(1.47)
%Bound	0.465	0.229	0.418*	0.426	0.815***	0.102
	(0.55)	(0.12)	(1.72)	(1.62)	(3.37)	(0.38)
Constant	2.702***	3.544***	4.816***	4.768***	4.338***	4.285***
	(4.94)	(4.89)	(9.41)	(9.89)	(7.44)	(6.69)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Dataset FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.41	.24	.47	.45	.33	.46
N	795	427	4,150	2,658	1,870	3,740

Table 6: Bargaining mechanism

The table presents estimates from firm-level main regressions defined in Equation 2b, testing the bargaining mechanism as a potential driver of the top executive pay increases established in Table 3. Column (1) uses a proxy for firm rents, while columns (2) and (3) use proxies for the strength of the bargaining power as dependent variables. The mechanism discussed in Section 4.3.2 of the text is defined in Section 6.2 of the text. The sample is outlined in Table 1. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry by year, firm, and top executive pay dataset 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.

	Firm rents	Executive bargaining power	
	(1)	(2)	(3)
	Profit per employee	log(%Institutional ownership)	log(Outsider ratio)
Post × %Bound × Affected	7.419	0.025	-0.227*
	(0.90)	(0.16)	(-1.69)
Post × %Bound	-7.857	-0.026	0.036*
	(-0.91)	(-1.33)	(1.75)
Post × Affected	-2.063	0.113	0.088
	(-0.94)	(0.30)	(1.07)
%Bound × Affected	-3.299	0.183	-1.135***
	(-1.02)	(0.75)	(-2.77)
Post	2.070	0.005	0.036**
	(1.03)	(0.15)	(2.11)
%Bound	2.793	0.071	0.109
	(1.04)	(0.60)	(1.29)
Constant	2.180	2.622***	3.739***
	(0.80)	(25.31)	(19.97)
Controls	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Dataset FE	No	No	No
Adj-R-squared	-.28	.87	.86
N	3,814	4,340	3,307

Table 7: Labor mechanisms

The table presents estimates from firm-level main regressions defined in Equation 2b, testing mechanisms based on the labor economics effects of the minimum wage raise as potential drivers of the top executive pay increases established in Table 3. The test of the cost-of-living adjustment mechanism discussed in Section 4.3.3 of the text is defined in Section 6.3.1 of the text and presented in column (1). The naive test of the efficiency wages mechanism discussed in Section 4.3.3 of the text is defined in Section 6.3.2 of the text and presented in columns (2) and (3). The sophisticated test of the efficiency wages mechanism discussed in Section 4.3.3 of the text is defined in Section 6.3.3 of the text and presented in column (4). The sample is outlined in Table 1 (columns (1)-(3)) and in Section 6.3.3 of the text (column (4)). Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). FY2008 is an indicator equal to 1 for $t \in$ [December 15, 2007; May 25, 2008] and 0 for $t \in$ [December 15, 2006; May 25, 2007]. %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry by year, industry, firm, and top executive pay dataset 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.

	Cost-of-living adjustment	Naive efficiency wages		Sophisticated efficiency wages
	(1)	(2)	(3)	(4)
	log(total real pay)	industry-adj. ROA	log(1+ann. ret.)	log(total pay)
Post × %Bound × Affected	0.670***	0.044	0.183	
	(4.17)	(0.69)	(0.51)	
FY 2008 × %Bound × Affected				5.572***
				(5.21)
Post × %Bound	-0.088*	-0.002	0.082**	
	(-1.91)	(-0.16)	(2.00)	
FY 2008 × %Bound				-0.416*
				(-1.82)
Post × Affected	-0.981*	0.027	0.040	
	(-1.92)	(0.51)	(0.25)	
FY 2008 × Affected				-1.782***
				(-4.85)
%Bound × Affected	0.370	-0.001	-1.102***	-5.930***
	(0.57)	(-0.02)	(-2.78)	(-3.68)
Post	0.205	-0.011	0.097	
	(1.41)	(-1.21)	(1.44)	
FY 2008				0.014
				(0.09)
%Bound	0.254	0.004	-0.075	0.149
	(1.31)	(0.25)	(-0.79)	(0.54)
Constant	3.737***	0.209***	2.941***	2.963***
	(8.45)	(3.33)	(12.64)	(13.75)
Controls	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	No
Industry FE	No	No	No	Yes
Dataset FE	Yes	Yes	Yes	Yes
Adj-R-squared	.47	.58	.45	.28
N	5,610	5,609	5,610	2,274

