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## **Say Pays! Shareholder Voice and Firm Performance**

**Upjohn Institute Working Paper No. 13-192**

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### **ABSTRACT**

This paper estimates the effect of increasing shareholder “voice” in corporations through a new governance rule that provides shareholders with a regular vote on pay: Say on Pay. We apply a regression discontinuity design to Say on Pay shareholder proposals to deal with prior expectations and the endogeneity of internal governance rules. Adopting Say on Pay leads to large increases in market value (5.4 percent), firm profitability, and long-term performance. In contrast, we find small effects on the level and structure of pay. This suggests that Say on Pay operates as a regular confidence vote, increasing efficiency, and market value.

**JEL Classification Codes:** G34, D21, G14

**Key Words:** Agency Cost, Corporate Governance, Shareholder Meetings, Regression Discontinuity, Event Studies

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## INTRODUCTION

How much “voice” should shareholders have in a large modern corporation?

Shareholders who disagree with the course of events at their corporations have two main mechanisms to express their dissent: they can threaten to sell their shares, that is, “exit,” or alternatively, they can engage with management and express their opinions, that is, use the “voice” mechanism (Hirschman 1970). While the impact of exit on firm value and policies has been studied extensively, less attention has been devoted to estimating the value of voice in corporations. Hirschman (1970) first introduced the idea that voice was an important mechanism for the correct operation of institutions (from firms to public schools); yet, to date, there is little systematic evidence on the actual impact of voice as a disciplining mechanism within firms.

This paper studies the consequences of Say on Pay, a mechanism that allows shareholders to express their voice by voting on a crucial corporate matter: the pay policy of its executive officers and its relationship to firm performance. Firms with a Say on Pay policy in place offer shareholders a regular advisory vote on whether they approve of the relationship between executive pay and performance in their companies. Given that the focus of this vote is not just on pay itself but on whether pay is commensurate with the value that the CEO adds to the firm, the vote resembles an explicit confidence vote on the CEO: the vote effectively aggregates the opinion of shareholders into a simple and highly visible metric.

Our goal is to provide a causal estimate of the effect of increasing shareholder voice on shareholder value, firm performance, and executive pay. To do so, we use a regression discontinuity design on the vote outcomes of shareholder-sponsored Say on Pay proposals at annual meetings between 2006 and 2010. This provides direct evidence on the consequences of giving shareholders more voice in the affairs of their companies through Say on Pay.

Proponents of Say on Pay argue that it strengthens shareholder oversight and can limit executive compensation excesses; critics contend that it does not effectively monitor compensation, and consider it to be a costly, intrusive policy that undermines the power of the board. This view is reflected in the fact that management is systematically opposed to the policy.<sup>1</sup> The interest in Say on Pay culminated with its inclusion in the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act, which made Say on Pay compulsory at all U.S. firms starting in 2011. This has been a highly contentious policy that continues to be a source of debate. Still, our current knowledge on the effects of the policy is limited, and the debate has been hampered by the lack of causal evidence on its consequences.<sup>2</sup>

Adopting a Say on Pay policy is correlated with multiple firm attributes and hence is highly endogenous. Given this endogeneity problem, to evaluate the consequences of Say on Pay, ideally one would like to randomly allocate this policy measure to different firms and examine their subsequent stock market reaction, performance, and pay policy changes. However, this is an impossible experiment de facto. Furthermore, investors in the stock market incorporate expectations as they receive information on the value of adopting a Say on Pay proposal. Thus, it is difficult to capture the effect of the policies using changes in market prices in the absence of clear events where unexpected information is released. We argue that Say on Pay shareholder proposals voted in annual meetings provide us with this quasi-experimental setting.

Between 2006 and 2010, shareholders in a number of S&P 1500 firms proposed to adopt Say on Pay and held a vote to adopt the policy in 258 occasions.<sup>3</sup> Our approach is to use a regression discontinuity design that compares the stock market reaction and other outcomes of Say on Pay proposals that pass by a small margin to those that fail by a small margin (similar to Mas and Lee [2012] or, in an event-study setting, to Cuñat, Gine and Guadalupe [2012]). The

intuition behind this strategy is that the average characteristics of a firm in which a Say on Pay proposal passes with 50.1 percent of the votes are similar to those of a firm in which the proposal gathers only 49.9 percent and fails to pass. However, this small difference in the vote share leads to a discrete change in the probability of implementing these proposals. In other words, for close-call proposals, passing is akin to an independent random event that is correlated with the implementation of the proposal, but it is “locally” exogenous and therefore uncorrelated with other firm characteristics. We show that for votes around the majority threshold, passing is uncorrelated with observed firm and meeting characteristics. Moreover, when studying the stock market reaction, it is precisely for these close-call proposals that the vote contains substantial information—switching from an unpredictable outcome to either pass or fail—that is not already fully incorporated in prices. Therefore, the regression discontinuity design delivers a causal estimate of the expected value of adopting Say on Pay.

We find that Say on Pay significantly increases shareholders’ value. On the day of the vote, a Say on Pay proposal that passes yields an abnormal return of 2.7 percent relative to one that fails. Given that the shareholder vote outcome is not binding, the market reaction should only account for the increase in the probability with which the proposal will be implemented after a positive shareholder vote. We collected information on whether each proposal in our sample was implemented and found that there is a 50 percent higher probability of implementation for proposals that narrowly pass at the vote threshold. This implies that implementing Say on Pay will deliver an increase in shareholder value of about 5.4 percent.

Where do these large market gains come from? In principle, there are two distinct channels through which a Say on Pay policy can improve firm performance. First, by giving a clear mechanism for shareholders to express their voice, monitoring and pressure on boards and

CEOs increases, potentially leading to an increase in performance. Second, the policy can affect the level and structure of pay such that there is a better alignment of pay to performance.

Our results show that Say on Pay has a strong positive impact on firm accounting and operational performance in the years following the vote, beyond the short-term market reaction: firms that pass Say on Pay proposals have higher growth in earnings per share, return on assets, return on equity, and Tobin's Q one year after the vote. We also find that these companies have a higher increase in labor productivity (sales per worker) one year and two years after the vote. Some of the increase in labor productivity is associated with a decline in the number of employees, but only one year after the vote. These results provide strong evidence of efficiency and profitability gains achieved through the implementation of the Say on Pay proposals.

The effects on compensation are smaller. While we do find that following a positive Say on Pay vote, firms have lower salary growth and a small increase in the sensitivity of pay to performance, we do not find large systematic changes in the level or structure of CEO compensation. We find no evidence that CEOs are more likely to leave the firm after a positive vote. Given that performance at the Say on Pay firms is improving, arguably resulting from higher effort from management, it is not surprising that there are no dramatic changes in pay: to the extent that pay is linked to performance, and performance increases, pay can remain unchanged even if shareholders are stricter on pay awards given a level of performance. Overall, while Say on Pay may tie compensation more closely to performance, our results rule out that it leads to a large and across the board reduction in the level of executive compensation.

Our results suggest that Say on Pay operates as a mechanism to monitor and incentivize CEOs to deliver better firm performance, as it creates a clear mechanism for shareholders to express their voice. This leads to large improvements in shareholder value and firm performance

for firms in our sample.<sup>4</sup> We interpret these results, together with the strong opposition of executives to adopting these policies, as indicating that current governance structure may give insufficient voice to shareholders in large corporations.

These results are therefore important to determine the appropriate role of government regulation and shareholder activism in shaping corporate governance structures. Say on Pay has been made compulsory in countries such as the Netherlands, Norway, and the UK. In the United States, the controversy around Say on Pay continues: after the 2010 Dodd-Frank Financial Regulation Act made the policy compulsory at all firms starting in 2011, the 2012 Jumpstart Our Business Startups Act eliminated that requirement for firms with gross annual revenues of less than \$1 billion. This paper provides causal evidence on the effect of Say on Pay for the early U.S. adopters. We show that Say on Pay specifically, but also more broadly giving shareholders more voice, can have substantial effects. This is a relevant result to guide the debate.

## **BACKGROUND**

Say on Pay policies are the result of a general trend toward requiring more accountability from CEOs, improved transparency, and increased shareholders rights. They emerge following an increase in the number of shareholder proposals submitted to a vote at annual meetings that focus on compensation-related matters. These proposals typically express shareholder discontent with executive pay policies and are aimed at reinforcing the pay for performance link, eliminating or reducing “exit packages,” or improving disclosure (see Ertimur, Ferri, and Muslu [2011] for an analysis of shareholders activism and pay).

