Corporate Governance and the Firm's Workforce

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Abstract

This paper uses matched employer-employee data to study the effects of corporate governance on the earnings and composition of the firm's workforce. I build a new dataset that links over 2,000 public companies to their employees in Texas. Focusing on shareholder-sponsored proposals, I measure stronger corporate governance using the passage of proposals to declassify the board of directors. I find that vote passage lowers firm's average employee earnings by 11%, directionally consistent with the previous literature. This has often been interpreted as wage decreases for individual workers. However, I show that half of this decrease is due to the changing composition of the workforce. Firms shift from higher-earning to lower-earning employees, and this effect is concentrated amongst the lower end of the earnings distribution. This evidence suggests that stronger corporate governance does not simply cause a wealth transfer from employees to shareholders. Instead, it causes real changes in the types of employees selected and retained by the firm.

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1 Introduction

How does corporate governance affect the firm's workforce? Answering this question is important for three main reasons. First, there is evidence that stronger corporate governance, in the form of more shareholder-friendly policies, often improves firm value. However, we know less about how this improvement comes about and what real changes within the firm are caused by corporate governance. Secondly, stronger corporate governance is most often interpreted as the resolution of an agency problem. Therefore understanding its effects reveals the differences between the goals of shareholders and managers, as they relate to the workforce. The third benefit to answering this question is that understanding the effects of corporate governance mechanisms on the workforce allows us to compare them to other types of interventions. If the effects of stronger corporate governance are similar to those of private equity buyouts, corporate governance policy can be a less invasive way to implement similar reforms.

Corporate governance has been shown to affect a wide range of firm decisions, from capital structure to risk-taking. Therefore it plausibly has the potential to materially change the compensation and structure of the workforce. The main empirical evidence on the relationship between corporate governance and employees is provided by Bertrand and Mullainathan (2003). The authors show that stronger corporate governance is associated with lower wages.[1] Using plant-level workforce data, they find that average wages rise by 1% after governance weakens. This finding suggests that agency problems lead to higher employee wages. Indeed, several studies have shown that average wages fall following other interventions that might limit an agency problem (Brav et al., 2013; Lichtenberg and Siegel, 1990a).

These results that link corporate governance to average wages have contributed greatly to our understanding of how governance leads to real changes within the firm. The fact that weaker governance leads to higher wages is commonly interpreted as evidence of the "quiet

[1]They use changes in state level anti-takeover laws to measure the effects of a plausibly exogenous weakening of corporate governance.

life". That is, in the absence of strong shareholder oversight, managers limit their effort and psychic cost. This is in line with Jensen and Meckling (1976), who suggest that managers obtain significant non-pecuniary benefits through their relationship with their employees. However, this interpretation has also raised a new question. What is actually happening within the workforce to cause these higher wages? There are two possible explanations.

The first is that when governance is weaker managers pay more to the same, or similar, workers. The second explanation is that managers choose to employ different, more expensive, workers. To see the link between managers and both of these outcomes, one need look no farther than the high-profile hiring of Marissa Mayer as the CEO of Yahoo. Mayer, who has come under fire from major shareholders recently, changed both the compensation and selections of Yahoo's workforce when she joined the firm. She invested in employee perks such as better phones, better food, and even nicer bathrooms. However, she also instituted a performance review system that ranked employees within groups, forcing relative rather than absolute evaluations.[2] Although managers are clearly able to affect both compensation and selection, their approaches have quite different implications for the effects of corporate governance.

The first interpretation implies that managers choose to pay higher rents to workers when they are subject to less governance. This could be due to a desire to remain popular, entrench themselves within the firm (Pagano and Volpin, 2005; Atanassov and Kim, 2009), or focus on long-term contracts (Shleifer and Summers, 1988). Regardless, this implies that stronger corporate governance lowers wages by transferring wealth from employees to shareholders. This is true whether individual employees are paid less or employees are replaced by equivalent employees that are paid less. The implication is that the value gain to shareholders from stronger governance may be a financial effect, moving profits from one stakeholder to another. So far, the literature has focused on this explanation for the link between governance and wages.

[2]Both of these approaches are described in an article in The New York Times Magazine by Carlson (2014).

The second interpretation is that managers choose to hire more expensive employees when they are weakly governed. As suggested by Bertrand and Mullainathan (2003), managers may want to work with higher quality employees. Alternatively, they may institute different employee screening, employee training, or even corporate culture than preferred by shareholders. Indeed, stronger governance has been show to make corporate culture more profit-focused (Popadak, 2013). In this case, stronger governance creates value through a real effect on firm organization. The key distinction between this channel and the previous one is that if the rents paid to employees do not change, but rather employees are redistributed across firms, then gains to shareholder value do not come at the expense of employee earnings. In other words, they do not simply capture the financial effect of a wealth transfer.

A key hurdle in empirically disentangling the two explanations provided above is the lack of employee-level data. Analysis has mostly focused on plant-level wage data.[3] Without data on individual employees, it is impossible to determine whether the observed changes in average wages are due to changes in the wages of individual workers or changes in the composition of the workforce. Employee-level data makes it possible to determine the relative strength of these two effects linking corporate governance and wages. This in turn allows us to decompose the relationship into a financial effect and a real effect.

I create a new dataset to overcome this hurdle and identify the mechanism by which earnings decrease. I link over 2000 publicly traded US firms to quarterly earnings data for all workers employed in Texas from 1997 through 2012. This data is collected by the state in order to administer unemployment insurance benefits and it was provided to me by the Texas Workforce Commission. Using this constructed dataset, I am able to observe changes in average earnings following changes in corporate governance, and to decompose these changes into an individual earnings effect and a composition effect. I am able to follow individual employees over time, as long as they remain in the Texas workforce. This allows

[3]One exception is Cronqvist et al. (2009), who use employer-employee matched data from Sweden to confirm that stronger governance is associated with lower wages. Results may be particular to Swedish labor markets, which are quite different from those of the US, and focus on endogenous differences in a cross-section of firms.

me to identify turnover within the firm, as well as address the selection into and out of the firm.

When measuring corporate governance, I focus on board declassification. A declassified board of directors is one in which all directors are reelected annually. This is in contrast to classified, or staggered, boards in which only a fraction of the board is up for reelection in any given year. A declassified board allows shareholders to more quickly and easily replace board members. This considerably strengthens their control over the board's composition and thereby the firm's decisions.

Because board declassification, like all corporate governance measures, is highly endogenous, I focus a sample of plausibly similar firms. I study shareholder votes on non-binding proposals to declassify the board of directors.[4] Although these proposals are non-binding, they lead to significant changes in actual board declassification. My empirical strategy is to use a flexible difference-in-difference design to compare firms in which these votes passed to a natural control group, those firms in which the votes did not pass.

In the first part of the empirical analysis, I verify that the sample of employees matched to firms shows no evidence of selection bias related to corporate governance, even though the matching process is fuzzy. I also address external validity by showing that many large corporations in a variety of industries have an employee presence in Texas. I then address internal validity and show that firms are comparable on a variety of dimensions prior to differential vote outcomes, and highlight where they may differ. Next, I confirm that vote outcomes do have a substantial effects on firm policies. Firms in which the vote passes are 29 percentage points more likely to have a declassified board four years later.

Having established the relevance of the sample and the importance of votes to declassify, I turn to my main analysis: examining the effects of board declassification on employee earnings. First, I show that the passage of a vote to declassify leads to lower earnings on average, consistent with the results of Bertrand and Mullainathan (2003). When including

[4]Votes on shareholder-sponsored proposals are also used to measure the effects of corporate governance by Cuñat et al. (2012) and Popadak (2013), but they focus on a broader range of proposal topics.

firm fixed effects, analogous to their plant fixed effects, but not employee fixed effects, I find an 11% decrease in earnings during the four years after the vote. This is a directionally similar but much larger than the 1% effect of Bertrand and Mullainathan (2003).

I then move beyond the firm-level analysis by introducing employee fixed effects. These fixed effects control for the average wage that each employee earns over their time in the sample. After including these controls, I find that earnings after the vote are only 5% lower than before the vote and this difference is not statistically significant. This means that the falling average earnings are not solely driven by lowering the earnings of existing workers or replacing them with similar workers. Instead, at least half of the effect is due to the changing composition of the workforce. Stronger corporate governance leads firms to hire workers that are lower paid on average.

To better understand the shifting composition of the workforce, I run a series of analyses to determine the types of employees that are affected. I find that earnings fall most for employees in the lower part of the earnings distribution within each firm. This differs from the findings in Bertrand and Mullainathan (2003) and Lichtenberg and Siegel (1990a,b) that the wage changes are more drastic for white-collar workers. In contrast, I find only a small decrease in earnings for the employees near the top of the distribution. So employee selection leads to more variance in the distribution of earnings at the firm.

I then directly analyze the turnover of the workforce. I find that the number of employees joining and leaving the firm does not change systematically after the vote, although there is a brief spike in the number of people leaving in the year after vote passage. The changes in earnings are not caused by mass layoffs but by a more targeted employee selection and retention process. Indeed, the employees that leave the firm are different from those that join. Those leaving the firm are generally able to find jobs in Texas and actually experience a relative increase in earnings to other movers. In contrast, those joining the firm seem to be more stable, lower-trajectory employees. They have been with their previous employer longer than usual and they do not experience an outsized wage hike when they change jobs. These

results reinforce that the shifting workforce is driven by changes in the types of employees joining and leaving the firm.

To determine what might drive the strength of the composition effect of employees in my sample, I separately estimate the effects of vote passage on firms that are headquartered in Texas and those that are not. Both types of firms show a decline in wages but those in Texas achieve this by decreasing individual employee compensation, while the others rely solely on the changing composition of the workforce. This difference suggests that although managers may choose to pay employees more when they are close to them, consistent with evidence from Landier et al. (2009) that firms are reluctant to fire employees closer to headquarters, this effect is less important for large publicly traded firms that have employees in many disparate locations.

My results suggests that strengthening corporate governance does not simply generate a wealth transfer from employees to shareholders but also materially changes the selection and retention of workers. In addition to shedding light on what exactly the managerial "quiet life" means for employees and for the firm, this allows us to compare strengthening corporate governance to more drastic interventions. The reorganization of the workforce is reminiscent of that following private equity buyouts, suggesting that improvements in corporate governance may partially replicate the effects of privatization.

My paper proceeds as follows. In Section 2, I review the relevant literature. In Section 3, I introduce my data sources and key measures, and verify that there does not seem to be biased selection into my sample. In Section 4, I present my empirical strategy, using votes on shareholder-sponsored proposals. In Section 5, I discuss my main results, the effects of corporate governance on the firm's workforce. I conclude in Section 6.