Table 8: Executive-employee pay ratio mechanisms

The table presents estimates from firm-level main regressions defined in Equation 2b, testing mechanisms based on the top executive-employee pay ratio effects of the minimum wage raise as potential drivers of the top executive pay increases established in Table 3. The test of the tournament incentives mechanism discussed in Section 4.3.4 of the text is defined in Section 6.4.1 of the text and presented in columns (1) and (2). The test of the shareholder power mechanism discussed in Section 4.3.4 of the text is defined in Section 6.4.2 of the text and presented in columns (3) and (4). The test of the employee morale mechanism discussed in Section 4.3.4 of the text is defined in Section 6.4.3 of the text and presented in columns (5) and (6). The sample is outlined in Table 1. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry by year, firm, and top executive pay dataset 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.

Dependent variable: log(top executive total pay)

	Tournament incentives		Shareholder power		Employee morale	
	(1)	(2)	(3)	(4)	(5)	(6)
	Low pay ratio	High pay ratio	Large num. block	Small num. block	High union	Low union
Post × %Bound × Affected	1.179***	0.587	0.594*	0.880***	0.540***	1.039**
	(6.08)	(1.32)	(1.68)	(4.35)	(4.35)	(2.48)
Post × %Bound	-0.093	-0.187**	-0.207	-0.037	-0.053	-0.105*
	(-1.08)	(-2.36)	(-1.41)	(-0.38)	(-0.59)	(-1.82)
Post × Affected	-0.443***	-0.720*	-0.684	-1.575***	-1.480***	0.696
	(-2.88)	(-1.95)	(-0.77)	(-6.12)	(-2.76)	(1.64)
%Bound × Affected	-3.389***	-1.509**	3.999***	-0.047	-0.245	2.069
	(-3.83)	(-2.55)	(3.20)	(-0.08)	(-0.69)	(1.04)
Post	0.806***	-0.388**	0.787***	-0.118	0.063	0.526
	(3.16)	(-2.62)	(3.17)	(-0.66)	(0.28)	(1.33)
%Bound	0.357	0.568	0.410	-0.368	0.316	0.141
	(0.85)	(1.33)	(1.29)	(-1.22)	(1.19)	(0.49)
Constant	3.924***	4.509***	4.551***	5.022***	4.725***	4.170***
	(5.58)	(4.34)	(6.41)	(9.76)	(7.45)	(7.23)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Dataset FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.46	.5	.37	.46	.45	.47
N	1,811	1,826	1,850	2,845	2,645	2,915