Starting in 2006, shareholders of several companies proposed to adopt a Say on Pay policy in their firms. Between 2006 and 2010, 258 shareholder proposals were filed with the

SEC and voted to adopt an advisory vote. A firm that adopts a Say on Pay policy commits to offer a regular vote to its shareholders on whether they approve of the relationship between executive pay and firm performance. Companies such as Motorola, Target, Raytheon, or Pfizer were all targets of Say on Pay proposals in that period. A noteworthy case is the Verizon proposal in 2007, which was approved by a very narrow margin (50.18 percent) and implemented in 2009. Shareholders gave the following rationale for proposing to adopt Say on Pay at Verizon: “We believe that the current rules governing senior executive compensation do not give shareholders sufficient influence over pay practices—nor do they give the Board adequate feedback from the owners of the company.” This suggests that greater voice, in the form of increased “feedback” and “influence,” was an important goal. The proposal also states that Say on Pay would “. . . encourage shareholders to scrutinize the new, more extensive disclosures required by the SEC,” suggesting that the incentives for shareholders to monitor increase when they have better tools to take action (a recurrent argument in Hirschman [1970]).

The increasing focus on Say on Pay in the United States culminated with its incorporation in the Dodd-Frank Act of July 2010. The law changes several aspects of the governance and disclosure practices of all public companies. Among these changes, it provides shareholders the right to a regular advisory vote on a company’s current and future executive compensation and is mandatory for all U.S.-listed firms starting in 2011.<sup>5</sup> Proponents of the bill have argued that Say on Pay strengthens the relationship between the board of directors, executives, and shareholders, ensuring that board members fulfill their fiduciary duty. Critics argue that Say on Pay does not effectively monitor compensation, and consider it to be an intrusive policy that undermines the power of the board.



The proposal of Say on Pay policies prior to the resolution of the bill by the Senate in July 2010 (between 2006 and early 2010), received substantial support by shareholders: on average, shareholders voted 43 percent in favor of adopting Say on Pay proposals (Table 1), which is large relative to the average vote on corporate governance shareholder proposals (36 percent) or, in particular, relative to all other compensation proposals (23 percent). We now turn to the expected effects of the policy.

Given that Say on Pay is not binding, it has been argued that it should have no effect on executive and director behaviors, and hence on firm outcomes. However, given that there are potential costs associated with the vote (e.g., legal costs, costs of managing the relationship with investors), the net effect of putting in place the Say on Pay mechanism may very well be negative even if it has no effect on behavior. Say on Pay can also be detrimental to firm performance for other reasons: To the extent that the board of directors is better informed on the affairs of the company than the average shareholder, they should be better placed to make the right decisions for the firm; directors (and CEOs) may also have private information that it is in the interest of shareholders that it is not divulged to the market. In those circumstances, restricting directors' actions can be value reducing for shareholders.

There are also a number of channels through which Say on Pay proposals can positively affect firm performance. A direct channel, often echoed by popular views, is that these policies can help curb excessive pay. Indeed, Say on Pay policies may reduce the share of firm surplus that CEOs are able to capture; however, the potential gains from this effect are modest from the point of view of shareholder value. Given the size of CEO and executive pay relative to total firm value, even a substantial reduction in total pay would represent a small change in shareholder value. A slightly different channel operates through better alignment of pay with

performance: any improved incentives resulting from Say on Pay would make CEOs more effective at generating higher profits.

A Say on Pay policy is also an automatic mechanism that allows shareholders to express dissent. If the policy is adopted, Say on Pay votes are held regularly and are part of the set of votes that shareholders emit in annual meetings (along with director elections and other governance votes, for example). But Say on Pay is the only vote that allows shareholders to express a clear opinion on the relationship between pay and performance and as such is akin to a referendum on CEO performance, a vote of confidence on the CEO. This mechanism for increased shareholder voice empowers shareholders, who have a mechanism through which they can punish a CEO for poor performance. Even though the Say on Pay votes themselves are only advisory by nature, they are very visible, they aggregate shareholder opinion into a simple metric, and they can also serve as a coordination mechanism for further votes to remove management or board members. This is why they are potentially an effective “voice” mechanism.

The Say on Pay process also requires boards to disclose more information about CEO pay, and in particular about the rationale behind the chosen compensation package, including its relationship to past and planned performance. To the extent that shareholders have more information and a better way to discipline managers, their monitoring is more effective, and hence the incentives to monitor are higher.

The existing empirical literature on Say on Pay in the United States provides mixed results. Cai and Walkling (2011), using an event study methodology, find that the Say on Pay bill that passed in the House of Representatives in April 2010 created value for firms with inefficient executive compensation and with weak governance. However, they find a negative price effect

when examining the price reaction upon announcement of a shareholder Say on Pay proposal between 2006 and 2008, and a positive effect when the proposal is defeated. For the UK, Ferri and Maber (2013) examine the implementation of Say on Pay regulation in 2002 in the UK and find, also in an event study setting, a positive market reaction to the regulation in firms with weak penalties from poor performance.

One possible reason for these mixed findings is that with standard event study methodologies, the event date can be confounded by different news and information being released to the market on the same date. As we discuss below, our estimation strategy (the regression discontinuity design) actually estimates a causal effect and deals with this problem.

Finally, Balachandran, Ferri and Maber (2008) examine the effect of the UK Say on Pay regulation on pay ex post and find some evidence that it increased the sensitivity of CEO pay to poor accounting performance (but not to stock performance); that is, it curbed the “pay for failure” scenario. To date, however, there is no evidence on the impact of Say on Pay on the detailed components of pay for the United States, or more importantly, on long-term firm performance.

## **DATA AND IDENTIFICATION STRATEGY**

### **Data description**

We obtain data on Say on Pay proposals from RiskMetrics. The data set includes information on all the proposals voted in the S&P 1500 universe plus an additional 500 widely held firms. There were 258 shareholder-sponsored proposals voted at annual meetings from 2006 until 2010 to implement Say on Pay provisions. RiskMetrics provides information on the company name, the date of the annual meeting, and the percentage of votes in favor of the proposal. Panel A of Table 1 shows the distribution of proposals by year and some vote statistics.

The number of voted proposals increased throughout the period as well as the proportion of votes in favor. As a result, the percentage of passed proposals increased from 15 percent in 2007 to 25 percent in 2010. Our identification strategy relies on proposals with a close-call vote outcome. More than half of the voted proposals in our sample fall within 10 percentage points of the majority threshold and provide power to our identification.

Any shareholder that owns at least 1 percent or \$2,000 of the securities for at least one year is entitled to vote and can submit a proposal to implement a Say on Pay provision. The proponents of Say on Pay proposals are diverse and are classified in Panel B of Table 1. The most frequent sponsors are unions, followed by individuals and socially responsible funds.

We use additional information from a number of different sources: security prices from CRSP are used to calculate daily abnormal returns with a standard OLS model, and also with the three Fama-French factors plus a momentum factor (Carhart [1997]).<sup>6</sup> Financial information comes from Compustat and executive compensation from Execucomp.

### **Identification strategy**

We are interested in the impact of passing a Say on Pay proposal on an outcome variable  $y_{ft}$  such as the stock market reaction or subsequent performance and pay policies. We can define  $v_{ft}$  as the votes in favor of a Say on Pay proposal for firm  $f$  at time  $t$ ,  $v^*$  as the majority threshold for a proposal to pass and an indicator for pass as  $D_{ft} = 1(v_{ft} \geq v^*)$ , so we can write

$$y_{ft} = K + D_{ft}\theta + u_{ft}. \quad (1)$$

The effect of interest is captured by the coefficient  $\theta$ , while  $u_{ft}$  represents all other determinants of the outcome ( $E[u_{ft}] = 0$ ). However, this regression cannot be estimated directly given that passing a proposal is likely to be correlated with omitted variables that are themselves

correlated with  $y_{ft}$ . The estimated  $\hat{\theta}$  will be biased given that  $E(D_{ft}, u_{ft}) \neq 0$ . Moreover, it would be difficult to interpret the causality of the results given that some outcome variables (e.g., future expected pay structure) may affect shareholder votes and lead to reverse causality.