2 Literature Review

Interventions that align management more closely with shareholders have been shown to lead to increases in firm value. Private equity buyouts are followed by excess returns and improved operating performance (Kaplan, 1989), leveraged buyouts result in increased profitability and growth (Boucly et al., 2011; Lichtenberg and Siegel, 1990b), and shareholder activism ends in significant and permanent excess returns on average (Brav et al., 2008). Corporate governance has also been linked to superior performance, both in the cross section of firms (Bebchuk et al., 2009; Cremers and Nair, 2005; Gompers et al., 2003), and following plausibly exogenous improvements in governance (Cuñat et al., 2012). The link between governance and firm performance is especially true in competitive industries (Giroud and Mueller, 2010). However, there are a wide range of estimates to come out of this literature, and it is difficult to pinpoint how exactly firms are affected by stronger corporate governance.

To better understand the effects of corporate governance on firms, there has been a push to identify what exactly changes within the firm. For example, stronger corporate governance leads to increased leverage (Berger et al., 1997) and decreased acquisitions and capital expenditure (Cuñat et al., 2012). There is mixed evidence on the directional change in cash reserves (Harford et al., 2008; Calomiris and Carlson, 2014) but the firm's cash does become more valuable (Dittmar and Mahrt-Smith, 2007). Governance also alters managerial risk appetite, with stronger governance leading managers to take on more firm risk (Gormley and Matsa, 2014; Ferreira and Laux, 2007; John et al., 2008). The use of detailed firm-level data has been especially useful in measuring the effects of corporate governance. Bertrand and Mullainathan (2003) use plant-level data to estimate how anti-takeover legislation affects plant-level wages, productivity, and investment.

Although Bertrand and Mullainathan (2003) present the main contribution regarding corporate governance and employment, several other studies have addressed similar questions. Cronqvist et al. (2009) show that in the cross-section in Sweden, stronger governance is associated with lower wages. More drastic interventions have also been found to prompt

workforce reforms. Private equity firms have stronger people management practices through better hiring, firing, pay and promotions (Bloom et al., 2009). Private equity buyouts have also been associated with significant workforce changes such as high turnover (Davis et al., 2011) and differential human capital investment (Agrawal and Tambe, 2013), as well as restructuring activity in general (Bharath et al., 2014).

Another strand of research has focused more specifically on the effects of declassified boards. Board declassification has been shown to lead to real changes managerial power, exposing managers to market discipline (Faleye, 2007) and increasing the probability of a successful takeover (Bebchuk et al., 2002; Bates et al., 2008). These increases in managerial accountability also translate into higher firm value. Classified boards, which represent weaker corporate governance, decrease shareholder returns and firm value (Bebchuk and Cohen, 2005; Bebchuk et al., 2002; Faleye, 2007). Although government mandates requiring declassified boards are value decreasing (Larcker et al., 2011), this is not true for firms that end up declassifying the board without regulatory intervention.

Declassification also has significant effects on other firm policies. Firms with declassified boards tend to invest more in R&D and other company-specific capital assets (Faleye, 2009). Employee ownership is higher when board are declassified, presumably because their ownership serves as an alternate takeover deterrent (Rauh, 2006). However, I am not aware of any research that has linked declassified boards to the wages and composition of the workforce.

3 Data and Key Measures

In this section I discuss declassified boards as a measure of strong corporate governance. I then introduce the dataset and describe why it allows me to conduct analysis not previously possible. Finally, I address the potential issue of sample selection and argue that the dataset is appropriate for studying the effects of corporate governance on employees.

3.1 Declassified Boards

I focus on the specific corporate governance mechanism of board declassification for four main reasons. The first is that, from a practical perspective, board declassification is the key policy in granting power to shareholders. When boards are classified, shareholders are unable to change the majority of the board at any given time. This makes it more difficult to elect a board that is willing to enact shareholder's wishes. In contrast, a declassified board grants shareholders more control over the composition of the board of directors, and thereby the firm.

Although there are a number of other popular governance mechanisms, Klausner (2013) demonstrates that all other corporate governance policies are relatively easy to change if the board is willing to do so. The presence of a poison pill, a popular antitakeover defense, is often used as a measure of weak corporate governance. However, boards can adopt a poison pill at any time. Another common measure of corporate governance is liberal voting rights for shareholders. However, strong voting rights are of little use in Director elections if those directors are not up for reelection to begin with. At the end of the day, the main determinant of governance seems to be the ability of shareholders to elect a sympathetic board of directors and this depends on whether or not the board is declassified.

The second benefit of board declassification is that it has been an immensely popular policy over the past decade; it has been the preferred corporate governance mechanism for shareholders. A variety of investors have pushed for board declassification in recent years: pension funds, mutual funds, individual investors and activist hedge funds. Harvard's Shareholder Rights Project has emphasized board declassification as a best practice for strong corporate governance. From 1987 through 1994, board declassification was the most popular shareholder-sponsored proposal (Gillan and Starks, 2000) and it has only grown in popularity since. Whereas 48% of S&P500 companies had declassified boards in 2005, this increased to 89% at the end of 2013.[5] These trends demonstrate that board classification

[5] As reported by the law firm Fried, Frank, Harris, Shriver & Jacobson LLP.

is far from static and that as investors push to strengthen corporate governance, more and more firms declassify their boards.

Despite the growing importance of board declassification, most corporate governance research has focused on different or broader measures of governance. So the third reason for my focus on board declassification is that the analysis has clear ties to a popular policy. In previous research, the most popular measure of corporate governance has been the governance index created by Gompers et al. (2003). This index sums indicators for twenty-four different policies that empower managers at the expense of shareholders. Although this index is thorough, this measure assigns equal weight to all policies, making it difficult to understand the mechanism at work. By limiting my analysis to board declassification, I narrow the scope of my analysis but sharpen its focus.

The fourth and final reason for my focus on board declassification is that I am able to use a source of variation that is less prone to omitted variable bias than endogenous variation. I study non-binding votes on shareholder-sponsored proposals to declassify the board of directors. The passage of these proposals results in significant changes in actual board declassification. For proposals regarding other corporate governance measures, vote outcomes are not as tightly linked to policy implementation in my sample. This limits my ability to identify plausibly similar firms that end up with different implementations of other corporate governance mechanisms. I explore this empirical strategy in more depth in Section 4.

3.2 Employer-Employee Matched Data

In order to study the effect of corporate governance on individual employee earnings, it is necessary to create a matched employer-employee dataset. This makes it possible to follow both firms and employees over time, as well as following employees across different firms. Data on individual employees makes it possible to compute changes in individual earnings. This in turn allows for the decomposition of changes in average earnings into

changes in individual earnings and changes in the composition of the workforce. Panel data on individual employees also provides information on turnover and tenure, making it possible to identify the types of employees that enter and Exit the firm.

To create a dataset that meets these needs, I combine data from two main sources. I first use company level data to obtain financial and corporate governance records for all publicly held firms in the US. Financial variables come from Compustat. Data on corporate governance policies, such as classified boards, comes from the IRRC (Investor Responsibility Research Center) Governance database and is available for the years 1990-2007. These two sources combined form the firm-level data for my analysis.

The second main dataset provides employee level data. This is provided by the Texas Workforce Commission, a state agency tasked with promoting and supporting the state's workforce. The data is drawn from the the unemployment insurance (UI) records of the state of Texas.[6] In order to calculate and administer UI claims, the state must know the quarterly earnings of all eligible employees. Therefore these records cover all employees in Texas and provide quarterly data on individual earnings from 1997 through 2012, as well as an employee identifier that allows for the observation of the same employee over time. This is the state-level data that is used to report wages to the Census Bureau.

The quarterly earnings include all employee wages. This include bonuses, stock options, severance pay, profit distributions, cash value of meals and lodging, tips and other gratuities. In each quarter, earnings reflect both the wages of the employee and the amount of time that employee has worked for the firm. I therefore limit my sample to those employees that only have one employer in a single quarter. This excludes employees that are working multiple part-time jobs, as well as those employees that are transitioning between two firms, making the quarterly earnings more likely to be representative of hourly wages or salary.

Although limited to the state of Texas, I argue that my sample of employees constitutes an employee sample that is comparable to similar datasets. Texas has a population of 26.4

[6]I am able access this data as a visiting researcher at the Ray Marshall Center, a research center affiliated with the Lyndon B. Johnson School of Public Affairs of The University of Texas at Austin.

million people, roughly 8% of the total US population. Previous US employer-employee matched datasets have relied on the Census's Longitudinal Business Database (LBD) and Longitudinal Employer-Household Dynamics (LEHD), through which only a handful of states usually make their data available. As the second most populous state in the country, the Texas sample is not much smaller than many of these other employee samples. It is also true that the Texas workforce is similar to that of the US overall. Sixty-three percent of employees are blue-collar, close to the national average of 61%.[7] While employees in Texas are less likely to be unionized, this makes Texas a better setting for studying changes in individual wages because employers have more leeway to shift employee earnings.[8] All of these factors make the employee sample of Texas roughly comparable to that of the US in general.

In order to link these two datasets I use four data sources in two steps. The first step is to link firms to the names of their subsidiaries and establishments. The second links these names to publicly available federal EINs (Employer Identification Numbers). These EINs can in turn be linked to the UI employee level data.

To determine subsidiary names, I first link historical company names from CRSP to establishment names from the ReferenceUSA database. This is a private data collection effort that documents the names, locations, and parent-subsidiary relationships between businesses for 1997-2007. This dataset is created by combining over 5,000 public sources and calling and interviewing all locations on file. I supplement these relationship links with the SDC database of mergers and acquisitions to ensure that I record the correct parent company for every business. The result is 5,367 firms that correspond to 343,849 business names over these ten years.

The final step is to link these business names to EINs. Employer Identification Numbers are assigned by the IRS and many firms have more than one EIN, especially the larger

[7]Data comes from the Henry J. Kaiser Family Foundation

[8]In 2012, 3.5% of private employees in Texas were unionized, while 6.7% of private employees were unionized nationwide. Data is from the outgoing rotation group of the Current Population Survey (CPS).

public firms that make up my sample. To obtain EINs, I use the business names of each establishment to search two sites maintained by the Texas Comptroller of Public Accounts. These sites contain records of all entities in Texas subject to the franchise tax or the sales and use tax. Taxable entities for these taxes include any corporations that are chartered in, do business in, or sell or lease property or services in Texas. This covers a wide range of firms. Therefore even though not all business EINs are available on these sites, I am able to find at least one EIN for 4,176 firms.

The final result is a employer-employee matched dataset that combines firm financial and corporate governance data to the quarterly earnings of its employees, covering 6.8 million unique employees linked to 2,246 firms from 1997 through 2012. Descriptive statistics are displayed in Table 1. From the # Quarters we see that the average firm that matches to employees is in the sample for 23.6 quarters, almost six years. The firms are roughly split between strong and weak corporate governance, 44.2% have a Declassified Board.[9] The firms in the sample are publicly traded firms and therefore generally large, with an average of 27,260 employees (as measured by Compustat) in total. On average, the number of employees that match to the Texas data is 13.6% of the total number of employees reported in Compustat (displayed as Fraction Employees Matched). This percentage is to be expected because most firms do not hire only within Texas. However, the ratio of to total employees increases to 42.6% for the 12% of firms that are headquartered in Texas (Texas HQ). Reassuringly, this is exactly the trend we would expect to see in the data if firms were successfully matched to all of their Texas employees.