Appendix

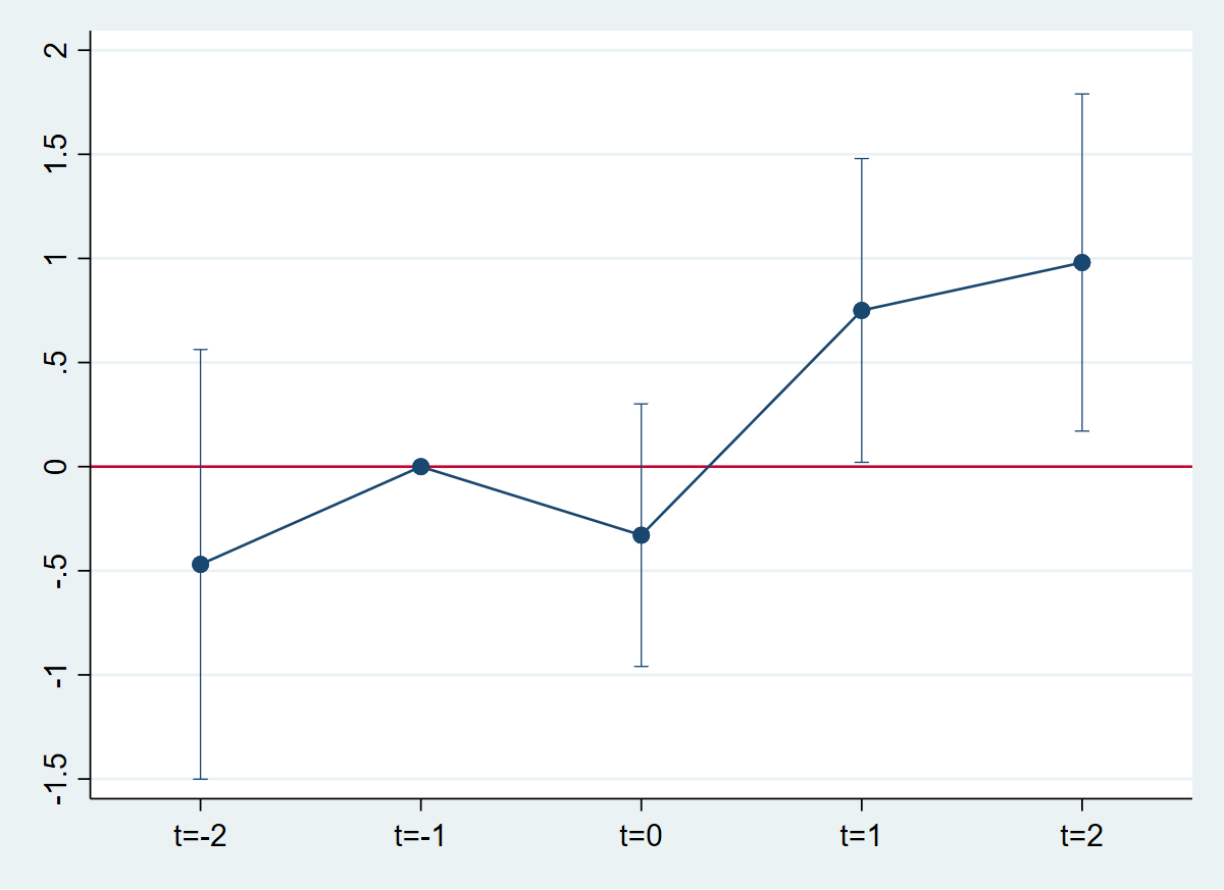


Figure A.1: DDD identification: Parallel pre-trends

This figure displays the OLS regression coefficients β_k and their respective 95% confidence intervals, estimated from the following model:
$\log(\text{top executive pay}_{i,j,t}) = \sum_{k=-2}^{k=2} \beta_k \times \text{D}_k \times \text{Affected}_j \times \%\text{Bound}_i) + \Gamma \text{X}_{i,t-1} + \delta_{j,t} + \gamma_i + \eta_d + \epsilon_{i,j,t}$. Top executive pay is total top executive pay in firm i of industry j at period t; D is a dummy variable equal to 1 for observations in period $t = k$; %Bound is the share of firm workers employed at establishments located in bound states; Affected is a dummy variable equal to 1 if a firm belongs to a NAICS2 71, 72, 44, or 45 industry; X is a vector of firm characteristics; δ are industry by year fixed effects; γ are firm fixed effects; and η_d are the top executive compensation dataset fixed effects. The sample is outlined in Table 1.