To obtain a causal estimate of the effect of Say on Pay proposals we use a regression discontinuity estimate that exploits that, in an arbitrarily small interval around the discontinuity (the threshold  $v^*$ ), whether the proposal passed or failed, is akin to a random outcome. Cuñat, Gine, and Guadalupe (2010) show the conditions under which one can recover the value of implementing a proposal in an event-study setting using a regression discontinuity design.

More formally, Lee (2008) shows that as long as there is a (possibly small) random component to the vote, the assignment into “treatment” (pass and  $D_{ft} = 1$ ) and “control” groups (fails and  $D_{ft} = 0$ ) is random around the threshold. A simple nonparametric way to estimate  $\hat{\theta}$  is therefore to measure the difference in average  $y_{ft}$  between Say on Pay proposals that either pass or do not by a narrow margin of votes. This is an unbiased estimate of  $\theta$  that can be interpreted as causal. However a more efficient way to estimate the effect consists of fitting a flexible function that captures the continuous relationship between  $y_{ft}$  and  $v$ , allowing for a discontinuous jump at the discontinuity  $v^*$ . Following Lee and Lemieux (2010), we approximate the underlying relationship between  $y_{ft}$  and  $v_f$ , with two different polynomials for observations on the right-hand side of the threshold  $P_r(v_{ft}, \gamma^r)$  and on the left-hand side of the threshold  $P_l(v_{ft}, \gamma^l)$ , and we also include year dummies  $\alpha_\tau$ :

$$y_{ft} = D_{ft}\theta + P_r(v_{ft}, \gamma^r) + P_l(v_{ft}, \gamma^l) + \alpha_\tau + u_{ft}. \quad (2)$$

The polynomials  $P_r(v_{ft}, \gamma^r)$  and  $P_l(v_{ft}, \gamma^l)$  capture any continuous relationship between  $y_{ft}$  and  $v_{ft}$ , and in particular, the effect of any confounding factors that are correlated both with the vote and firm characteristics in a continuous way. At the same time,  $\theta$  captures the discrete

changes in  $y_{ft}$  at the majority threshold, and it is a consistent estimate of the causal effect of the passing of a proposal on  $y_{ft}$ . This procedure is a more efficient way to estimate the effect than a simple comparison of means around the threshold, as all the observations participate in the estimation. The estimate of  $\theta$  captures the weighted average effect across all firms, where more weight is given to those firms in which a close election was expected. The order of the polynomial has to be chosen to balance having a function that is flexible enough to capture the effect of any omitted variables that are continuous at the threshold and the loss of degrees of freedom. We choose a polynomial of order four to each side of the discontinuity and after checking that the results are robust to using polynomials of order three and five.

### **Sample characteristics and preexisting differences**

In this section we investigate two selection issues that are important to understand the scope and external validity of our results (i.e., whether one could plausibly expect that our results would also apply to firms outside our sample). The first one is to assess whether the firms in our sample are representative of a broader population of firms. To do so we compare firms with a Say on Pay proposal in our sample to the general population of S&P 1500 firms. The second issue relates to the selection into treated and nontreated firms within our sample. To the extent that the exact vote outcome around the threshold is random, our identification strategy implies that there is no selection into treatment; that is, firms that pass a Say on Pay provision by few votes should be ex ante comparable to firms that reject a Say on Pay provision by a short margin. We run a number of tests to evaluate the validity of this assumption.

We start by assessing what types of firms constitute our sample. From the RiskMetrics sampling universe (S&P 1500 plus 500 additional firms that are widely held), only a subset of

firms is targeted with votes on Say on Pay, and 64 percent of those have votes within 10 percent of the threshold. To assess how different the average S&P 1500 firm is from the firms identifying our estimate, we explore the determinants that make firms more prone to having a contested Say on Pay vote. Table 2 presents summary statistics of firm characteristics for firms in our sample, as well as for the universe of S&P 1500 firms in our sample period. The one systematic difference between them appears to be firm size. Firms are different in terms of total market value, number of employees, total CEO pay, and the extent of dispersed ownership, as is expected in larger firms. However, based on ratios (that control for size), there do not seem to be systematic differences in terms of profitability (this is also shown in Cai and Walkling [2011]). Furthermore, once one controls for firm size in total CEO pay (variable labeled abnormal pay in Table 2), we do not find that CEOs of targeted firms earn more.<sup>7</sup> In fact, we find that targeted firms are typically larger and have less institutional ownership than nontargeted firms. Although these differences do not generate biases to our estimate of the treatment on the treated, they have to be taken into account when generalizing the results to a broader population of firms.

From the bottom panel of Table 3 one can also infer the typical structure of votes in our sample. Institutional investors have, on average, 70 percent of the votes, although these are quite dispersed among them. There are two shareholders with holdings above 5 percent, and the top five investors accumulate, on average, 21 percent of the votes.<sup>8</sup> There are a substantial amount of votes held by dispersed shareholders, which reduces the ex ante predictability of the vote.

Table 3 examines whether there are any of the preexisting differences between firms that pass a Say on Pay proposal and firms that don't. Columns 1 and 3 compare the characteristics of the whole population of firms, while columns 2 and 4 report only the effect at the discontinuity by including polynomials of order four on either side of the threshold. Columns 1 and 2 refer to

the variables in levels and columns 3 and 4 in growth rates. Column 1 shows that, on average, firms that pass a proposal have different characteristics than firms that fail a proposal. For instance, firms that pass a proposal have, on average, lower prior return on assets and lower earnings per share than firms that fail a proposal. These are the kinds of selection problems that would make the estimates of regression (1) biased. In contrast, when we control for a polynomial in the vote share and estimate the effect at the discontinuity (in columns 2 and 4), we find that these average differences across firms on each side of the threshold disappear. Hence, we do not find any systematic differences between firms on each side of the majority threshold.

Next, we concentrate on the distribution of shareholder votes. Figure 1 shows the distribution of votes within the sample. First, the average and median vote is slightly below the majority threshold, but 64 percent of the observations fall within 10 percentage points from the majority threshold. This implies that our regression discontinuity coefficient is estimated from a large and significant share of the actual votes and hence can be thought of as representative of the effect of Say on Pay on the average firm in our sample. Second, Figure 1 shows that the distribution of votes is also continuous at the 50 percent threshold.<sup>9</sup> The fact that there is no sharp discontinuity in the distribution of votes at the threshold indicates that there is no strategic voting or withdrawal of proposals for close-call votes. Cuñat, Gine, and Guadalupe (2012) show a similar lack of strategic voting for all shareholder-sponsored proposals, whereas Listokin (2008) documents that strategic withdrawal of proposals is a real issue for management-sponsored proposals.

Overall, this section shows that the assumptions behind our identification strategy—continuity of votes at the majority threshold and lack of preexisting differences in the neighborhood of pass—do hold and allow us to estimate a clean causal effect. It also shows that



the main distinguishing difference between firms in our sample and the sampling universe is firm size rather than profitability, which may be the result of large firms being more visible.

## **RESULTS**

### **The effect of Say on Pay on abnormal returns**

To evaluate the impact of Say on Pay proposals on shareholder value, we first examine the market reaction to passing a Say on Pay proposal. Table 4 shows estimates of the difference in abnormal returns between proposals that pass and those that do not. We compute this difference for increasingly close intervals around the majority threshold, to isolate the causal effect of Say on Pay on value, under our identification strategy. To compute abnormal returns we use two benchmarks: the market model and the four factor model (Carhart [1997]).

Columns 1–5 present nonparametric estimates, where the estimate of  $\hat{\theta}$  is the difference in abnormal returns between proposals that pass and those that do not pass for increasingly small intervals around the voting threshold. Column 1 estimates are based on the whole sample. As expected, we find that there is no difference, on average, between proposals that pass and those that fail (a small point estimate of  $-0.00270$  that is not statistically different from zero). This reflects that, for proposals that pass or fail by a large margin, the market already incorporates the expectation of vote outcomes in the prices. Columns 2 and 3 restrict the sample to within 10 and 5 percentage points of the threshold, respectively. As we narrow the margin of votes around the pass threshold, we begin to appreciate a small increase in the estimates though the standard errors are still large. For votes within 2.5 percentage points of the threshold (column 4), we observe an estimate of 1.27 percent abnormal return that is significant at the 5 percent confidence level. Finally, if we narrow the window to within 1.5 percentage points, we observe that the

estimate still follows an increasing pattern, reaching a statistically significant abnormal return of 1.65 percent.