3.3 Factors Driving the Employer-Employee Match

Given the multiple steps involved in compiling this dataset, it is important to check whether there seems to be sample selection that might bias the results of analysis. In particular, the probability that a firm is matched to employees in Texas should not be related to the

[9]When weighted by the number of employees in the firm, the percentage is similar; 42.6% of employees are in a firm with a declassified board

explanatory variable, the presence of a declassified board. If a firm is differentially likely to match to employees when its board is declassified, there may be data attrition following board declassification. It would then be incorrect to attribute the observed changes in employment and earnings, truly due to selection, to the changing corporate governance.

To address the issue of differential selection into the sample, Table 2 shows how match probabilities vary with firm attributes. In the first two columns the dependent variable *Matched* is an indicator for whether a firm-quarter observation matches to at least one Texas employee in the data. The regression displayed in first column estimates a linear probability model. The mean displayed in the last row of the table shows that on average 49.3% of the firm-quarters in the sample match to an employee in Texas. The results in the first column control for industry fixed effects, using 3-digit NAICS codes. The main takeaway from this table is that *Declassified*, an indicator for a declassified board, has no impact on the probability of matching to employees. Therefore it does not seem that selection into the sample is related to corporate governance.

Other firm characteristics do affect selection into the sample, and these coefficients go in the expected directions. When a firm is headquartered in Texas, the variable Texas HQ is equal to one, and the firm is 13.4 percentage points more likely to match to employees in Texas. This is intuitive because firms that are headquartered in Texas are also more likely to have employees located within the state. The coefficient on ln(Assets), which represents firm size, is not significant.[10] However, Firm Age has a positive and significant effect on the probability of a match. A firm that has been in the Compustat sample for 10 more years is one percentage point more likely to match to employees in Texas. This is to be expected because firms with a longer history are more likely to be in all of the different datasets that I use to create the links between employers and employees. There are also positive relationships between Matched and both ln(Capex) and ln(Liabilities).[11]

In the second column I include firm fixed effects to determine how time-varying firm

[10] Assets are measured in millions of dollars and represents total assets.

[11] Both capital expenditure and total liabilities are measured in millions of dollars

characteristics affect the probability of matching to the data. Again, Declassified Board is small and not significant. In this specification the only significant coefficient is that on ln(Assets), which is significant at the 0.1% level. It implies that a one standard deviation increase in firm size leads to an 8.6 percentage point increase in the probability of matching to the sample. This indicates that as firms grow larger they are more likely to match to employees in Texas. One would expect this pattern to emerge if firms increase the total number of employees as they grow, also increasing the number of employees in Texas. In short, the factors that driving match probability are those that would be expected to drive whether or not a firm has employees in Texas.

In the last two columns, I limit the sample to firm-quarters that matched the employee data and examine the proportion of employees that were matched. The dependent variable, $\%employees$, is the proportion of employees matched to a firm in the Texas data, relative to Compustat employees, for each firm-quarter. The mean in the last row shows that in the average firm-quarter, the Texas employees represent 11.6% of Compustat employees. As in the previous columns, Declassified Board does not have a significant effect on the outcome. This is true when using either industry or firm fixed effects. Indeed, the only statistically significant coefficient is on Texas HQ. Firms headquartered in Texas match another 16.4% of their total employees to the Texas data and this is significant at the 0.1% level. This result is consistent with the assumption that firms headquartered in Texas will also have more of their employees located in Texas. The fact that no other firm characteristics are significant, either with or without firm fixed effects, implies that standard time-varying firm attributes do not influence the percentage of employees matched to the data.

To further investigate any potential differences between the firms that match and those that do not, Table 3 shows the industries of these firms. The first two columns show the fraction of firm-quarters that belong to every industry, as a portion of all firm-quarters that did not match to the data. The second two columns show the same for all firm-quarters that did match to the data. For brevity, only the eight most common industries in the sample

are shown. Generally the means are very similar for matched and not matched firms. The one exception is in finance. Firms that did not match to the data were more likely to be financial firms. This is to be expected if financial firms are less likely to have geographically disperse employees, as this decrease their likelihood of having employees in Texas.

In all of the above specifications, the match of firms to employees is shown to rely only on the factors that might reasonably be expected to drive the actual number of the firm's employees in Texas, or the probability that the firm reports employee data to the UI office in Texas. There is no evidence that selection into the sample is influenced by corporate governance. Thus the constructed dataset has the properties expected from an unbiased sample, which matches each employer to all of their Texas employees.

4 Empirical Strategy

In this section I explain my main empirical strategies. I provide an estimate of the effect of corporate governance by conducting a difference-in-difference analysis. This analysis uses votes on shareholder-sponsored proposals to classify the board of directors. In this section I explain the process by which shareholder-sponsored votes are passed and show that they lead to real changes within the firm.

4.0.1 Votes on Proposals to Declassify

In general, those firms that choose to declassify their board of directors maybe very different from those that do not choose to do so. They may have a greater incentive to protect shareholder rights or strengthen shareholder oversight. For this reason, it may be problematic to use all endogenous variation in board declassification to estimate its causal effects.

Board declassification can come about in two main ways. The first is that the board itself can choose to declassify. For example, this may happen as a result of private conversations with hedge fund activists, as a concession to an overall push for stronger governance. The

second manner in which boards may become declassified is following a shareholder vote on a proposal to declassify. These proposals may be put forward by either management or a shareholder. Once proposed, they appear on the proxy statement and are voted on at the annual general meeting of shareholders. Investor participation is generally high in these votes, 70% percent of street name shares were voted in 2013, mostly by institutional investors.[12] If the votes for the proposal pass a firm-specific threshold, usually 50%, then the proposal passes.

Although these votes are non-binding, they do put pressure on management to subsequently declassify the board of directors. If management does not choose to follow the recommendation of the shareholders, there are a number of actions they can take to embarrass the firm and further pressure management. Shareholders may withhold votes at the next director's election in order to voice their discontent. Such votes are generally successful in convincing the board to make the desired changes (Yermack, 2010; Guercio et al., 2008). In light of these possible reactions, non-binding proposals are often an effective way to declassify the board of directors.

The ability of shareholders to embarrass boards into action is exemplified by the experience of Barnes Group Inc, a manufacturer of industrial and aerospace components based in Connecticut. Shareholders passed proposals to declassify the board of directors at the annual general meeting in 2010 and 2011. However, the board refused to implement annual elections for directors. As a result, two major proxy advisory firms, Institutional Shareholder Services and Glass Lewis & Co, recommended that shareholders reject all four directors up for election. Two of the four board members were rejected at the annual meeting in 2012. Both directors were able to keep their seats, because the firm did not have a mechanism in place for the replacement of those directors. However, the board decided to recommend declassification in October 2012. The measure was successfully passed in May 2013, and declassification was phased in beginning in 2014. This example highlights the process by

[12]Data comes from a report from The Corporate Secretary on June 5, 2013.

which shareholder votes can translate into policy changes even when all shareholder actions are non-binding.[13]

The use of proposals to enact changes in board classification offer an opportunity to use variation that is less endogenous than that offered by the full sample of firms. Companies in which shareholder propose board declassification are a select sample and therefore more likely to be comparable. It is clear that at least a portion of shareholders believe that a declassified board would benefit the firm. By comparing the firms that passed the vote to those that did not, I use a sample of plausibly similar firms to estimate the effects of the shareholder's intent to treat a firm with board declassification. The identifying assumption necessary for my analysis is that in the absence of different vote results, all firms with shareholder-sponsored proposals would have evolved similarly.

To study the outcomes of these proposals, I use data on votes at shareholder meetings taken from three sources: ISS Voting Analytics, Riskmetrics Voting Results Data, and Riskmetrics Shareholder Proposal Data. I combine these to create a comprehensive list of proposals from 1997 through 2011. For each proposal, I know the content and date of the proposal, the percentage of votes in favor of the proposal, the passage threshold for that firm, and some information about who proposed it.

I choose to focus on shareholder-sponsored proposals in order to avoid selection bias. Although managers can propose declassification and indeed there are many proposals introduced by management, there is considerable selection into the sample. Because managers are able to, and often do, remove proposals that do not seem likely to pass, passed proposals are not comparable to failed proposals (Cuñat et al., 2012). On the other hand, shareholders do not remove proposals that seem likely to fail. Therefore there is no selection into the sample of shareholder-sponsored proposals around the passage threshold.

I further restrict my sample to focus only on firms in which there was plausibly some uncertainty about whether or not the vote would pass. I use only those proposals that passed

[13]These events are culled from new reports over the course of several years.

or failed by at most 30 percentage points. Therefore if the passage threshold for a firm was 50% of shareholder votes, I would only include it in my sample if it received greater than 20% or less than 80%.[14] This excludes the firms in which the shareholder proposal was extremely likely to pass or fail, eliminating the firms that were most likely to differ ex ante. Because most votes are not like to pass or fail with huge margins, this restriction only shrinks the sample by 22%, from 869 votes to 680. Excluding these extreme firms limits the sample to those firms that are even more likely to be comparable prior to the vote.

In the sample, proposals to declassify receive support from a wide variety of investors. In 56% of the votes to declassify I can identify the type of shareholder that proposed the vote. In two thirds of these cases, the vote was proposed by an individual investor (including wealthy individuals and some hedge fund managers). Public pensions propose 14% of these votes and unions propose 13%. Therefore ignoring calls for declassification could disappoint a variety of investors.

After matching to the firm and employee data, only 333 of these proposals remain.[15] These are the votes that I use to identify the effects of corporate governance in the remainder of the paper. Of these 333 votes, 76% earn enough votes to pass the required threshold, reflecting the general popularity of this proposal. These votes results in a steep increase in the likelihood of actually having a declassified board, as shown in Figure 1. The graph compares firms that passed the vote to those that did not. The dark blue line represents the former and the light blue line the latter. The x-axis measures the year relative to the year in which shareholders voted on the proposal. The y-axis measure the fraction of firms with declassified boards in every year for the two types of firms. Clearly neither type of firm had a declassified board prior to the proposal to declassify. However, those firms that passed the proposal diverge from those that did not in the four years following the vote. Four years after the vote, 61% of the firms that passed the vote have a declassified board. In contrast,

[14]Not all firms have a threshold of 50%, although most do. This varies slightly from firm to firm and creates some variation in passage even when the percentage of votes for the proposal is the same.

[15]If the same firm experiences multiple votes I use only those votes that were preceded by at least four years of no votes to declassify.

only 29% of firm boards are declassified among those firms in which the vote failed. This differential response to passed and failed proposals verifies that these non-binding proposals still have bite.

To demonstrate the effects of vote passage in a more detailed manner, Figure 2 plots the probability of having a classified board for companies around the passage threshold. Each graph plots board classification for firms that voted on a proposal to declassify the board of directors. The x-axis shows the number of percentage points that the vote gained, relative to the threshold necessary to pass the vote. So if a certain firm requires a 50% vote to pass a proposal and the proposal to declassify received 70% of the vote, it would be included in the point marked as 20 on the x-axis. Observations are grouped into bins of size 2, so each point represents all firms within 2 percentage points of the x-axis value. The y-axis measures the proportion of firms in each bin that have a declassified board.