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	Executive pay datasets
Cash pay	Salary + Bonus	Executive pay datasets
Incentive pay	Total pay - Salary - Bonus	Executive pay datasets
Post	1 if t $\geq$ 0	YTS
Bound	1 if firm HQ is located in a bound state	Clemens and Wither (2019), Gustafson and Kotter (2023), Chava et al. (2023)
%Bound	% of employees located in bound states establishments	YTS
Affected	1 if in NAICS2 industries 71, 72, 44, 45	BLS
%ΔMW	percentage change in MW	See Table A.2
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
Total real pay	Total pay/CPI-U * 100	Executive pay datasets, BLS
FY 2008	1 if $t \in$ [12/15/2007; 5/25/2008]	see Section 6.3.3
Distressed	1 if Z-score $<$ 1.85 or O-score $>$ 0.5 at $t = -1$	Compustat
Stable	1 if Z-score $>$ 3 or O-score $\leq$ 0.5 at $t = -1$	Compustat
Very small	1 if in bottom 1/3 of sample at $t = -1$	YTS
Profits per employee	ib + xido + txdi + xint + dp or xdp	Compustat
% Institutional ownership	% ownership of 5 largest holders	Thomson Reuters
Board independence	(#Outside directors/Board size) * 100	BoardEx
Executive-employee pay ratio	Total executive pay/Avg. employee pay	see Section 6.4.1
#Blockholders	#Blockholders	Schwartz-Ziv and Volkova (2024)
Unionization	(%Cov * NAICS3 Employment)/(NAICS3 Employment)	Union Membership and Coverage Database
Employment	Sum of Employment across establishments per firm HQ	YTS
Leverage	$(dlc + dltt)/at$	Compustat
CapEx	$capx/at[n-1]$	Compustat
R&D	$xrd/at[n-1]$	Compustat
%Republican	% of employees located in Republican states establishments	YTS

Table A.2: 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. The evolution of the minimum wage policy in the United States is discussed in Section 2 of the text.

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 A.3: The effects of the minimum wage law on other firm characteristics – robustness

The table presents additional estimates of firm-level main regressions defined in Equation 2b, using control variables (Panel A) and alternative firm characteristics (Panel B) as dependent variables. The variables are defined in Table A.1. The sample is outlined in Table 1. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0;2]$). %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Control variables and industry by 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: Control variables

	Profitability		Size		ROA		Q		M/B		log(return)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post × %Bound × Affected	-0.047	-0.022	-0.075	0.116	0.052	0.073	1.099	0.986*	1.060	0.952	0.464	0.193
	(-1.21)	(-0.44)	(-0.51)	(0.91)	(0.71)	(0.96)	(1.61)	(1.71)	(1.50)	(1.58)	(1.04)	(0.53)
Post × Affected	0.048	0.019	0.049	-0.041	0.075***	0.052	-0.112	-0.215	-0.103	-0.206	-0.309**	0.020
	(1.09)	(0.37)	(0.47)	(-0.33)	(2.70)	(1.62)	(-0.39)	(-0.72)	(-0.35)	(-0.68)	(-2.18)	(0.13)
Post × %Bound	0.014*	0.009	0.053**	0.013	0.001	-0.002	0.088	0.088	0.086	0.086	0.018	0.081*
	(1.88)	(1.55)	(2.07)	(0.72)	(0.09)	(-0.16)	(1.20)	(1.36)	(1.16)	(1.32)	(0.52)	(1.97)
%Bound × Affected	0.085	0.061	0.617	0.229	0.042	0.018	-0.972	-0.675	-0.961	-0.672	-1.272***	-1.112***
	(1.20)	(0.87)	(1.40)	(0.48)	(0.90)	(0.36)	(-1.01)	(-0.79)	(-0.99)	(-0.77)	(-3.34)	(-2.78)
Post	-0.005	-0.007	0.019	0.017	0.000	-0.002	-0.114	-0.112	-0.113	-0.111	0.112	0.097
	(-0.52)	(-0.83)	(0.97)	(0.66)	(0.02)	(-0.15)	(-1.27)	(-1.35)	(-1.27)	(-1.34)	(1.65)	(1.46)
%Bound	0.032**	0.026**	0.068	-0.002	0.013	0.003	-0.234	-0.151	-0.231	-0.149	-0.122	-0.074
	(2.24)	(2.05)	(0.75)	(-0.03)	(0.67)	(0.17)	(-1.56)	(-1.14)	(-1.55)	(-1.12)	(-1.23)	(-0.77)
Constant	0.043***	0.078	6.270***	2.441***	-0.008	0.213***	1.934***	5.179***	1.928***	5.177***	-0.093*	2.943***
	(5.36)	(1.36)	(240.12)	(17.42)	(-0.84)	(3.20)	(28.68)	(9.28)	(28.78)	(9.29)	(-1.91)	(12.68)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.73	.75	.98	.98	.61	.65	.75	.77	.75	.77	.33	.45
N	5,609	5,609	5,609	5,609	5,609	5,609	5,609	5,609	5,609	5,609	5,610	5,610