Column 6 shows the regression for Equation (2) for the entire sample, when we allow for a discontinuous jump at the majority threshold, but we control for two polynomials of order four in the vote share on each side of it. The results are consistent with the nonparametric ones: the abnormal return of firms that pass a Say on Pay proposal is 2.7 percent higher than for firms that do not pass such proposals. The point estimate in column 6 is larger and more precisely estimated than that in column 5, but the two estimates are not statistically different.

Panel B of Table 4 shows the same set of regressions using the Carhart four factor model as an alternative benchmark. We find a similar pattern of increasing estimates as we narrow the interval around the threshold. When fitting a polynomial on each side of the threshold, we obtain an estimate of the differential abnormal return of 2.23 percent, which is statistically significant at the 5 percent level.

Another way of visualizing these results is to plot the abnormal returns on the day of the meeting. Figures 3 and 4 show the impact of passing Say on Pay proposals on abnormal returns on the day of the vote. The daily abnormal returns were calculated from CRSP using the market model for Figure 3 and the three Fama-French factors and the fourth factor model from Carhart (1997) for Figure 4. The graphs plot the smoothed average daily abnormal return for the day of the meeting ( $t = 0$ ) when the voting results are revealed.<sup>10</sup> The X-axis reflects the margin of victory (the vote share minus the threshold for that vote). On the day of the vote, Say on Pay proposals that pass by a small margin have positive abnormal returns, and comparing those to proposals that fail by a small margin gives us the differential effect of passing such proposals on abnormal returns. For votes further away from the threshold, the abnormal return is

indistinguishable from zero. As Cuñat, Gine, and Guadalupe (2012) show, in an event study–based regression discontinuity analysis, the price reaction as a function of the vote should decrease in the absolute distance to the threshold. This is because to the extent that the market has an unbiased prediction of the vote outcome prior to the vote, it should incorporate that information into prices. Therefore, for proposals that pass (or fail) by a large margin, the market should already have incorporated the value of the proposal prior to the vote, and we should have no abnormal returns on the day of the vote itself. It is only close to the majority threshold that the vote contains new information (whether the proposal effectively passes or fails) and resolves the uncertainty, triggering a market response. For proposals that narrowly pass, the adjustment is positive, and for proposals that narrowly fail, it is negative. In fact, how fast the abnormal return becomes zero as a function of the distance to the threshold is an indication of the precision with which the market was able to predict the vote.

In our data, proposals that pass with a very small margin of victory (up to 3 percent) have a positive abnormal return, and it decreases sharply with the distance to the threshold, denoting that the market is able to predict the vote outcome quite precisely.

Say on Pay proposals sponsored by shareholders have been at the center of controversy and have been closely followed by the media. Moreover, there are a variety of outlets such as news wires and real-time broadcasts that disclose the vote outcome on the same day as the annual meeting. However, even if a substantial part of the information about the vote is released on the day of the meeting, we need to explore any further gains (or potential reversals) beyond the date of the vote. Table 5 reports the regression for Equation (2), where the outcome variable  $y_{ft}$  is abnormal returns computed in different event windows around the day of the vote. We use the entire sample of data and a polynomial of order four in the vote share on each side of the

threshold. First, in column 1 the dependent variable is abnormal returns the day before the vote. The small and statistically insignificant coefficient indicates that the market cannot foresee the effect of Say on Pay the day before the vote for any of the benchmarks in Panel A and B. Second, in columns 3–5, we find that passing a Say on Pay proposal delivers abnormal returns beyond the day of the vote. Column 3 shows the impact of pass on a two-day window that includes the day of the vote and the following day. The coefficients are 3.6 percent for the market model and 3.7 percent for the Carhart four factor model, which are larger than the ones on the day of the vote and statistically significant at 5 percent level. Column 4 displays an even larger estimate for the one-week window: 3.8 percent for the market model and 5.1 percent for the Carhart four factor model. Finally, column 5 shows sustained estimates of 3.4 percent and 6.7 percent, indicating that there is no reversal one month after the vote. Standard errors are much larger at longer windows, since there are many other events driving stock prices and creating noise; however, the fact that the estimated coefficients remain stable suggests that the Say on Pay effect is persistent. Overall, we find that the large positive market reaction to passing a Say on Pay proposal is sustained and even increases following the vote.

Overall, the results in this section show that the market reacts to the passing of Say on Pay proposals with market returns of up to 5 percent of firm value. Next, we explore the different channels that could be driving this market reaction.

## **Implementation**

In this section we document how much the implementation probability of a Say on Pay proposal changes at the vote majority threshold. There are three main purposes of this section. First, given that the vote outcome on shareholder proposals is typically nonbinding, it is

important to shed some light on the implementation of Say on Pay proposals and establish whether these votes do matter. Second, our identification strategy relies on a discontinuity of (a discrete change in) the implementation probability of a Say on Pay proposal at the majority threshold, so it is important to explicitly test for this assumption. Finally, in the previous section we established the market reaction of passing a proposal. However, this market reaction takes into account the fact that proposals will be implemented with a certain probability. In order to estimate the actual value of implementing a Say on Pay proposal, we need to rescale the market reaction dividing by the discrete jump in the probability of implementation of these proposals around the vote threshold between passing and not passing.

We have gathered complete implementation data for all voted proposals (note that since the Dodd-Frank legislation was passed in mid 2010 we omit late 2010 Say on Pay votes from this analysis since the law mandated it in all firms). Table 6 displays the effect of passing a proposal on the probability of implementation. Column 1 shows an estimate of 0.55 for the whole sample, that is, the probability that a proposal is implemented is 55 percentage points higher if it passes than if it does not. This is an average estimate for all vote outcomes, but we would like to estimate whether the probability of implementation changes just around the discontinuity. To do this we replicate the analysis in Table 4 with implementation as the dependent variable. From columns 2–5 we estimate the probability of implementation as a function of passing for increasingly small intervals around the voting threshold. Passing leads to a significant 48 percentage points higher probability of implementation for proposals within 10 percent as well as within 5 percent of the majority threshold in columns 2 and 3. As we narrow the interval further to as much as 2.5 percent and 1.5 percent of the majority threshold (columns 4 and 5), the differential probability of implementation is still 40 percent and statistically

significant. Finally, column 6 displays the full model given by Equation (2) and estimated using a polynomial in the vote share of order three on each side of the threshold. We obtain a very similar coefficient of 50 percent, significant at the 5 percent significance level.

With this estimate in hand of the probability of implementation in hand, we can provide a back-of-the envelope estimate of the value of a Say on Pay proposal. Using the abnormal returns from Table 5—2.7 percent on the day of the vote and 3.6 percent for the two-day window—rescaling by a probability of implementation around the threshold of 50 percent, we estimate that the value of a Say on Pay proposal ranges from 5.4 percent to 7.2 percent. Of course, the true estimate will depend on what the expectation of the market was at the time of these votes. If the market expected a higher than 50 percent probability of implementation around the threshold, the estimate will be lower. If the expected change in the probability of implementation was 1 around the threshold, then the 2.7 percent abnormal reaction would be the actual value of the proposal as perceived by the market.

#### **4.3 The effect of Say on Pay on firm outcomes**

We have established that the market reaction to passing a Say on Pay provision is positive. This increased market value may reflect the market perception of the cost saving and managerial efficiency gains that would be induced by the Say on Pay provision. As described in the second section of this paper, there are at least two channels that can deliver better performance for these Say on Pay firms. First, through a stricter alignment of pay with performance: these improved incentives would make the CEO more effective at generating higher profits. Second, through more efficient monitoring: the annual vote on Say on Pay may work as a vote of confidence on the CEO, providing enough pressure for delivering better

performance at the risk of being dismissed if the vote does not pass. In addition, the fact that there is a new established venue for expressing shareholder voice lowers the cost of coordinating and aggregating shareholders' opinions regarding management and increases the incentives to monitoring. In this section we evaluate the real effects of Say on Pay proposals that may be induced by more intense and effective monitoring and better contractual incentives.

Tables 7 and 8 show the impact of passing a Say on Pay proposal on variables that capture firm profitability, long-term performance, and other real outcomes. Each cell corresponds to a different regression that measures the effect of passing a proposal at the discontinuity. We again use the identification strategy given by expression 2 with fourth order polynomials to each side of the majority threshold. Each column corresponds to a different dependent variable  $y_{ft}$ , and each panel to a different year-to-year effect.

