Say Pays! Shareholder Voice and Firm Performance

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This paper estimates the effects of Say-on-Pay (SoP), a policy that increases shareholder "voice" by providing shareholders with a regular vote on executive pay. We apply a regression discontinuity design to the votes on shareholder-sponsored SoP proposals. Adopting SoP leads to large increases in market value (5%) and to improvements in long-term profitability. In contrast, it has limited effects on pay levels and structure. Taken together our results suggest that SoP can be seen as a repeated regular vote of confidence on the CEO and that it serves as a disciplining device.

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1. Introduction

How much "voice" should shareholders have in a modern corporation? When shareholders disagree with the course a corporation is taking, and exercising control is not possible or too costly, there are two main mechanisms by which to express their dissent: they can either sell their shares (exit), or engage with management and express their opinions, i.e. use their "voice" (Hirschman, 1970). This paper studies the effect on shareholder value, firm performance and CEO compensation of one channel through which shareholders can voice their views: Say-on-Pay (SoP). Say-on-Pay is in essence a vote on executive pay and its relationship to firm performance. Since they are not merely voting on the level of pay but whether that level reflects the value that the CEO adds to the firm, it can be seen as an explicit vote of confidence which aggregates the opinions of all shareholders (not just the most active) into a simple, highly visible metric.

Before the introduction of Say-on-Pay, shareholders could vote on general governance provisions and compensation-related proposals.[1] To date, along with the direct election of board members, Say-on-Pay is the only mandatory mechanism imposed on U.S. firms to allow shareholders to directly and publicly vote on how the firm is run.[2]

Our goal is to provide a causal estimate of the effects of the adoption of Say-on-Pay on stock market returns and shareholder value, as well as its longer-term effects on accounting performance, firm policies, productivity and CEO compensation. To do this we use a regression discontinuity design on the vote outcomes of shareholder-sponsored Say-on-Pay proposals at annual meetings between 2006 and 2010. Previous investigations have been based on the cross-

[1] Some of the other important and related voice mechanisms that have received attention include the role of activist funds in negotiations with management (e.g. Gantchev, 2013), or more general governance proposals (e.g. Gillan and Starks, 2000, 2007; Cuñat, Gine and Guadalupe, 2012).

[2] Ferracone and Harris (2011) and Cotter, Palmiter and Thomas (2013) provide evidence that in the post Dodd Frank period, failed Say-on-Pay votes followed poor firm performance relative to CEO pay.

sectional impact of the announcement or implementation of regulatory changes, and have yielded mixed results on the effect of Say-on-Pay: Cai and Walkling (2011) find no overall effect of Say-on-Pay and negative value effects for labor union sponsored proposals, Larcker, Ormazabal and Taylor (2011) do not find a consistent pattern; Ferri and Maber (2010) and Vitanova and Iliev (2104) find generally a positive effect.

We contribute to the literature in two ways. First, we provide an identification strategy that is particularly well suited to isolate a causal effect of Say-on-Pay on stock returns. Second, we study the changes in performance, firm policies and CEO compensation resulting from the Say-on-Pay vote, which provides evidence on the channels through which Say-on-Pay operates.

Proponents of Say-on-Pay argue that it strengthens shareholder oversight and limits executive compensation excesses. Its critics counter that it undermines the power of the board and can be very costly to the firm, a view seemingly borne out by the fact that management is systematically opposed to this policy. Indeed when we looked at the proxy materials mailed to shareholders of the firms in our sample, in over 99 percent of cases management recommended a 'vote against' the shareholder-sponsored Say-on-Pay proposal.[3]

In 2010, the Dodd-Frank Wall Street Reform and Consumer Protection Act made Say-on-Pay compulsory at all U.S. firms with effect from 2011. A policy that continues to be a source of contention. However, the debate on the merits of Say-on-Pay has been hampered by a lack of causal evidence on its consequences, in particular the performance effects of Say-on-Pay beyond the short-term market reaction. The adoption of Say-on-Pay is correlated with multiple firm attributes that also affect performance and hence is highly endogenous. Moreover, since

[3] This highlights the strong resistance to these proposals by management. This opposition, which is common to most shareholder-sponsored proposals, contributes to the high costs of a garnering enough votes to pass proposals (as highlighted by Gantchev, 2013), even when passing them is associated with positive market reactions.

investors incorporate expectations as they receive information on the value of adopting a Say-on-Pay proposal, it is difficult to capture its effect on market prices in the absence of individual unexpected events where new information is released.