Panel B: Other firm variables

	log(Employment)		Leverage		CapEx		R&D	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × %Bound × Affected	-0.794**	-0.746**	-0.005	-0.009	-0.011	-0.014	-0.003	-0.016
	(-2.51)	(-2.15)	(-0.11)	(-0.20)	(-0.44)	(-0.60)	(-1.17)	(-1.25)
Post × Affected	0.179	0.203	-0.023	-0.011	0.058	0.050	0.001	-0.005
	(0.47)	(0.59)	(-0.76)	(-0.44)	(0.99)	(0.97)	(0.21)	(-0.45)
Post × %Bound	-0.035	-0.046	0.005	0.007	-0.006	-0.006	0.003	0.002
	(-1.02)	(-1.39)	(0.84)	(1.11)	(-1.31)	(-1.37)	(1.15)	(1.03)
%Bound × Affected	4.483***	4.298***	0.419***	0.411***	-0.155***	-0.146***	-0.001	0.031
	(4.06)	(3.99)	(9.02)	(7.56)	(-3.42)	(-3.51)	(-0.09)	(1.56)
Post	-0.036	-0.038	-0.005	-0.004	0.003	0.003	0.000	0.000
	(-1.44)	(-1.36)	(-0.48)	(-0.41)	(0.41)	(0.39)	(0.09)	(0.03)
%Bound	-0.335*	-0.366**	-0.034	-0.034	0.007	0.012	-0.000	0.009
	(-1.86)	(-2.14)	(-1.47)	(-1.44)	(0.77)	(1.08)	(-0.02)	(1.22)
Constant	6.148***	4.155***	0.192***	0.056	0.044***	0.275***	0.050***	0.232***
	(126.53)	(18.86)	(23.45)	(0.53)	(10.17)	(3.40)	(17.52)	(3.44)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	.94	.94	.8	.8	.62	.64	.82	.85
N	5,610	5,610	5,609	5,609	5,592	5,592	5,610	5,610

Table A.4: Matched sample – identification

This table presents the results of a clustered t-test of difference in means of top executive pay and firm characteristics 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 discussed in Section 7.2 of the text. 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 15.40% (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	1576.435	1210.735	365.7	1.366	.172
Cash pay	514.494	703.49	-188.996	-1.486	.138
Salary	375.821	356.375	19.446	.775	.439
Bonus	111.232	285.631	-174.399	-1.714	.087
Incentive pay	1521.691	1179.58	342.111	.838	.403
Size	6.688	6.643	.045	.319	.75
Profitability	.109	.112	-.004	-.353	.724
ROA	.051	.054	-.003	-.352	.725
Q	1.762	1.797	-.035	-.376	.707
M/B	1.76	1.795	-.035	-.377	.706
log(annual return)	.112	.113	0	-.015	.988
%Bound	4.038	63.282	-59.243	-29.313	0
Number of distinct firms	291	484	.	.	.

Table A.5: The effects of differences in observable firm characteristics and local economic conditions – robustness

The table presents additional estimates of firm-level main regressions defined in Equation 2b within the matched sample (column (1)), the bordering counties sample (column (2)), a sample excluding the financial crisis years 2008 - 2009 (columns (3)), and the main sample controlling for political ideology (column (4)). The main sample is outlined in Table 1. Matched sample and bordering counties sample construction is discussed in Sections 7.2 and 7.3, respectively. Post is an indicator equal to 1 for periods post-July 24, 2007 onwards (i.e., $t \in [0; 2]$). %Bound is equal to the share of employees located in bound (where federal MW is greater than or equal to state MW) states. 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. %Republican is equal to the share of employees located in Republican (won by the Republican party in the corresponding presidential election cycle) states. Control variables are winsorized at the 0.5% and 99.5% levels. Industries are defined by NAICS3 codes at t = -2. Industry by year, firm, state border, and top executive pay dataset 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.