We denote as year  $t$  the year in which the Say on Pay proposal is voted.<sup>11</sup> Annual meetings are held between the two fiscal year-ends, which is when the variables used in this and the following section are recorded. Therefore, we define the time periods such that there were at least six months between the annual meeting when the vote is held and fiscal year end  $t$ . This means that the change between  $t$  and  $t - 1$  includes some pretreatment months and the first few posttreatment months. The coefficients may capture early effects since most of our proposals are voted six months before the end of their fiscal year. The first panel measures changes in the variables from  $t - 1$  to  $t$ . The second panel measures the change in variables from the end of the year of the vote  $t$  until the first full year after the Say on Pay vote ( $t + 1$ ). Similarly, the bottom panel shows the change from  $t + 1$  to  $t + 2$ . Variables are winsorized at a 5 percent level.

Table 7 reports the effect of passing a Say on Pay proposal on commonly used profitability measures. We define the dependent variables in this table (earnings per share, return

on equity, and return on assets) as changes within the firm, so that we estimate the differential effect of Say on Pay on profitability across treatment and control firms, net of preexisting differences in profitability (we identify the change within these firms as a result of Say on Pay). Since these variables have large outliers, and to make sure that these are not driving the results, we also define variables stating whether the change in profitability was positive or negative.

Overall, Table 7 shows that there are no effects of Say on Pay on profitability between  $t - 1$  and  $t$ , and sizable increases in profitability between  $t$  and  $t + 1$  that are sustained through  $t + 2$ . (there are no significant reversals between  $t + 1$  and  $t + 2$ ). Let us discuss in more detail the results between  $t$  and  $t + 1$ : Columns 1 and 2 of Table 7 show that one year after the vote, firms that pass Say on Pay have a 71 percent higher chance of increasing their earnings per share (positive earnings per share growth) than firms that fail to pass such provision. The estimated magnitude of the change (column 2) is non-negligible, exceeding \$3 per share (which is around 30 percent of the standard deviation of the change in EPS). Columns 2–6 show a similar pattern for the within-firm changes in return on equity and return on assets as a result of Say on Pay. Companies that pass Say on Pay have a 67 percent (column 3) and 71 percent (column 5) higher chance of reporting an increase in those variables. Column 5 (6) shows that the average increase in ROA (ROE) as a result of Say on Pay is 20 percent (5 percent). One interpretation of these results is that CEOs at Say on Pay firms have stronger incentives to increase firm performance under this new monitoring environment.

Next, we examine other broader measures of performance beyond short-term earnings. Again, we find that there are no significant changes between  $t - 1$  and  $t$ , and that all the significant improvement occurs between  $t$  and  $t + 1$  and is sustained thereafter. Column 1 of Table 8 shows that firms that pass Say on Pay report improvements in Tobin's Q one year later.



The differential increase of 18 percent in Tobin's Q is significant, and there is no reversal over the next year. Although the effect is substantial in levels, note that the variable itself is quite volatile, with a standard deviation of 78 percent. Improvements in Tobin's Q may denote a more long term growth potential. How is this better performance attained? Column 2 shows that firms reduce costs as a result of Say on Pay as reflected in a 14 percent lower growth in overheads. We also see that they increase labor productivity, as reflected by the growth of sales per worker in column 3. Over the two years following the vote, firms that pass Say on Pay display a stronger productivity growth: 21 percent higher growth the first year and 24 percent higher growth the second year. Since these gains in productivity could come from delivering higher sales or, alternatively, from lowering employment growth. Columns 4 and 5 try to tell these channels apart. Column 6 shows that employment grows less in firms where Say on Pay passes one year after the vote: firms that pass Say on Pay lower their employment growth by 13 percent relative to those that do not pass Say on Pay. These results do not necessarily show that Say on Pay firms are cutting on employment, but rather a differential growth for Say on Pay firms relative to our control group. Two years after the vote, sales per worker continues to grow without further declines in employment. Column 5 shows that net income increases one year after the vote, and continues to increase, although not significantly two years later. This implies that while some of the increase in labor productivity was driven by a slower employment growth, it was also the result of continued sales growth. Finally, column 6 shows that firm assets did not respond in a significant way to Say on Pay.

In sum, firms that pass Say on Pay are delivering stronger performance. CEOs seem to be reacting to having a Say on Pay provision in place by providing shareholders with better earnings, as well as better Tobin's Q, which may denote more long-term firm growth

opportunities. These performance results are accompanied by better productivity ratios, a reduction in overhead and a lower employment growth rate. Say on Pay provisions seem to be pushing CEOs to deliver stronger performance. CEOs cut costs and employment growth, but also increase their labor productivity. In the next section, we examine whether Say on Pay has an effect on the level of CEO pay and on the incentive structure.

### **The effect of Say on Pay on CEO compensation**

The main stated objective of Say on Pay proposals is to improve the alignment of CEO incentives with firm objectives. In general, one should see firms as diverse in their pay policies and in how they intend to improve them. However the declared emphasis of Say on Pay proposals on improving the relationship between pay and performance often translates into common proposed practices across these firms that can be seen in the pay proposals submitted to subsequent proxy materials. For example, new incentive schemes are intended to become more explicitly linked to quantitative performance measures that are easier to monitor. Similarly, pay components that are perceived as not directly linked to performance may be challenged. In this section we examine whether passing a Say on Pay proposal has an impact on the level and on the incentive structure of CEO pay.

In Table 9 we report the effect of Say on Pay at the discontinuity threshold on changes in different elements of CEO compensation. To deal with the fact that firms in our sample are heterogeneous in size and other characteristics, we measure all the monetary variables in growth rates, so that the effects we report are in one-percentage -point changes. Coefficients can then be interpreted as the percentage change between two periods induced by Say on Pay. Column 1 reports the effect on total CEO compensation. We do not observe any significant change in the

growth rates of CEO compensation on the three years following the passing of a Say on Pay proposal. Column 2 reports the effect of Say on Pay on the probability of CEO turnover. If Say on Pay proposals induce better shareholder monitoring they may increase the probability of turnover. On the other hand, if CEOs are going to be watched more closely they could respond by performing better and, therefore, offsetting the increased monitoring and lowering the chances of being dismissed. We observe that the estimates for the effect on the probability of turnover are negative but not significant. In other words, CEOs in firms that pass Say on Pay are not more likely to leave than those in firms that do not pass Say on Pay (one cannot distinguish between voluntary quits and forced turnover with existing data). The probability of leaving (which includes dismissal) does not seem to be affected by Say on Pay proposals. Next we look into the changes on CEO compensation for firms that do not change their CEO. Column 3 reports a pattern similar to column 1, and the estimates are again not statistically different from zero. Taken together, the results in columns 1–3 show no differential effect between firms that pass Say on Pay proposals in terms of total CEO compensation or turnover.

We now turn to the different components of CEO pay. Column 4 reports the impact of passing Say on Pay on the change in salary: it decreases 4 percent one year after the Say on Pay proposal passes, and there is no reversal the following year. Given the fact that salary is a component of total compensation that is not directly linked to performance, this result is in line with the efforts to reduce the amount of compensation that is not sensitive to performance. Column 5 reports the effect on the growth of variable compensation (granting of stock, options, and bonus) and shows no particular differential pattern between firms that pass Say on Pay proposals and those that do not. Columns 6–8 look instead to the total portfolio of options and stock owned by the CEO. Columns 6 and 7 show no particular pattern in terms of the value of

the portfolio of stock (column 7) and options (column 6) held by the CEO. A decline in stock portfolio in the first panel ( $-0.509$ ) is then followed by an increase in the second panel ( $0.468$ ). Overall, there are no systematic and sustained changes in these variables as a result of Say on Pay. A similar pattern emerges in column 8, where we analyze changes in the overall delta of the portfolio of stock and options held.<sup>12</sup> An insignificant drop in the first panel is followed by a significant increase of similar magnitude and a subsequent insignificant drop. Even though one of the coefficients is significant, there is no sustained identifiable change over the period analyzed.

Overall, the results in Table 9 show that the value of total CEO compensation or its separate components do not seem to be affected by Say on Pay in a systematic and sustained way. While some of the coefficients are large, they also have large standard errors, and most of the changes we see are reversed over time.