The figure in the top left shows firms on either side of the passage threshold two years before the vote is proposed. Almost all firms have classified boards, as would be expected.[16] The top right figure shows the distribution of board classification in the period of the vote. Firms to the right of the threshold have already begun to declassify their boards, although they are only slightly more likely to have a declassified board. The bottom left figure plots board declassification two years after the vote. Here it is clear that firms to the left of the cutoff have remained classified but those to the right have declassified in many cases. Finally, the bottom right figure shows results four years after the vote. Although firms to the left of the cutoff have begun to declassify as well, the trend is much stronger on the right side.

Several other papers have used shareholder-sponsored proposals in a regression discontinuity design. However, the graphs in Figure 2 support the use of of a difference-in-difference analysis in this case. The firms that lie just to the right of the passage threshold do not experience a change in classification. Rather, the change is most pronounced in the firms

[16]The fact that not all of the firms have declassified boards comes from discrepancies in information and timing between the dataset on corporate governance policies (from the IRRC) and the dataset on shareholder votes.

that are farther away from the threshold. Therefore I proceed with a flexible difference in difference design.

4.0.2 Validity of Difference-in-Difference Design

Although I narrow the sample to identify plausibly similar firms, there may still be sources of endogenous variation that drive differences between the firms that pass the vote and those that do not. In order to test these difference, in Table 4 I estimate the linear probability between vote passage and firm characteristics in the period of the vote. There do not seem to be any significant differences in terms of firm age, size, profitability, or number of employees. However, the ratio of institutional owners (IO) is statistically significant and suggests that firms with 10% more of their shares held by institutions are 6 percentage points more likely to pass the vote. This is consistent with the widespread popularity of board declassification among proxy advisory firms used by institutional shareholders to guide voting behavior. For all board declassification proposals studied, proxy advisory firms recommended voting in favor of passage.

Surprisingly, there does not seem to be a strong relationship between vote passage and existing governance regulations. The relationship between vote passage and the governance index of Gompers et al. (2003) is negative, suggesting that when governance is weaker (the index is higher, out of a possible 24 points), the vote is less likely to pass. However, this relationship is small and economically not significant. The trends described above are consistent with the idea that vote passage is driven by the prevalence of investors that see board declassification as a best practice in governance, rather than by other firm characteristics. Of course there is no way to rule out all other potential sources of endogenous variation, but the patterns are consistent with a set of firms that are similar apart from vote passage outcomes.

In order to formally estimate the effects of board classification, I compare firms in which the shareholder-sponsors proposals passed to those in which they failed. For all firms in

which there was a vote on a proposal to declassify, I only include observation starting four years before the vote and ending four years after. I exclude observations from these firms in all other years. I focus on the four years before and after the vote in order to balance attrition from the sample with studying long-term effects.

Following the firms both before and after the sample allows me to do two things. The first is to verify that there are no significant differences between the firms prior to vote passage. The identifying assumption of my analysis is that in the absence of differential vote outcomes, firms in which the vote passed would have evolved similarly to those in which the vote did not pass. If there were differential pre-trends in outcome variables prior to the vote, it is less likely that the firms were comparable ex-ante. Then differences after the vote might also be due to these trends. By verifying the similarity of firms across the passage threshold prior to the vote, I establish that the differences following the vote can be plausibly attributed to the passage of proposals to declassify the board.

The second benefit of this approach is that it offers a detailed view of how firms begin to differ after the passage of the proposal, and shows the timing of those differences. Given the graphs in Figure 1, it is unlikely that changes within the firm are instantaneous. Rather, they are likely to unfold over the course of a few years. Even though the vote to declassify might send an instantaneous signal to management, it is likely that large-scale changes in the organization of the workforce might take longer to put into practice.[17]

The exact specification that I use is given by

$$y_{ijst} = (\beta_t + \gamma_t Vote\ Passed_j)\mathbb{1}(t) + x_{ijs} + \alpha_s + \alpha_j + \epsilon_{ijst} \tag{1}$$

The variable of interest y_{ijst} takes on a range of outcome variables, most notably board classification $Declassified_{jst}$ and (log) quarterly earnings $ln(Earnings)_{ijst}$. The subscript i denotes the individual, j denotes the firm, s denotes the calendar time, and t denotes the

[17]Indeed, although Cuñat et al. (2012) find instantaneous abnormal stock market returns, the changes that they document in acquisitions, capital expenditure, and book-to-market emerge up to four years after the vote.

number of years relative to the quarter of the vote. The variable t ranges from four years before the vote (-4) to four years after (4). Then $\mathbb{1}(t)$ is an indicator for observations t years after the vote, $Vote\ Passed_j$ is an indicator for those firms in which the vote passed, and α_s represents time fixed effects, including fixed effects for both the year and the quarter. Time-varying individual characteristics are included in x_{ijs}, which only includes a two-piece linear function of employee tenure at firm j, the number of quarters the employee has spent at that firm.

In some specifications I include α_j for firm fixed effects and in others I use α_i instead, to control for individual fixed effects.These fixed effects are necessary because there is attrition in firms, meaning that differential attrition would be reflected in β_t and γ_t, in the absence of firm fixed effects. It also allows me to include multiple votes at the same firm if they are far apart enough in time. In order to better control for the time fixed effects, I also include observations on firms without any shareholder-sponsored proposals to declassify. When controlling for individual fixed effects, I also include observations of individuals when they were employed at a firm that never voted on a proposal to declassify. These observations contribute to the estimation of the employee fixed effects if the employee ever moves to or from a firm with a vote. However, it is important to note that these outside observations are not used to identify β_t and γ_t, although they do have an indirect effect. Standard errors are robust and clustered at the firm level. The coefficients of interest is γ_t and so, for brevity, these will be the coefficients displayed in tables.

First I establish that vote passage is meaningful, that proposals to declassify actually lead to changes in corporate governance. These results are shown in Table 5. The dependent variable, Declassified Board, is an indicator for whether the board of directors is declassified, so the regression estimated is a linear probability model. The two main takeaways from this table are, first, that there were no differences in board declassification in the years before the vote and, second, that there were significant differences after. The coefficients are small and not statistically significant in the top half of the table. Therefore in the four years prior

to each shareholder-sponsored proposal, firms in which the vote ended up passing were just as likely to be declassified as those in which it ended up failing.

In contrast, in the years following the vote, firms that pass the vote are significantly less likely to have a classified board. In the year after the vote, identified by the coefficient "Vote Passed x t+1", the point estimate goes from negative to positive, but remains not significant. However, significant differences in board declassification emerge gradually. By the fourth year, the coefficient on "Vote Passed x t-4" widens to .292 and is statistically significant at the 5% level.

To quantify the overall difference in declassified board between the pre and the post period, I show the F(Post=Pre). This is the F-statistic on the test for equality between the average difference between firms for the four years prior to the vote ($\gamma_{-4} + \gamma_{-3} + \gamma_{-2} + \gamma_{-1}$), and the average difference in the four years following the vote ($\gamma_4 + \gamma_3 + \gamma_2 + \gamma_1$). This tests one restriction, that the average pre-vote coefficient equals the average post-vote coefficient, excluding the year of the vote. This is similar to running a pure difference-in-difference estimate that compares the periods before and after the vote, except that there is more variance in the coefficients. The average coefficient in the four years prior to the vote was -.078. In the four years after the vote, this rose steeply to .133. The difference between these two averages represents a 21 percentage point change in the likelihood of a classified board. Equality of the average coefficients is rejected at the .01% level, with an F-statistic of 12.15. This reinforces the strong change in board declassification following the passage of the proposal.

These results establish that there were no differences in corporate governance before the vote and that the passage of a non-binding proposal is an effective tool for strengthening corporate governance. In the remainder of the difference-in-difference analysis I will use the same specification to verify that outcome variables do not differ across the threshold prior to the vote, and to test for changes following the vote.

5 Effects on the Firm's Workforce

Following the empirical strategy from the previous section, I test for for real effects of corporate governance on the earnings of employees. First I establish that my results are consistent with those in the prior literature by including only firm fixed effects. Then I present new results, controlling for employee fixed effects. I follow up with more detailed analysis by studying different types of employees and turnover.

5.1 Employee Earnings

Having established that board declassification leads to real changes in governance, I address what this means for the organization of the firm's workforce. The effects of board declassification on individual earnings are shown in Table 6. In both columns, the dependent variable is ln(Earnings), log quarterly employee earnings. The first column includes firm fixed effects and not employee fixed effects so the resulting estimates are analogous to changes in average earnings within a firm. Both columns also control for year fixed effects and a linear spline in the tenure of the employee with the firm. The observations included in the sample are from firms in which there was a declassification proposal and they span from four years before the vote to four years after.[18]

Looking at the coefficients in column one, It is clear that there are no significant differences in earnings prior to the vote but that average earnings drop after the vote for the firms that pass the proposal. The average coefficient on Vote Passed for the four years prior to the vote is -.03, indicating that firms that would pass the vote paid their employees 3% less on average. However, these differences are not statistically significant. This is strikingly different from the years following the vote. The coefficients on Vote Passed in year t and later are all negative and statistically significant at the 5% level. They are most negative starting two years after the vote, indicating that the change in average earnings is gradual.

[18] Observations from firms in which there was never a vote are not included in this regression due to computational limitations.

In the four years following the year of the vote, the average coefficient on Vote Passed is -.14. Therefore firms that passed the proposal paid their employees 14% less than those that did not. This is 11 percentage points lower than the difference in the years before the vote. We can conclude that the vote to declassify the board of directors led to an 11% decrease in average earnings. The F-statistic on this estimate is 5.20, with a p-value of 2.4%, as shown at the bottom of the table.

At the median firm in the sample, the average quarterly wage is roughly $16,000. Therefore an 11% decrease is equivalent to $1,760 less in earnings over the course of a quarter. This translates into $7,040 less per year, an economically large effect. Although I use different variation from that of Bertrand and Mullainathan (2003), I can compare my estimate to theirs. They found that wages rise by 1% following the passage of laws that enforces stricter anti-takeover provisions. The stronger effect in my paper is consistent with the differences in our studies. Because board declassification is plausibly a stronger determinant of corporate governance than business combination laws, it is not surprising that it has a stronger effect on wages. They also focus on all firms affected by state legislation whereas I focus on those firms in which shareholders prompted stronger corporate governance. This arguably identifies exactly those firms in which corporate governance would have a large effect.

Having established that earnings fall after corporate governance reforms, I focus on exploiting the benefits of the employer-employee matched data to determine how these changes come about. In the second column I use employee fixed effects rather than firm fixed effects.[19] In this sample I include all individual observations in firms with proposals, using the four years before and after the vote. In order to better control for employee and time fixed effects I also include observations on these employees during their employment at other firms, ones that never proposed declassification.[20]

[19]I do not include both fixed effects because the connected sample of employees switching between these firms is not large enough to estimate all of the firm fixed effects. Generally, when firms exit the sample, their employees do so as well. So controlling for either firm or employee fixed effects controls for this sample selection.