	Matched	Bordering counties	Exclude financial crisis	Political ideology
	(1) log(1+total pay)	(2) log(1+total pay)	(3) log(1+total pay)	(4) log(1+total pay)
Post × %Bound × Affected	1.150***	0.773*	0.597*	0.671***
	(2.91)	(1.79)	(1.84)	(4.23)
Post × %Bound	-0.075	-0.159	-0.032	-0.088*
	(-0.83)	(-1.03)	(-0.34)	(-1.90)
Post × Affected	-1.349***	-0.238	-1.445***	-0.983*
	(-3.08)	(-0.53)	(-7.70)	(-1.92)
%Bound × Affected	1.275	6.167***	-0.869	0.367
	(1.40)	(4.28)	(-0.59)	(0.57)
Post	0.171	0.270	0.574***	0.205
	(0.91)	(1.10)	(3.32)	(1.41)
%Bound	-0.197	0.064	0.838*	0.240
	(-0.57)	(0.11)	(1.87)	(1.23)
%Republican				0.030
				(0.34)
Constant	5.355***	3.535***	5.091***	4.463***
	(8.87)	(5.07)	(9.18)	(10.01)
Controls	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State border FE	No	Yes	No	No
Dataset FE	Yes	Yes	Yes	Yes
Adj-R-squared	.58	.54	.45	.47
N	5,495	2,680	3,366	5,610

Internet Appendix

The Positive Effect of Employee Pay on Executive Pay

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IA.1. Constructing executive pay datasets

I use three executive compensation datasets to construct top executive pay variables: Capital IQ, MSCI, and Execucomp. I define the highest-paid executive in a given year to be the top executive (often the CEO, as documented by Bloom et al. (2021)). I require top executives to have positive total pay, following Matveyev (2017), since zero top executive pay observations do not convey meaningful information. I use the firm identifier and fiscal year to link top executive 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.

IA.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.

IA.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.

IA.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*.

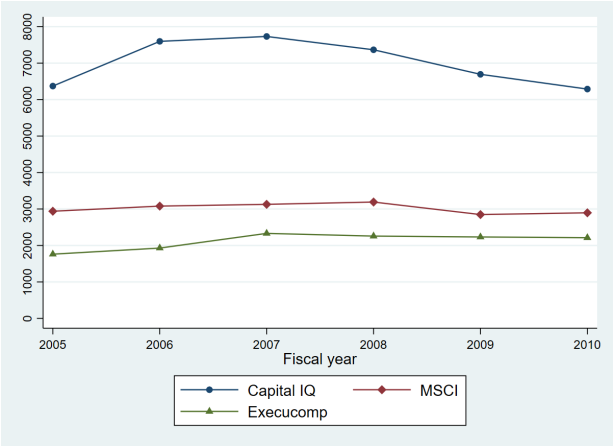
2. (a) I impute **bonus** to be equal to zero if **bonus** is missing and *TOTAL_CURR* is non-missing.

(b) I impute **salary** to be equal to zero if **salary** is missing and *TOTAL_CURR* is non-missing.

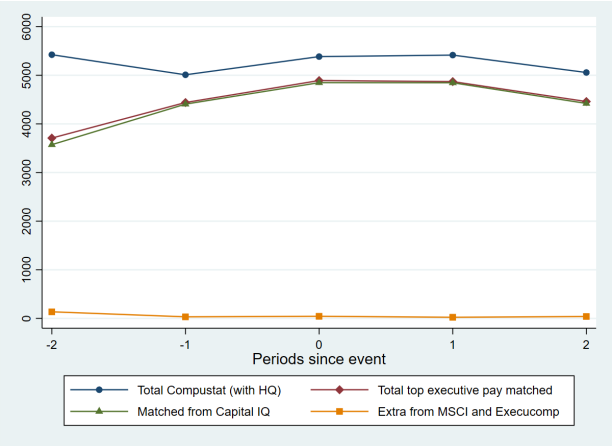
(c) I replace **bonus** = *TOTAL_CURR* - **salary** if **bonus** is missing.

(d) I replace **salary** = *TOTAL_CURR* - **bonus** if **salary** is missing.