Since we found no systematic changes in the level of compensation, we also explicitly evaluate changes in the structure of pay. All dependent variables in Table 10 are calculated as the change in the share of each pay component in total compensation (as measured in Execucomp by the variable *tdc1*). Column 1 shows that the share of bonus has a slight increase for firms that pass Say on Pay in the first period ( $0.025$ ), but this is followed by a decline of similar size ( $-0.032$ ). Columns 2 and 3 display the changes in share of stock awards and option awards relative to total compensation. Again, there is no clear pattern on the granting of new options and shares.

One possible effect of the implementation of Say on Pay is that CEOs should try to reduce unpopular or excessively visible parts of CEO pay when they do not represent a large share of total compensation. Column 4 examines the effect of passing a Say on Pay proposal on

the share of pay that is deemed as private benefits or perks. The effects are small and insignificant, suggesting no effect of Say on Pay of the propensity to award perks to CEOs. Finally, column 5 focuses on the realizations of deferred compensation. It shows that the share of deferred compensation is not affected just after Say on Pay is approved; however, we find a sizable and significant increase two years later. CEOs may be less prone to cash in already accrued earnings after Say on Pay has been approved and decide to recover them later on. This is by definition a transitory effect and may be reflecting some degree of “window dressing” in CEO pay.

Overall, the results in this section show no systematic or sustained effects of Say on Pay on CEO compensation. Total pay does not change (although we find a small decline in salary), and the different components of compensation do not change in an identifiable and consistent manner. This can be explained by two mechanisms that are not mutually exclusive. First, as seen in the previous section, firm performance substantially increases after implementing Say on Pay. CEOs are performing better because of the increase in shareholder monitoring, and as a result, they may be able to justify levels of pay that do not differ substantially from their previous ones. Second, the adjustments of the pay packages may be heterogeneous across firms. Even if there is room for improvement in CEO pay packages, there may not be systematic deviations across firms. If each firm requires a different treatment, it would induce imprecise estimates of the effect of Say on Pay. In any case, we can rule out that Say on Pay systematically curbs compensation across firms.

## **CONCLUSION**

Say on Pay policy is an important governance change mandated by the Dodd-Frank Act that provides shareholders with a vote on executive pay. It is part of a general trend toward more CEO accountability and increased shareholder rights. Shareholders may use this new channel to voice their discontent regarding the link between pay and performance. This new policy is at the forefront of the debate on executive pay and its efficacy to deliver firm performance.

However, so far it has been difficult to assess its economic impact. Its mandatory imposition is not useful to identify its effects, as it is mandated together with other changes in governance practices at the firm level. Moreover, prior voluntary adoption of Say on Pay is an endogenous decision of the firm and is correlated with firm characteristics. To overcome these difficulties we use a regression discontinuity design on the outcomes of shareholders' proposals to adopt a Say on Pay policy. This allows us to deal with the presence of prior expectations and estimate the causal effect of adopting the policy. We first show that adopting Say on Pay generates value for shareholders. Say on Pay proposals that pass yield, on average, an abnormal return of 2.7 percent relative to ones that fail on the day of the vote. This positive market reaction delivers a cumulative abnormal return of 5 percent one week after the vote. We can estimate the actual value of a Say on Pay proposal, which ranges from 5.4 percent to 7.2 percent of firm value. This is an economically sizable effect, which may arise through different potential channels.

The declared role of Say on Pay proposals is to improve CEO pay policies of firms. As such, these policies may affect firm value through better designed pay policies that motivate CEOs more efficiently. Moreover it may also help curb excessive pay, generating cost savings for the firm. Finally, the policy lowers the shareholder cost of expressing dissent, and therefore makes monitoring by shareholders more attractive and effective. We explore the relative

relevance of all of these mechanisms that could potentially be behind the shareholder reaction to the implementation of Say on Pay.

We find that firms that pass Say on Pay display stronger performance outcomes. CEOs seem to be reacting to having a provision in place by providing shareholders with better EPS marks, stronger profitability, and higher Tobin's Q. We also find better productivity ratios and a lower employment growth rate. In short, Say on Pay provisions lead to stronger firm performance.

Regarding the effect of Say on Pay on the level of compensation, we find no effect on the total CEO compensation for firms that pass the policy. In terms of the composition of pay, we do observe a decrease in the fixed salary component and an increase in the variable component of pay. Despite finding small effects on CEO pay, we cannot rule out that part of the performance effects are due to adjustments in the pay structure that provide better incentives. It is important to note that the adjustments of the pay packages may be heterogeneous across firms. Even if there is room for improvement in CEO pay packages, there may not be systematic deviations across firms. If each firm requires a different treatment, this would induce small and imprecise estimates of the effect of Say on Pay.

Our results are consistent with viewing Say on Pay policy as resembling an annual confidence vote in which shareholders approve or reject the CEO's performance relative to pay. This empowers shareholders, who have a new costless mechanism through which they can punish a CEO for poor performance. Overall our results suggest that CEOs are performing better due to the increase in shareholder monitoring and potentially due to better alignment of incentives. As a result, they may be able to justify total levels of pay that do not differ substantially from their previous ones.

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TABLE 1

**Shareholder Say on Pay Proposals**

<b>Panel A. Shareholder Proposal Summary Statistics</b>						
<b>Year</b>	<b>Voted proposals</b>	<b>Passed proposals</b>	<b>Percentage passed proposals</b>	<b>Average vote outcome</b>	<b># -5, +5</b>	<b># -10, +10</b>
2006	7	0	0%	40.2	0	5
2007	52	8	15.38%	41.26	13	32
2008	72	11	15.28%	41.38	18	44
2009	79	24	30.38%	45.67	35	54
2010	48	12	25.00%	44.83	20	35
<b>Total</b>	<b>258</b>	<b>55</b>	<b>21.37%</b>	<b>43.37</b>	<b>86</b>	<b>170</b>

**Panel B. Type of Sponsor**

<b>Type of sponsor</b>	<b>Freq.</b>	<b>Average vote</b>	<b>St. dev.</b>	<b>Min.</b>	<b>Max.</b>
Fund	8	45.68	3.90	40.6	51.5
Individual	44	40.9	10	6.8	55
Public Pen. Fund	19	49.5	11.9	25.2	69.9
Religious	21	43.37	8.7	30.4	62.4
SRI Fund	33	45.8	8.6	30	69.6
Union	68	41.38	10.1	13	69
Foundation	4	42.6	21.1	23	67
Other	6	35.5	12.5	20	52.7

NOTE: Panel A displays the frequency of Say on Pay voted proposals, the percentage passed, and the average support over time. Data are collected by RiskMetrics on all shareholders' Say on Pay proposals from 2006 until 2010 for all S&P 1500 companies, plus an additional 500 firms widely held. We have a sample of 258 voted proposals. For all of our observations the threshold for approval is 50%. Panel B classifies proposals by type of sponsor.

TABLE 2  
Descriptive Statistics and Sample Selection

	Say on Pay Target					S&P 1500		
	N	Mean	Median	Std. dev.	10th per.	90th per.	Mean	t-stat
Market value (\$mil)	257	57,354	27,389	76,953	2,574	154,375	6,783.303	10.32
Tobin Q	249	1.62	1.34	0.78	0.96	2.74	1.7	-1.71
Return on equity	257	0.06	0.14	1.14	-0.10	0.34	0.10	-0.55
Return on assets	257	0.04	0.04	0.1	-0.01	0.13	0.11	-0.86
Leverage (debt/assets)	256	0.271	0.246	0.167	0.079	0.549	0.205	6.1
Payout (dividend/net income)	256	0.29	0.22	0.82	0	0.74	0.23	1.17
Overheads	215	0.287	0.248	0.189	0.06	0.559	0.311	-1.85
Sales per worker	257	742	422	945	218.4	1,511	501.27	3.9
Log number employees	257	3.73	3.95	1.57	1.47	5.71	1.6	20.9
CEO pay (thousands)	244	15,095	12,793	14,132	3,557	25,569	5,126.1	10.9
CEO abnormal pay	245	-0.191	0.108	2.18	-0.705	0.703	-0.015	-1.26
CEO stock awards Fair Value	236	4,870.1	3,788	5,548	0	12,22	1.6	8.9
CEO option awards Fair Value	242	3,960.8	2,319	7,797	0	8,479	1,138.5	5.6
Ownership by instit. shareholders	251	0.71	0.69	0.15	0.54	0.91	0.78	-6.1
Ownership by top 5 shareholders	251	0.241	0.21	0.088	0.15	0.36	0.29	-9.1
Number shareholders own > 5%	176	2.2	2	1.22	1	4	2.8	-5.9