[20]The inclusion of these employees explains why the number of observations differ between the first and second column. If I include these observations in the first column, they do not contribute to estimates other

Including employee fixed effects controls for the average wage earned by each employee. This is best understood as a proxy for skill and bargaining ability. Therefore the coefficients on Vote Passed in every period identify the changes in employee earnings relative to their usual earnings. If the decrease in earnings was a result of individual employees getting paid less than usual, or earning lower rents than usual, we would expect to see strong negative effects following board declassification. However, if the changes in earnings were due only to changes in composition, we would expect to see no significant effects.

In fact there are only slight difference between individual earnings before and after the passage of the vote. This indicates that the change in average earnings is partially driven by the composition of the workforce. The average coefficient on Vote Passed is -.04 in the top half of the table, for the four years prior to the vote, and none are significant. The average coefficient on $Vote\ Passed$ in the four years after the vote is -.09. Again, these coefficients are typically not statistically significant. This means that on average there was a 5% decrease in individual employee's wages after the passage of the vote. To illustrate this point, the F-statistic on the equality of earnings before and after the vote is 0.82. Therefore at least half of the decrease in average earnings, a 6% decline, is explained composition of employees rather than falling individual earnings.

The contrasting results for the two regressions shown in Table 6 are displayed graphically in Figure 3. The graphs plot the coefficients on Vote Passed for the four years before and after the vote. The solid blue line represents the coefficient in each year and the dotted blue lines represent the 95% confidence intervals. The dotted red lines represent the average coefficients in the years before and after the vote. The first graph includes firm fixed effects whereas the second graph includes employee fixed effects. These plots more clearly demonstrate that without employee fixed effects there is a significant drop in average earnings after the vote, which shrinks significantly after controlling for employee fixed effects.

The results described above are robust to a number of untabulated robustness checks.

than to pin down year and quarter fixed effects, because these observations are at firms that never proposed declassification. So regression estimates remain extremely similar.

So far I have focused on estimating the intent to treat, comparing firms where the vote passes to those where it does not. If I instead compare firms in which the vote passed and declassification subsequently occurred to those in which the vote failed, results are similar. The wage drop is larger but again half is explained by a change in composition. Results are similar to those in Table 6 if firm attrition is limited by using only firms that I observe for two years around the vote, if tenure controls are dropped, or if industry wage controls are included. These results have strong implications for the effects of corporate governance on the workforce. The decreases in average wages are not driven by a wealth transfer from employees to shareholders. Instead they are significantly shifted by changes in the types of employees in each firm.

5.2 The Distribution of Earnings

Although there is a documented shift in earnings following vote passage, it is not clear whether this effect is consistent across the earnings distribution within a firm or concentrated among certain types of employees. And while there are no changes in individual earnings for the average employee, it could be that this is masking some heterogeneity in effects. For example, less standardized payment schemes could mean that high paid employees would start earning even more, while low paid employees earned less. Alternatively, a more uniform compensation plan could force earnings to tend toward the median.

To determine whether board declassification affects different types of employees similarly, I study its effect on ln(Earnings) at three different points of the wage distribution. In Table 7, I estimate the effect of governance on ln(Earnings) for three different percentiles within each firm-quarter. I use a similar approach to the previous regression but use data at the firm-quarter level, weighted by the number of employees in that firm-quarter. I again control for firm and year fixed effects.

The drop in wages is strongest in the bottom of the earnings distribution. The top half of the table shows that there are no differences in earnings for either high or low earners in the

years leading up to the vote. However, this changes once the votes pass. Earnings decrease sharply for employees at the 25th percentile of each firm's earnings distribution, as shown in the first column. The average change in coefficients from the four years before the vote to the four years after is -.124 for the 25th percentile. The F-statistic is not large enough to reject equality, as might be expected with this much less detailed sample. However, there is a striking differences in point estimates across the earnings distribution. The changes for employees earnings median wages or at the 75th percentile were less than -.05. This result differ from those of Bertrand and Mullainathan (2003) and Lichtenberg and Siegel (1990b), which show that wage changes were more drastic for white-collar employees following changes in corporate governance or ownership.

Together these patterns suggest that the distribution of earnings within the firm should widen following board declassification. Rather than opting for cheaper or less skilled employees across the board, assuming an individual's average earnings are a proxy for skill, the firm is careful to look for cheaper employees only at the lower skill level. This also suggests that it is not the case that firms become less able to hire and retain high-skilled employees, whether due to a change in corporate culture or other practices.

5.3 Employee Turnover

From the results of the previous section, it is clear that turnover is an important factor in the reorganization of the workforce. In order to better understand how board declassification affects employees, it is important to understand how this turnover is taking place: who is joining the firm, who is leaving, and the rate at which this is occurring.

Drastic shifts in the firm's workforce are sometimes brought about by large structural changes, such as mergers and acquisitions. In these cases, reorganization is associated with large turnover or mass layoffs. Indeed, private equity buyouts are followed by significant employee and plant churn (Davis et al., 2011; Bharath et al., 2014). Even more related, weaker corporate governance has been shown to lead to fewer plant closings and openings

(Bertrand and Mullainathan, 2003). One must wonder whether the changes in workforce composition are caused by the firm shutting down or opening plants.

The timing and nature of the turnover is addressed in Table 8. In the first column, the dependent variable End Job is an indicator for whether an employee ends their job with a firm. In the second column, New Job is an indicator for whether an employee joins a new firm. The regression follows the original specification outlined in Equation 1 but with binary outcome variables. The main takeaway from this table is that there is no systematic change in turnover following the passage of the vote. Almost all coefficients are small and not statistically significant and the F-statistics at the bottom of the table indicate that are no changes in turnover from the period before the vote to that after.

The one exception is the coefficient of End Job on Vote Passed x t+1, which is much larger than the others. It is .034 and significant at the 5% level. This means that in the year after the vote, employees with firms that passed the vote are 3.4 percentage points more likely to leave their jobs. This is off of a base level of 5.6% average turnover per quarter within the sample. So although there was no systematic change in hiring practices, there was a temporary uptick in the number of employees leaving the firm shortly after the vote. Although I cannot observe whether employees are quitting or being fired, I am able to see whether they become reemployed at another firm in Texas or whether they exit the sample. The increase in employees leaving firms where votes pass are due wholly to employees moving to other firms in Texas, not exiting the Texas labor force altogether.

The overall stability of turnover within these firms indicates that the changing composition of the workforce is not caused by a wave of hiring and firing. This result, combined with the fact that most employees remain employed in Texas, rules out a large variety of hypotheses that rely on drastic shifts in turnover. These include shutting down or opening new plants, mass layoffs, moving from illegal to legal workers, moving production out of state, and offshoring. Instead, the trends in turnover are more consistent with the implementation of a more targeted employee selection process. To see how this squares with the

wage changes observed in Table 6, assume turnover is 5% per quarter and a wage drop of 6% occurs over the course of 2 years, or 8 quarters. This would imply that the wage difference between employees leaving the firm and those entering the firm every quarter is 15%.

To better understand this movement of employees into and out of the firm, it is important to study the types of employees joining and leaving the firm. Table 9 shows the changing tenure and earnings changes of these workers. The regression follows the original specification but limits the sample to only those employees joining (Joiners) or leaving (Leavers) the firm. In the first two columns, the dependent variable Tenure is the number of quarters that an employee has been with their firm. The first column focuses only on Joiners, those employees joining the firm. Their tenure is measured as the number of quarters they had worked for their previous employer. The coefficients on Vote Passed are not significant before the vote but rise dramatically after the vote. The coefficients on Vote Passed x t+2 and Vote Passed t+4 are 3.75 and 2.31, respectively, and significant at the 5% level. So two years after the vote, the workers joining firms that pass the vote have an average of 3.75 more quarters of experience with their previous employer. The increased tenure of hired employees is even more evident from the F-statistic of 6.88, which shows that equality of coefficients before and after the vote is rejected at the 1% level.

Because firms begin hiring more highly tenured workers from other firms, it might be natural to think that they are also letting go of their more highly tenured workers. However, the second column shows that this is not the case. Here tenure is measured as the number of quarters each worker spent with the firm before leaving. None of the coefficients on Vote Passed are significant and there are no trends in these coefficients. The F-statistic on the equality of coefficients before and after the vote is 0.10, signifying no difference over time. Therefore the tenure of employees leaving firms that passed the vote are similar to those leaving firms that did not pass it.

It is surprising that there is no change in the tenure of workers leaving the firm. Usually employees that have been with the firm for the longest amount of time are most likely to

have wages above their marginal product. This arises under wage rigidity, tenure-based pay schemes, or implicit long-term contracts (Shleifer and Summers, 1988). Therefore it is commonly thought that higher tenure employees would be the first to be affected once shareholders choose to reform pay or contracts. I show that this does not happen. This lends more credence to the fact that the shift in average wages is not driven by correcting wages paid to individual employees but substantially affected by the changing composition of the workforce.

The next two columns of the table study the changing selection of employees into and out of the firm along a different dimension, the change in earnings. The goal of this exercise is to directly test what happens to employees after they leave or join the firm. In particular, are highly paid employees being replaced by similar employees that are paid less? This may occur if firms bear some cost to changing individual wages. These costs may take the form of employee dissatisfaction and lower performance.[21] To avoid these costs, firms might instead replace the overpaid employees with equally skilled new employees, but pay them a lower wage. This could lead to changes in workforce composition that resulted in lower average wages. Such an explanation is not consistent with the fact that individual employees do not earn less within the firm. However it would challenge the interpretation of observed wage changes as not pure financial effects.

Table 9 measures Δ ln(Earnings), the change in log quarterly earnings that accompanies a switch to a new firm. To avoid partial earnings in the first quarter of employment, it compares the quarter before leaving a firm to the quarter after joining a new firm. Because the measure only reflects total earnings for each quarter, it combines employee wages and work hours. Therefore decreases in earnings can be driven by lower hourly wages, fewer hours worked per day, fewer days worked, or some combination of the three. Although these effects cannot be teased apart, total quarterly earnings are still likely to be informative of employee productivity in that quarter.

[21] In fact productivity has been shown to decline with labor strife, or once wages fall below an employee's reference point (Mas, 2006, 2008; Hart et al., Forthcoming, 2011).

The third column shows the $\Delta ln(Earnings)$ when new employees join the firms in which the vote passed. There are no significant effects in any period and there are no changes that emerge after the passage of the vote. The last column demonstrates earnings changes for employees leaving firms in which the vote passed. In this case there is a significant increase in the earnings trajectory of employees leaving the firm. Prior to vote passage, employees leaving the firm were likely to experience larger pay decreases than other firms. In fact, the decreases are so large, including a 60% decrease in period t-2, that they must be partially due to movements from full-time to part-time work. However, after the vote exiting employees experience a greater pay increase than in other firms. Equality of the coefficients before and after the vote is rejected, with an F-statistic of 9.96 and a p-value of .002.