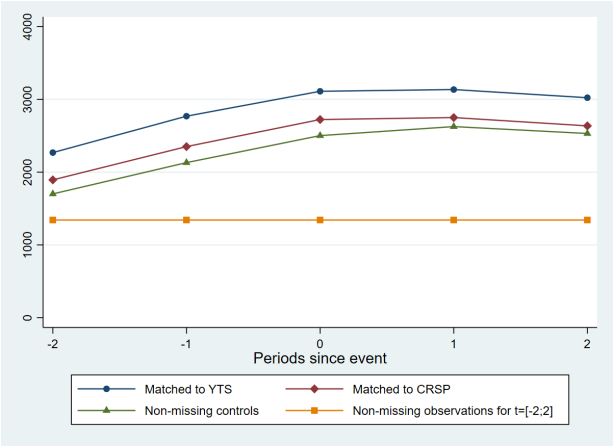
IA.2. Tables and figures



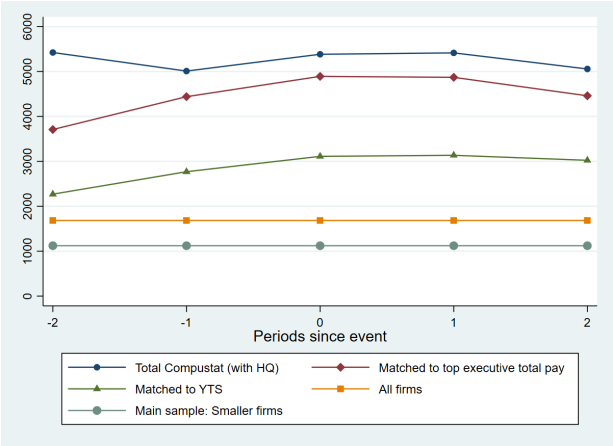
(a) Step 1: Top executive pay coverage



(b) Step 2: Match to top executive pay data



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



(d) Summary: Sample

Fig. IA.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 top executive 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 top executive pay datasets ("Total pay matched"), and the distribution of matched firms by dataset ("from Capital IQ" and "Extra from MSCI and Execucomp). 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).

Table IA.1: Composition of top executive pay data

This table displays the dataset distribution of top executive pay observations used in this study. Panel A presents the number of observations per period coming from Capital IQ, MSCI, and Execucomp. Panel B presents the correlations between these datasets for a subsample of firms that are covered in the intersection of Capital IQ, MSCI, and Execucomp. Data cleaning and homogeneous definitions of top executive total pay, salary, bonus, cash pay, and incentive pay for each of the three top executive pay datasets are discussed in Section IA.1.

Panel A: Number of observations per period

t	**Capital IQ**	**MSCI**	**Execucomp**
-2	929	176	17
-1	1,091	28	3
0	1.094	23	5
1	1,111	10	1
2	1,095	23	4
Total	1,122	1,122	1,122

Panel B: Correlations between top executive total pay across datasets

Database	**Capital IQ**	**MSCI**	**Execucomp**
Capital IQ	1.0000		
MSCI	0.4214	1.0000	
Execucomp	0.5384	0.7752	1.0000

Table IA.2: Summary statistics: Top executive-employee pay ratio

The table displays summary statistics for the top executive-employee pay ratio and its components, total top executive pay and average employee pay (in thousands). The sample is outlined in Table 1. Construction of the average employee pay is defined in Equation 5, and construction of the top executive-employee pay ratio is discussed in Section 6.4.1 of the text. Panels A and B show the distributions obtained after applying the imputation procedure by Donangelo (2016) and after defining the variables based on the disclosed labor expense *xlr*, respectively.

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

	Mean	Median	Std	5th	95th	N
Top executive total pay	965.847	423.326	2,135.901	38.649	3,490.668	3,828
Average employee pay	249.497	90.424	414.922	24.028	1,283.451	3,828
Top executive-employee pay ratio	9.076	4.067	14.442	0.233	35.816	3,828

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

	Mean	Median	Std	5th	95th	N
Top executive total pay	750.239	331.733	2,064.629	30.100	2,350.181	1,672
Average employee pay	119.857	51.246	360.316	23.261	407.755	1,672
Top executive-employee pay ratio	10.258	6.182	14.060	0.401	34.012	1,672