NOTE: Tobin' Q is defined as the market value of assets (AT+mkvalt\_f-CEQ) divided by the book value of assets (AT), and balance sheet Deferred Taxes and Investment Tax Credit (TXDITC). Book-to-market is the ratio of book value of common equity (previous fiscal year) to market value of common equity (end of previous calendar year). CEO pay is defined as TDC1 in Execucomp. All monetary values are in 2010 US\$. The number of observations may change due to missing values in some of the variables. SOURCE: Our sample consists of 258 voted proposals. All accounting variables are obtained from Compustat: Market Value (mkvalt\_f), Leverage ((DLTT+DLC)/AT), Overheads (XSGA/XOPR), Payout (DVT/NI), Return on Equity (NI/(CEQ+TXDITC)), Return on Assets (NI/AT), Sales per Worker (SALE/EMP), Log Number of Employees (log(EMP)).

**TABLE 3**  
**Pre-differences in Firm Characteristics as a Function of the Vote Outcome**

	Before meeting ( $t-1$ )		Change, from ( $t-2$ ) to ( $t-1$ )	
	(1)	(2)	(3)	(4)
<b>A.</b>				
Abnormal return one day before Meeting, car (-1, -1) OLS	-0.006 (0.004)	-0.000 (0.009)	-0.016 (0.016)	0.012 (0.027)
Abnormal return one day before Meeting, car (-1, -1) FFM	-0.006 (0.004)	0.001 (0.008)	-0.016 (0.016)	-0.002 (0.029)
<b>B.</b>				
Tobin Q	-0.110 (0.154)	0.181 (0.518)	0.016 (0.056)	0.081 (0.148)
Return on assets	-0.043* (0.022)	-0.008 (0.036)	-0.027 (0.017)	-0.021 (0.033)
Return on equity	-0.516 (0.370)	-0.409 (0.343)	-0.437 (0.393)	-0.371 (0.369)
Leverage/assets	-0.068** (0.026)	-0.020 (0.072)	0.002 (0.006)	0.003 (0.025)
Overheads (SGA/op. exp.)	-0.072** (0.035)	-0.101 (0.104)	0.002 (0.04)	-0.004 (0.006)
Earnings per share	-1.289* (0.739)	-0.446 (2.115)	0.312 (0.960)	-1.106 (3.022)
Sales per worker	409.930 (265.020)	1,134.590 (883.621)	12.174 (21.229)	69.389 (64.241)
Log sales	-0.542* (0.282)	1.187 (0.768)	-0.022 (0.028)	0.109 (0.067)
Log number employees	-0.837*** (0.261)	0.607 (0.620)	0.016 (0.032)	0.148* (0.081)
<b>C.</b>				
CEO pay	-4,765.618*** (1,732.777)	1,519.208 (3,831.642)	-2,138.331 (2,240.518)	-4,284.773 (3,465.110)
CEO abnormal pay	-0.364 (0.662)	-1.397 (1.454)	-0.105 (0.087)	0.041 (0.184)
CEO stock awards FV	-1,092.653 (823.945)	761.297 (2,447.353)	0.039 (0.042)	-0.161 (0.100)
CEO option awards FV	-2,014.357** (986.480)	-2,046.357 (1,683.134)	0.015 (0.030)	-0.046 (0.065)
<b>D.</b>				
Number proposals	-0.383* (0.225)	0.679 (0.689)	n.a.	n.a.
Dummy proposal compensation	-0.137 (0.084)	0.115 (0.238)	n.a.	n.a.
Polynomial in the vote share	no	yes	no	yes

NOTE: Panel A  $t$  refers to days, while for the rest,  $t$  refers to years. Each row corresponds to a different dependent variable and each entry comes from a separate regression. Each entry in the table reports the coefficient on whether a proposal passed. All columns control for year fixed effects and standard errors (in parentheses) are clustered at the firm level. Car OLS is estimated using the market model, car FFM is estimated using a four factor model (Fama-French three factor model plus momentum) \*significant at the 0.10 level; \*\*significant at the 0.05 level; and \*\*\*significant at the 0.01 level.

TABLE 4  
Abnormal Returns around the Majority Threshold

	(1)	(2)	(3)	(4)	(5)	(6)
<b>A. Market model</b>						
	All votes	-10; +10	-5; +5	-2.5; +2.5	-1.5; +1.5	Full Model
Pass	-0.00225 (0.00318)	0.000910 (0.00408)	0.00176 (0.00490)	0.0127** (0.00550)	0.0165** (0.00601)	0.0269*** (0.00920)
Obs	255	170	89	43	28	255
R-squared	0.009	0.000	0.002	0.119	0.198	0.042
<b>B. m-French and Momentum</b>						
	All votes	-10; +10	-5; +5	-2.5; +2.5	-1.5; +1.5	Full Model
Pass	-0.00403 (0.00322)	-0.00272 (0.00414)	-0.00263 (0.00490)	0.00749 (0.00539)	0.0127** (0.00585)	0.0229** (0.00884)
Obs.	255	170	89	43	28	255
R-squared	0.012	0.002	0.004	0.045	0.128	0.040

NOTE: All columns control for year fixed effects; standard errors are clustered by firm. \*\*significant at the 0.05 level; and \*\*\*significant at the 0.01 level.

TABLE 5  
Abnormal Returns beyond the Day of the Meeting

	(1)	(2)	(3)	(4)	(5)
<b>A. Market Model</b>					
	Day before vote	Day of vote	Two days	One week	One month
Pass	-0.000254 (0.00859)	0.0269*** (0.00920)	0.0368*** (0.0128)	0.0381 (0.0330)	0.0340 (0.0479)
Obs.	255	255	255	255	255
R-squared	0.097	0.042	0.073	0.052	0.080
<b>B. Fama French &amp; Momentum</b>					
	Day before vote	Day of vote	Two days	One week	One month
Pass	0.000776 (0.00811)	0.0229** (0.00884)	0.0376*** (0.0109)	0.0507* (0.0299)	0.0674 (0.0473)
Obs.	255	255	255	255	255
R-squared	0.061	0.040	0.085	0.063	0.036

NOTE: All columns control for year fixed effects; standard errors are clustered by firm. \*significant at the 0.10 level; \*\*significant at the 0.05 level; and \*\*\*significant at the 0.01 level.

**TABLE 6**  
**The Effect of Pass on Implementation**

	(1) All	(2) -10; +10	(3) -5; +5	(4) -2.5; +2.5	(5) -1.5; +1.5	(6) Full Model
Pass	0.548*** (0.0791)	0.487*** (0.0963)	0.482*** (0.114)	0.380** (0.171)	0.425* (0.226)	0.501** (0.237)
Observations	208	135	68	30	18	208
R-squared	0.364	0.259	0.261	0.148	0.188	0.386

NOTE: All columns control for year fixed effects; standard errors are clustered by firm. \*significant at the 0.10 level; \*\*significant at the 0.05 level; and \*\*\*significant at the 0.01 level.

TABLE 7  
Effect of Say on Pay Proposals on Firm Profitability

	(1) EPS change sign	(2) EPS change	(3) ROE change sign	(4) ROE change	(5) ROA change sign	(6) ROA change
<b>Effect from <math>t - 1</math> to <math>t</math></b>						
Say on Pay	0.0512 (0.226)	0.238 (1.069)	0.156 (0.214)	0.0770 (0.0672)	0.182 (0.214)	0.0128 (0.0218)
Obs.	257	257	257	257	257	257
R-squared	0.109	0.151	0.095	0.066	0.153	0.117
<b>Effect from <math>t</math> to <math>t + 1</math></b>						
Say on Pay	0.713*** (0.198)	3.134** (1.284)	0.673** (0.292)	0.201* (0.105)	0.713** (0.296)	0.0508* (0.0271)
Obs.	198	198	198	198	198	198
R- squared	0.197	0.198	0.149	0.146	0.183	0.216
<b>Effect from <math>t + 1</math> to <math>t + 2</math></b>						
Say on Pay	-0.298 (0.383)	-1.310 (2.062)	-0.0831 (0.388)	-0.141 (0.158)	-0.0596 (0.400)	-0.0210 (0.0526)
Obs.	115	115	115	115	115	115
R- squared	0.168	0.119	0.140	0.185	0.114	0.119

NOTE: The dependent variables obtained from Compustat are all defined as changes: Earnings per Share (EPS), Return on Equity (NI/(CEQ+TXDITC)), Return on Assets (NI/AT). All dependent variables are winsorized at the 5th and 95th percentile. Standard errors in parentheses are clustered by firm. \*significant at the 0.10 level; \*\*significant at the 0.05 level; and \*\*\*significant at the 0.01 level.