Employees that leave the firms that passed the vote become better off, not worse. This is consistent with the evidence that they generally remain in the workforce. These results also underscore the point that it is not older, overpaid employees that are leaving the firm. Given the evidence, it seems more likely that they are employees are more skilled than their replacements, on a higher trajectory, and the firm may not be willing to meet the earnings increases they demand. Instead the firm replaced them with cheaper employees with more experience at their previous firms. These results reinforce the idea that the changing composition of the workforce is not simply a way to pay similar employees lower wages but due to a real organizational change.

5.3.1 Relation to Headquarter Location

The sizeable composition effect that I find runs counter to some of the more common explanations for the link between corporate governance and employee compensation, such as rent-sharing, and implicit contracts. These explanations assume that managers wish to pay employees more than shareholders do, perhaps because they dislike disappointing their employees or they wish to be generous. Although this is plausible in some settings, it is possible that it is more suited to smaller or more concentrated companies rather than the large pub-

licly traded firms that I study. Indeed, (Landier et al., 2009) find that managers are reluctant to fire employees closer to headquarters, which suggest they may also be willing to pay them more when entrenched.

To determine whether this is a contributing factor, I relate the effects I find to headquarter location. I separately estimate the effects of vote passage for twelve firms that are headquartered in Texas and those that are not. To be sure, there are a number of differences between these firms and others. Not only is their management team is closer to the employees in the sample, but Texas firms are more likely to include top-level employees in the sample.

Table 10 shows the results of estimating the initial earnings regressions separately for the firms headquartered in Texas. The first column contains the firm-level results and shows a strong negative effect on average wages, a decrease of roughly 17.5%. The F-statistic is 17.37, significant at the 1% level. However, this is matched, and actually surpassed in point estimate, by the effect on individual employee earnings, which is significant at the 0.1% level. In the case of Texas companies, it is indeed true that decreasing earnings come from cuts in wages to individual employees.

The next two columns show the effects of vote passage on firms that are not headquartered in Texas. Here there is still a significant decrease in earnings of 9.7%, show in the third column. However, after controlling for employee fixed effects in the last column, there is no change in individual employee wages whatsoever.[22] The fact that earnings changes remain suggest that these firms still cut wages but rely exclusively on changes in composition. These firms are less likely to employ higher-earning employees in Texas and their management teams were farther away, making them less susceptible to pressure, either by the public or their own interpersonal ties, to pay higher wages to their Texas employees. These contrasting results suggest that less governed managers choose to pay more to their employees when they are closer but simply change the skill composition of the workforce when farther away.

[22]Similar trends emerge if I use firms with headquarters in states bordering Texas and compare them to all other firms.

5.4 Firm Outcomes

Up until now, I have studied only employee outcomes. However, it is important to put this in the broader context of what is happening to the firm more broadly. Because the employee data in this paper is much more detailed than the firm-level data, it is more difficult to precisely measure firm-level outcomes. This is especially true given the small number of firms used to compare vote passage to failure. However, I highlight three main directional trends in firm outcomes in Table 11.

The first column of the table shows that NPM rose by 10.6 percentage points in the four years after vote passage, although this is not statistically significant. ROA, shown in the second column, increased by 5 basis points after vote passage, although this is again not significant. Finally, the last column shows that labor expenses, which include wages, benefits, incentive compensation, decreased by 202 million dollars on average after the vote. These patterns do not identify any strong effects but are broadly consistent with rising profitability and falling costs.

6 Conclusion

In this paper I have used a new dataset of public US firms linked to individual employees in Texas to identify how votes to declassify the board of directors affect the firm's workforce. I verify that average earnings decrease following stronger corporate governance, in line with previous results in the literature. However, these decreases are caused in large part by changes in the composition of the workforce rather than being solely driven by decreases in individual employee earnings, concentrated in the bottom of the distribution within each firm. Low-earning employees are replaced by even lower-earning workers from other firms.

The main implication of my result is that it shows how the resolution of agency problems between shareholders and management affect the general employees of the firm. Although it has been established that managers choose to lead the "quiet life" when they are protected

from shareholder oversight, and that this results in lower employee wages, it has not have been clear how this occurs. The focus has most commonly been on managers that pay employees more than shareholders would choose to, meaning that stronger corporate governance leads to a wealth transfer from employees to shareholders. I show that this is not generally the case. Instead managers lead the "quiet life" by also employing higher-earning workers. This is especially true when the headquarters of the firm are not in the same state as the employees studied.

I show in the paper that the evidence can rule out a number of hypotheses. In the majority of firms, it does not seem that shareholders pressure managers into reneging on long-term contracts, or firing employees due to sticky wages. This leaves only two explanations for why workforce composition shifts the way it does. The first is that managers prefer to hire high quality, high-trajectory employees but that this does not affect, or even improves, firm efficiency. The second explanation, which is more in line with the evidence on corporate governance and plant productivity, is that entrenched managers choose higher quality employees than is efficient.

This could happen, for example, if managers institute practices that are less able to identify firm-worker match quality or do not efficiently invest in training new employees. This idea is consistent with the observation of Agrawal and Tambe (2013) that private equity owned firm invest more in developing employees' technological skill. Of course it could also be that managers are reluctant to commit to a narrower range of employee types. If it is costly to adjust employee types, this effect is in line with the risk-aversion of entrenched managers (Gormley and Matsa, 2014; Ferreira and Laux, 2007). Differentiating between these explanations requires more detailed data on the productivity and training of employees, and is beyond the scope of this paper. Nevertheless, this discussion highlights the contributions of the paper and avenues for further research.

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Figure 1: Declassification Following Votes to Declassify Boards

Notes: This graph plots board classification for firms that voted on a proposal to declassify the board of directors. The dark blue line represents firms in which the vote passed. The light blue line represents firms in which the vote failed to pass. The y-axis measure the fraction of firms with Declassified boards in every year for the two types of firms. The x-axis measures the year relative to the year in which shareholders voted on the proposal.

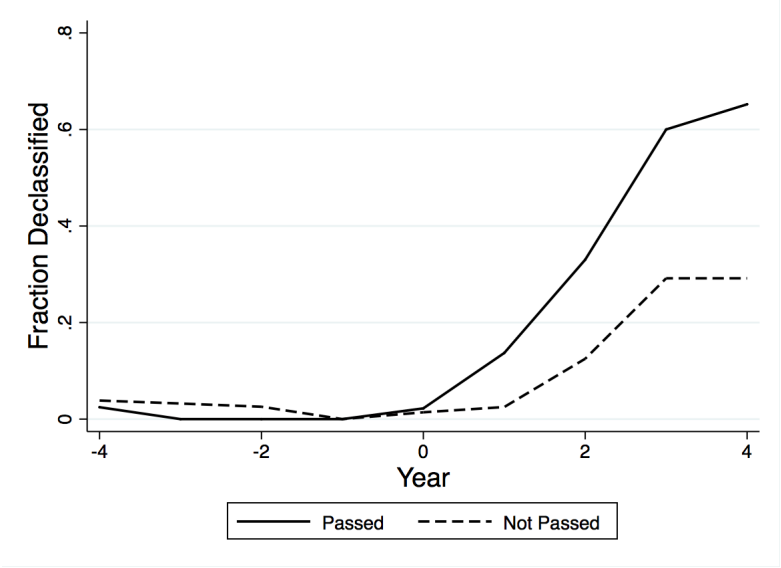

Figure 2: Votes to Declassify Boards

Notes: Each graph plots board declassification for firms that voted on a proposal to declassify the board of directors. The x-axis shows the number of percentage points that the vote gained, relative to the threshold necessary to pass the vote. Observations are grouped into bins of size 2 (percentage points). The y-axis measures the proportion of firms in each bin that have a Declassified board. The relationship between votes gained and board declassification is presented for four different time periods: two years before the vote, the year of the vote, two years after the vote, and four years after the vote.

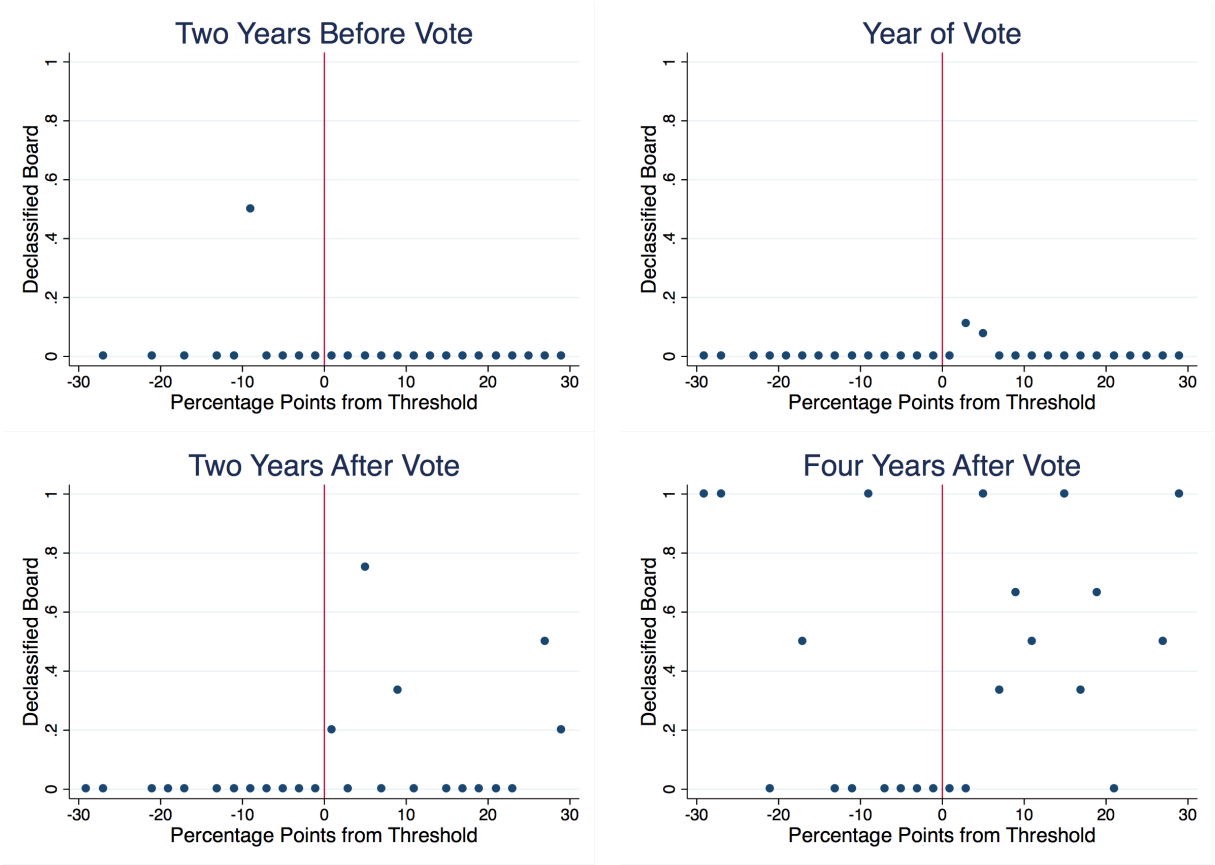

Figure 3: Estimates of Wage Effects

Notes: These graphs represent the coefficients from estimating Equation 1. The coefficient on "Vote Passed", measured by the y-axis, indicates the firms in which the proposal passed in year t, measured by the x-axis. The solid blue line represents the coefficient in each year and the dotted blue lines represent the 95% confidence intervals. The dotted red lines represent the average coefficients in the years before and after the vote. The displayed coefficients estimate the difference between employees in firms in which the vote passed and those in firms in which it did not, in every year relative to the vote. Every regression includes year and quarter fixed effects. The first graph includes firm fixed effects whereas the second graph includes employee fixed effects.

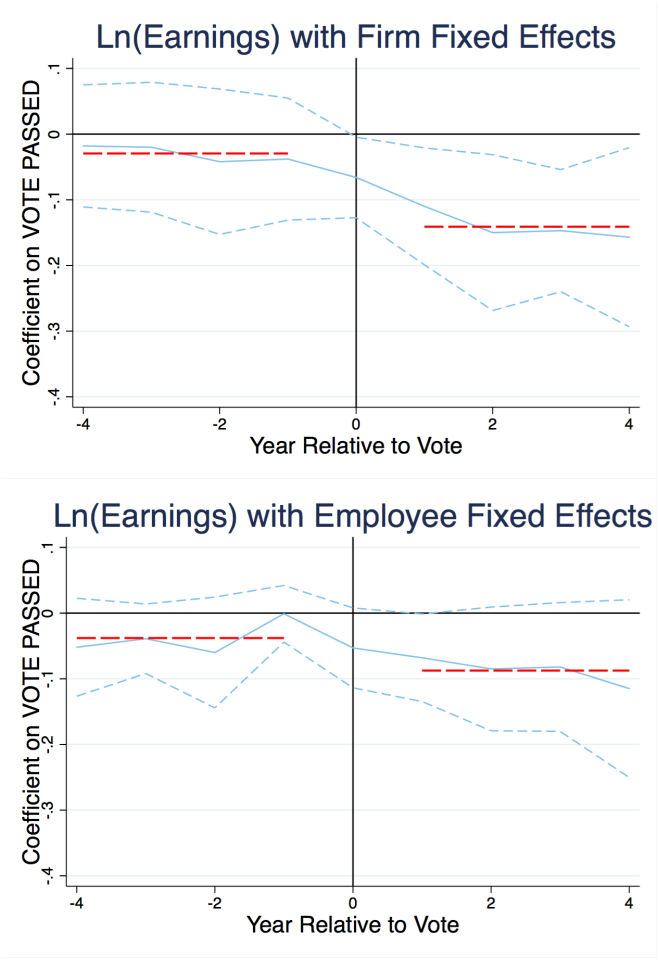

Table 1: Summary Statistics of Matched Firms

Notes: Summary statistics are presented for every firm that matches to the Texas employee data. "# Quarters" is the length of time that each firm is part of the employer-employee matched panel. The unit of observation for that variable is the firm. For all other statistics, the unit of observation is a firm-quarter. "Declassified Board" is an indicator for whether or not the board of directors is Declassified. "Employees (Compustat)" represents Compustat's measure of the total number of employees for the firm. "Matched Employees" is the number of Texas employees matched to the firm in each quarter. "Fraction Employees Matched" represents the ratio of employees in the Texas data to those in Compustat. "Texas HQ" is an indicator for whether the firm's headquarters are in Texas. The last row represents the ratio of Texas to Compustat employees only for those firms that are headquartered in Texas.

	Mean	Median	Std Dev	Observations
# Quarters	23.6	20	16.8	2,246
Declassified Board	.442	0	.497	38,148
Employees (Compustat)	27,260	8,400	77,433	38,148
Matched Employees	912	191	2,200	38,148
Fraction Employees Matched	.136	.0232	1.1	38,148
Texas HQ	.122	0	.328	38,148
Fraction Employees Matched | Texas HQ	.426	.154	2	6,762

Table 2: Match of Firms to Employee Data

Notes: In the first two columns the dependent variable "Matched" is an indicator for whether a firm-quarter observation matches to at least one Texas employee in the data. In the last two columns the dependent variable "% Employees" is the proportion of matched Texas employees to Compustat employees. This regression is only run for those firm-quarters that matched to the data. Observations are at the firm-quarter level. "Declassified Board" indicates whether the board of directors is declassified. "Texas HQ" is an indicator for whether the firm is headquartered in Texas and "Firm Age" is the number of years the firm has been in Compustat. Other controls include the log of financial assets (ln(Assets)), capital expenditure (ln(CapEx)), and liabilities (ln(Liabilities)). The first and third columns control for industry fixed effects while the second and fourth columns control for firm fixed effects. All regressions include year and quarter fixed effects. When controlling for industry fixed effects, industries are defined by the firm's 3-digit NAICS code. The last row shows sample means for each of the dependent variables. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	Matched	Matched	% Employees	% Employees
Declassified Board	-.012	-.0108	.0302	.0243
	(.0143)	(.0122)	(.0328)	(.0132)
Texas HQ	.134***		.164***	
	(.0272)		(.0431)	
Firm Age	.00125*	.0111	.000697	-.0376
	(.000582)	(.00888)	(.000798)	(.0416)
ln(Assets)	.0195	.0492***	-.00282	-.0274
	(.0168)	(.0135)	(.0312)	(.0229)
ln(CapEx)	.0117*	-.0026	-.0321	-.0187
	(.00564)	(.00367)	(.0186)	(.0226)
ln(Liabilities)	.0379**	.00906	-.0171	-.105
	(.0136)	(.0113)	(.0311)	(.0807)
Industry FE	Yes	No	Yes	No
Firm FE	No	Yes	No	Yes
Observations	73,481	73,481	35,806	35,806
Adj. R-Square	0.270	0.666	0.279	0.605
Mean	0.493	0.493	0.116	0.116

Table 3: Industries and Matching

Notes: Firm industries are presented for firms that match to the linked employer-employee data and for those that do not. Industry classifications come from each firm's 2-digit NAICS code. The unit of observation is the firm-quarter. Only the eight most common industries in the data are displayed. The unit of measurement is the proportion of matched (or not matched) firms that belong to that industry. Standard deviations are shown in parentheses.

	Not Matched	Matched
Mining	0.04	0.04
	(0.19)	(0.20)
Utilities	0.05	0.03
	(0.22)	(0.18)
Manufacturing	0.40	0.40
	(0.49)	(0.49)
Wholesale	0.02	0.04
	(0.15)	(0.19)
Retail	0.05	0.07
	(0.21)	(0.26)
Finance	0.17	0.11
	(0.37)	(0.31)
Real Estate	0.04	0.03
	(0.20)	(0.17)
Science & Technical	0.04	0.04
	(0.19)	(0.20)
Observations	73,659	53,086

Table 4: Firm Characteristics & Vote Passage

Notes: The regressions in this table estimate a linear relationship between the probability of vote passage and firm characteristics in the quarter in which a declassification vote took place. "Firm Age" is the number of years the firm has been in Compustat. Other controls include the log of financial assets (ln(Assets)), where assets are measures in millions of dollars, return on assets (ROA), the number of employees in Compustat (Employees) measured in thousands, institutional ownership as a ratio of total ownership (IO), and the governance index of 24 indicators developed by Gompers et al. (2003) (G-Index), which is higher when corporate governance is weaker. When any independent variables are missing, the mean value is assigned and I control for indicators that each variable is missing. The regression includes industry, year and quarter fixed effects. Standard errors are robust. * p<.05 ** p<.01 ***p<.001

	Vote Passed
Firm Age	.00172
	(.00193)
ln(Assets)	-.00347
	(.0257)
ROA	.0959
	(.406)
Employees	.000542
	(.000529)
IO	.597**
	(.223)
G-Index	-.0184
	(.0119)
Industry FE	Yes
Year & Quarter FE	Yes
Observations	333
Adj. R-Square	0.125

Table 5: Effects on Board Declassification

Notes: The regression in this table estimates Equation 1 and use observations at the firm-quarter level. For firms in which there was a vote, only observations within four years before the vote and four years after are included. Observations on firms without any votes are also included. The dependent variable "Declassified Board" is an indicator for whether the board of directors is declassified. Year t is the year in which a firm's shareholders votes on a proposal to declassify the board of directors. "Vote Passed" indicates the firms in which the proposal passed in year t. The displayed coefficients estimate the difference between firms in which the vote passed and those in which it did not, in every year relative to the vote. Every regression includes firm, year, and quarter fixed effects. F(Post=Pre) is the F-statistic testing for equality between the average effect prior to vote passage (years $t-4$ through $t-1$) and the average effect after vote passage (years $t+1$ through $t+4$). p(Post=Pre) is the p-value of that statistic. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	Declassified Board
Vote Passed x t-4	-0.082
	(0.070)
Vote Passed x t-3	-0.090
	(0.067)
Vote Passed x t-2	-0.068
	(0.060)
Vote Passed x t-1	-0.070
	(0.058)
Vote Passed x t	-0.015
	(0.046)
Vote Passed x t+1	0.001
	(0.059)
Vote Passed x t+2	0.093
	(0.073)
Vote Passed x t+3	0.147
	(0.100)
Vote Passed x t+4	0.292*
	(0.115)
Fixed Effects	Firm
Post-Pre	0.211
F(Post=Pre)	12.15
p(Post=Pre)	0.000
Observations	80,619
Adj. R-Square	0.888

Table 6: Effects on Employee Earnings

Notes: The regressions in this table estimates Equation 1 and use observations at the employee-quarter level. All observations are for employees of firms in which there was a vote, and only those employees that were with the firm within four years before the vote and four years after are included. The second column also includes observations on these employees during their time at other firms, ones that never voted. The dependent variable ln(Earnings) is log quarterly employee earnings. Year t is the year in which a firm's shareholders votes on a proposal to declassify the board of directors. "Vote Passed" indicates the firms in which the proposal passed in year t. The displayed coefficients estimate the difference between employees in firms in which the vote passed and those in firms in which it did not, in every year relative to the vote. Every regression includes year and quarter fixed effects. The first column includes firm fixed effects whereas the second column includes employee fixed effects. F(Post=Pre) is the F-statistic testing for equality between the average effect prior to vote passage (years $t-4$ through $t-1$) and the average effect after vote passage (years $t+1$ through $t+4$). p(Post=Pre) is the p-value of that statistic. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	ln(Earnings)	
Vote Passed x t-4	-.018	-.052
	(.047)	(.038)
Vote Passed x t-3	-.020	-.039
	(.050)	(.027)
Vote Passed x t-2	-.042	-.060
	(.056)	(.043)
Vote Passed x t-1	-.038	-.001
	(.047)	(.022)
Vote Passed x t	-.066*	-.053
	(.031)	(.031)
Vote Passed x t+1	-.110*	-.068*
	(.045)	(.034)
Vote Passed x t+2	-.150*	-.085
	(.060)	(.048)
Vote Passed x t+3	-.147**	-.082
	(.047)	(.050)
Vote Passed x t+4	-.157*	-.115
	(.069)	(.069)
Fixed Effects	Firm	Employee
Post-Pre	-.112	-.050
F(Post=Pre)	5.20	0.82
p(Post=Pre)	.0241	.3646
Observations	2,745,210	3,237,432
Adj. R-Square	.3882	.7573