TABLE 8  
Real Effects of Say on Pay Proposals

	(1) Tobin Q growth	(2) Overheads growth	(3) Sales/worker growth	(4) Employment growth	(5) Net income growth	(6) Total assets growth
<b>Effect from <math>t - 1</math> to <math>t</math></b>						
Say on Pay	-0.000632 (0.0701)	0.0185 (0.0606)	-0.0539 (0.0590)	0.0385 (0.0403)	0.0768 (0.352)	-0.00977 (0.0615)
Obs.	248	215	257	257	257	257
R-squared	0.280	0.074	0.157	0.079	0.075	0.095
<b>Effect from <math>t</math> to <math>t + 1</math></b>						
Say on Pay	0.181*** (0.0610)	-0.140*** (0.0511)	0.215*** (0.0756)	-0.130** (0.0649)	0.923** (0.385)	0.0566 (0.0934)
Obs.	190	163	196	196	198	198
R- squared	0.329	0.173	0.145	0.101	0.138	0.125
<b>Effect from <math>t + 1</math> to <math>t + 2</math></b>						
Say on Pay	0.0398 (0.122)	-0.0545 (0.0942)	0.249* (0.130)	-0.215 (0.133)	0.410 (0.660)	-0.0156 (0.170)
Obs.	109	93	113	113	115	115
R- squared	0.222	0.069	0.121	0.126	0.194	0.158

**NOTE:** The dependent variables are obtained from Compustat are all defined in growth terms: Tobin's Q is defined as the market value of assets (AT+mkvalt\_f-CEQ) divided by the book value of assets (AT), and balance sheet Deferred Taxes and Investment Tax Credit (TXDITC), Overheads (XSGA/XOPR), Sales per Worker is defined as SALE/EMP, Employment (EMP), Net Income (EBITDA-INTPN), Total Assets (AT). All dependent variables are winsorized at the 5th and 95th percentile. Standard errors in parentheses are clustered by firm. \*significant at the 0.10 level; \*\*significant at the 0.05level; and \*\*\*significant at the 0.01 level.

TABLE 9  
Changes in the Level of Compensation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total compensation growth	Change in CEO (turnover)	Total compensation growth <i>Within CEO</i>	Salary growth <i>Within CEO</i>	Variable compensation growth <i>Within CEO</i>	Option portfolio growth <i>Within CEO</i>	Stock portfolio growth <i>Within CEO</i>	Growth delta stock & option portfolio <i>Within CEO</i>
<b>From (t-1) to (t)</b>								
Yes	-0.209 (0.211)	0.129 (0.0853)	-0.175 (0.201)	0.0103 (0.0195)	-0.172 (0.208)	-0.0866 (0.453)	-0.509* (0.266)	-0.321 (0.218)
Obs.	232	232	208	206	202	195	201	200
R-sq.	0.067	0.036	0.112	0.072	0.107	0.395	0.124	0.350
<b>From (t) to (t+1)</b>								
Yes	0.0835 (0.460)	-0.0383 (0.103)	0.116 (0.456)	-0.0408* (0.0228)	0.276 (0.773)	-0.0443 (0.578)	0.468 (0.438)	0.362** (0.147)
Obs.	178	178	158	156	155	145	152	153
R-sq.	0.056	0.052	0.081	0.090	0.079	0.316	0.200	0.382
<b>From (t+1) to (t+2)</b>								
Yes	-0.464 (0.283)	-0.111 (0.167)	-0.315 (0.207)	-0.000326 (0.0279)	-0.352 (0.263)	-0.215 (0.894)	-0.157 (0.497)	-0.504 (0.325)
Obs.	102	102	96	94	91	87	93	91
R-sq.	0.147	0.024	0.165	0.044	0.243	0.335	0.150	0.256

NOTE: All dependent variables are winsorized at the 5th and 95th percentile. Standard errors in parentheses are clustered by firm. \*\* p<0.05, \* p<0.1.

TABLE 10  
**Changes in the Structure of Compensation**

	(1)	(2)	(3)	(4)	(5)
	Share of bonus	Share of stock awards	Share of option awards	Share of perks	Share of deferred compensation
<b>Change (<i>t</i> - 1) to (<i>t</i>)</b>					
Yes	0.0254** (0.0112)	-0.130 (0.0840)	0.0521 (0.0556)	-0.00515 (0.0104)	0.0468 (0.0646)
Obs.	208	203	203	208	203
R-squared	0.224	0.063	0.085	0.102	0.340
<b>Change (<i>t</i>) to (<i>t</i> + 1)</b>					
Yes	-0.0315 (0.0272)	-0.00826 (0.126)	0.0137 (0.0739)	-0.00644 (0.0151)	-0.0926 (0.0857)
Obs.	158	158	158	158	158
R- squared	0.139	0.126	0.119	0.030	0.166
<b>Change (<i>t</i> + 1) to (<i>t</i> + 2)</b>					
Yes	-0.00110 (0.0248)	-0.0188 (0.104)	0.0481 (0.0725)	0.0121 (0.0138)	0.464*** (0.171)
Obs.	96	96	96	96	96
R- squared	0.091	0.101	0.033	0.074	0.153

NOTE: All dependent variables are winsorized at the 5th and 95th percentile. Standard errors in parentheses are clustered by firm. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ .

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## NOTES

<sup>1</sup> In the proxy materials mailed to shareholders, management states a recommendation on all proposals included by shareholders to be voted on; in all but two shareholder Say on Pay proposals in our sample, the management recommendation was to vote against the proposal.

<sup>2</sup> Furthermore, the existing evidence on Say on Pay is silent on the performance effects of the policy. Balachandran, Ferri and Maber (2008) and Ferri and Maber (2013) provide some evidence for the UK, where Say on Pay regulation was introduced in 2002, suggesting that Say on Pay increases the sensitivity of CEO pay to poor performance, that is, it may curb “pay for failure.” For the United States, Cai and Walkling (2011) do an event study using the Say on Pay bill that passed in the House in 2007 and find that returns were higher on that date in firms with inefficient compensation contracts (high abnormal CEO pay and low pay-for-performance sensitivity).

<sup>3</sup> Note that we study the votes to adopt the policy. If the policy is adopted then shareholders vote on the relationship between CEO pay and performance in subsequent meetings.

<sup>4</sup> The main difference between firms in the sample (those targeted by a Say on Pay vote between 2006 and 2010) and the rest of the S&P 1500 firms is size. Firms in the sample are clearly larger (in sales and employment), but there is no difference in operating ratios or other variables once size is controlled for. Cai and Walkling (2011) find similar differences.

<sup>5</sup> The Dodd-Frank Act required an additional vote regarding the frequency of the compensation approval vote: to occur every one, two, or three years.

<sup>6</sup> The estimation period starts two months prior to the event date; the length of the estimation period is 200 trading days, and we require at least 15 days with available returns.

<sup>7</sup> We also did not find differences when we construct a more comprehensive model of abnormal pay that controls for size and profitability, as well as year and sector dummies.

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<sup>8</sup> In none of our observations did the top five shareholders accumulate enough votes to constitute a majority of votes cast.

<sup>9</sup> A formal continuity test (McCrary 2008) rejects the discontinuity of the distribution. See Figure 2.

<sup>10</sup> The nonparametric regression uses a tri-cube weight and a bandwidth of half of the sample to each side of the discontinuity.

<sup>11</sup> This is an intuitive way to set the cut from one year to another, though our results are robust to different cuts. Most of the proxy season takes place between April and June—88 percent of the proposals in our sample take place before June.

<sup>12</sup> The total delta of the portfolio is calculated following Core and Guay (1999).