Table 7: Effects on Earnings Distribution

Notes: The regressions in this table estimates Equation 1 and use observations at the firm-quarter level, weighted by the number of employees in each firm-quarter. All observations are for employees of firms in which there was a vote, and only those employees that were with the firm within four years before the vote and four years after are included. The dependent variable in every column is a different percentile of ln(Earnings), log quarterly employee earnings. Year t is the year in which a firm's shareholders votes on a proposal to declassify the board of directors. *Vote Passed* indicates the firms in which the proposal passed in year t. Every regression includes year, quarter, and firm fixed effects. F(Post=Pre) is the F-statistic testing for equality between the average effect prior to vote passage (years $t-4$ through $t-1$) and the average effect after vote passage (years $t+1$ through $t+4$). p(Post=Pre) is the p-value of that statistic. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	ln(Earnings)		
	25%	50%	75%
Vote Passed x t-4	-.063	-.012	.010
	(.068)	(.048)	(.046)
Vote Passed x t-3	-.086	-.034	-.008
	(.079)	(.046)	(.038)
Vote Passed x t-2	-.105	-.040	-.027
	(.069)	(.040)	(.052)
Vote Passed x t-1	-.045	-.025	.027
	(.075)	(.051)	(.046)
Vote Passed x t	-.153*	-.027	-.010
	(.064)	(.027)	(.017)
Vote Passed x t+1	-.172	-.031	-.031
	(.092)	(.032)	(.020)
Vote Passed x t+2	-.196	-.065	-.047
	(.111)	(.040)	(.030)
Vote Passed x t+3	-.197	-.067	-.062
	(.116)	(.046)	(.032)
Vote Passed x t+4	-.229	-.078	-.049
	(.128)	(.057)	(.041)
Fixed Effects	Firm	Firm	Firm
Post-Pre	-.124	-.033	-.048
F(Post=Pre)	1.94	0.44	1.28
p(Post=Pre)	.1662	.5105	.2596
Observations	4,641	4,641	4,641
Adj. R-Square	.9243	.9465	.9386

Table 8: Effects on Turnover

Notes: The regressions in this table estimates Equation 1 and use observations at the employee-quarter level. All observations are for employees of firms in which there was a vote, and only those employees that were with the firm within four years before the vote and four years after are included. The dependent variable "End Job" is an indicator for whether an employee ends their job with a firm and "New Job" is an indicator for whether an employee joins a new firm. Year t is the year in which a firm's shareholders votes on a proposal to declassify the board of directors. "Vote Passed" indicates the firms in which the proposal passed in year t. The displayed coefficients estimate the difference between employees in firms in which the vote passed and those in firms in which it did not, in every year relative to the vote. Every regression includes firm, year, and quarter fixed effects. F(Post=Pre) is the F-statistic testing for equality between the average effect prior to vote passage (years $t-4$ through $t-1$) and the average effect after vote passage (years $t+1$ through $t+4$). p(Post=Pre) is the p-value of that statistic. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	End Job	New Job
Vote Passed x t-4	.012	.037
	(.009)	(.040)
Vote Passed x t-3	.013	.033
	(.010)	(.039)
Vote Passed x t-2	.001	-.225
	(.016)	(.145)
Vote Passed x t-1	.019	-.001
	(.019)	(.038)
Vote Passed x t	.005	.016
	(.008)	(.012)
Vote Passed x t+1	.034*	-.016
	(.014)	(.025)
Vote Passed x t+2	.006	.015
	(.015)	(.018)
Vote Passed x t+3	.014	.014
	(.012)	(.018)
Vote Passed x t+4	.020	.015
	(.012)	(.019)
Fixed Effects	Firm	Firm
Post-Pre	.008	.046
F(Post=Pre)	0.33	0.81
p(Post=Pre)	.5646	.3688
Observations	2,745,210	2,745,210
Adj. R-Square	.0574	.1018

Table 9: Characteristics of Employees Joining and Leaving

Notes: The regressions in this table estimates Equation 1 and use observations at the employee-quarter level. All observations are for firms in which there was a vote, and only those observations within four years before the vote and four years after are included. Joiners denotes employees that just joined the firm that quarter and Leavers denotes employees in their last quarter with the firm. Year t is the year in which a firm's shareholders votes on a proposal to declassify the board of directors. *Vote Passed* indicates the firms in which the proposal passed in year t. The displayed coefficients estimate the difference between firms in which the vote passed and those in firms in which it did not, in every year relative to the vote. Every regression includes firm, year, and quarter fixed effects. F(Post=Pre) is the F-statistic testing for equality between the average effect prior to vote passage (years $t-4$ through $t-1$) and the average effect after vote passage (years $t+1$ through $t+4$). p(Post=Pre) is the p-value of that statistic. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	Tenure		Δ ln(Earnings)	
	Joiners	Leavers	Joiners	Leavers
Vote Passed x t-4	.446	.428	-.086	-.474
	(.412)	(1.75)	(.085)	(.275)
Vote Passedx t-3	.374	.143	.011	-.166
	(.468)	(1.94)	(.093)	(.287)
Vote Passed x t-2	-.838	-1.96	-.021	-.916**
	(1.05)	(1.61)	(.087)	(.324)
Vote Passed x t-1	-1.73	-.287	-.046	.192
	(.979)	(.672)	(.058)	(.192)
Vote Passed x t	1.18*	.201	-.017	-.134
	(.594)	(.422)	(.071)	(.150)
Vote Passed x t+1	-1.51	-.225	-.002	.060
	(1.22)	(.499)	(.087)	(.075)
Vote Passed x t+2	3.75*	-.031	-.059	.045
	(1.66)	(.883)	(.118)	(.113)
Vote Passed x t+3	1.01	.212	-.031	.129
	(.672)	(.990)	(.111)	(.166)
Vote Passed x t+4	2.31*	.153	.162	.314
	(.980)	(.829)	(.116)	(.314)
Fixed Effects	Firm	Firm	Firm	Firm
Post-Pre	1.83	.446	.053	.478
F(Post=Pre)	6.88	0.10	0.58	9.96
p(Post=Pre)	.0098	.7493	.4492	.0020
Observations	33,654	118,747	25,798	26,068
Adj. R-Square	.2870	.5129	.1172	.0854

Table 10: Texas vs. Non-Texas Headquarters

Notes: The regressions in this table estimates Equation 1 and use observations at the employee-quarter level. All observations are for employees of firms in which there was a vote, and only those employees that were with the firm within four years before the vote and four years after are included. The second and fourth columns also include observations on these employees during their time at other firms, ones that never voted. The dependent variable ln(Earnings) is log quarterly employee earnings. The regressions are split into those firms headquartered in Texas and all other firms. Year t is the year in which the vote occurred. "Vote Passed" indicates the firms in which the proposal passed in year t. The displayed coefficients estimate the difference between employees in firms in which the vote passed and those in firms in which it did not, in every year relative to the vote. Every regression includes year and quarter fixed effects. F(Post=Pre) is the F-statistic testing for equality between the average effect prior to vote passage (years $t-4$ through $t-1$) and the average effect after vote passage (years $t+1$ through $t+4$). p(Post=Pre) is the p-value of that statistic. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	ln(Earnings)			
	Texas HQ		Non-Texas HQ	
Vote Passed x t-4	.121	.078	-.055	-.080
	(.071)	(.073)	(.048)	(.041)
Vote Passed x t-3	.054	.010	-.053	-.063
	(.024)	(.034)	(.053)	(.029)
Vote Passed x t-2	.029	.027	-.052	-.089
	(.075)	(.064)	(.064)	(.052)
Vote Passed x t-1	.004	.000	-.026	-.005
	(.022)	(.024)	(.052)	(.022)
Vote Passed x t	.004	.001	-.082**	-.075*
	(.017)	(.020)	(.031)	(.031)
Vote Passed x t+1	-.170**	-.241**	-.147*	-.067*
	(.053)	(.092)	(.056)	(.030)
Vote Passed x t+2	-.178**	-.170**	-.174*	-.081
	(.039)	(.057)	(.077)	(.043)
Vote Passed x t+3	-.109**	-.165**	-.142*	-.052
	(.034)	(.061)	(.059)	(.033)
Vote Passed x t+4	-.033	-.142*	-.110	-.047
	(.069)	(.064)	(.098)	(.052)
Fixed Effects	Firm	Employee	Firm	Employee
Post-Pre	-.175	-.208	-.097	-.003
F(Post=Pre)	17.37	12.92	2.53	0.01
p(Post=Pre)	.0016	.0003	.1141	.9389
Observations	462,999	955,221	2,282,211	2,774,433
Adj. R-Square	.1181	.7874	.4096	.7634

Table 11: Firm Outcomes

Notes: The regressions in this table estimates Equation 1 and use observations at the firm-quarter level. The dependent variables are NPM (Net Profit Margin), ROA (Return on Assets) and Labor Expenses, measures in millions of dollars spent on employees' wages and benefits. Year t is the year in which the vote occurred. "Vote Passed" indicates the firms in which the proposal passed in year t. The displayed coefficients estimate the difference between employees in firms in which the vote passed and those in firms in which it did not, in every year relative to the vote. Every regression includes year and quarter fixed effects. F(Post=Pre) is the F-statistic testing for equality between the average effect prior to vote passage (years $t-4$ through $t-1$) and the average effect after vote passage (years $t+1$ through $t+4$). p(Post=Pre) is the p-value of that statistic. Standard errors are robust and clustered at the firm level. * p<.05 ** p<.01 ***p<.001

	NPM	ROA	Labor Expenses
Vote Passed x t-4	.0032	-.0145	407
	(.113)	(.0093)	(815)
Vote Passed x t-3	-.0505	-.0173	761
	(.0952)	(.00937)	(909)
Vote Passed x t-2	-.116	-.0131*	1,350
	(.113)	(.00666)	(1,153)
Vote Passed x t-1	-.143	-.0104	1,573
	(.13)	(.00733)	(1,186)
Vote Passed x t	-.083	-.0136	1,348
	(.107)	(.00735)	(972)
Vote Passed x t+1	.036	-.00811	1,384
	(.103)	(.00565)	(1,088)
Vote Passed x t+2	.0848	-.0147	628
	(.126)	(.0081)	(1,079)
Vote Passed x t+3	.00355	-.00186	215
	(.147)	(.0147)	(1,344)
Vote Passed x t+4	-.00646	-.0286*	1,055
	(.17)	(.0137)	(572)
Post-Pre	.106	.0005	-202
F(Post=Pre)	1.11	0.01	0.07
p(Post=Pre)	0.291	0.943	0.787
Observations	115,885	115,815	16,453
Adj. R-Square	0.023	0.257	0.884