To address these concerns we use votes on proposals to adopt a Say-on-Pay policy at annual meetings as a quasi-experimental setting. Our sample includes 250 cases of proposals to adopt the Say-on-Pay policy filed with the SEC by shareholders of S&P 1500 firms between 2006 and 2010. Specifically, we study votes to adopt the policy in the period in which Say-on-Pay was voluntary. Hence if the policy was adopted, shareholders voted on the relationship between CEO pay and performance in a subsequent meeting. We use a regression discontinuity design that compares the stock market reaction (as well as other outcomes) to Say-on-Pay proposals that pass by a small margin with those that fail by a small margin. The intuition being that the characteristics of firms where a Say-on-Pay proposal passes with 50.1% of the vote will be similar to those where it gets 49.9% and fails to pass – yet this small difference will have a major impact on the probability of the proposals being implemented. In other words, where the vote is a 'close call', passing is akin to an independent random event that is correlated with the implementation of the proposal but is 'locally' exogenous (uncorrelated with other firm characteristics).

When we compare firms with votes around the majority threshold, we find that the passing of Say-on-Pay is uncorrelated with the observed firm and meeting characteristics. Moreover, when studying the stock market reaction, it is precisely in such close-call situations that the vote contains substantial information—switching from an unpredictable outcome to either pass or fail—that is not already fully incorporated in the stock price. Thus the regression discontinuity design delivers a causal estimate of the expected value of adopting Say-on-Pay.

Cuñat, Gine and Guadalupe (2012) likewise use a regression discontinuity design to study a set of related governance issues, finding significant positive results of passing proposals that reduce anti-takeover protection but no results on the rest of shareholder proposals. Here we focus on a specific type of proposal – Say-on-Pay – which is inherently interesting given its controversial nature and its rapid incorporation into corporate governance in response to mandatory regulation. Over our sample period there are sufficient proposals to allow for the study of Say-on-Pay using a regression discontinuity design without having to pool different types of proposal for the analysis. Furthermore, since the sample of firms and the time period here are different from those in Cuñat, Gine and Guadalupe (2012), the two papers offer complementary interpretations of the effects of different governance structures.

We find that Say-on-Pay significantly increases shareholder value. On the day of the vote, a Say-on-Pay proposal that passes yields an abnormal stock market return of 1.8% to 2.7% relative to one that fails. Since the outcome of the vote is not binding, the market reaction should only account for the increase in the probability that the proposal will be implemented after a positive shareholder vote. When we investigate whether the proposals were implemented, we observe a 40% to 50% higher probability of implementation for proposals that narrowly passed the threshold, implying that Say-on-Pay delivers an average increase in shareholder value of about 5% (3.4% to 6.75% depending on the estimates). This is of the same order of magnitude as removing two anti-takeover provisions (as estimated in Cuñat, Gine and Guadalupe, 2012). We show that this effect is driven by Say-on-Pay per se, not by other proposals voted on the same day. In addition we also show that the effect of Say-on-Pay proposals is different from other compensation-related proposals which, in contrast to the large positive response to Say-on-Pay, prompt no significant market response. This heterogeneity in the responses across compensation-related issues suggests it is not simply having a positive vote on the generic issue of compensation that matters, but a positive vote on a specific proposal, in this case, Say-on-Pay.

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 shareholders a mechanism through which to express their opinions it intensifies board monitoring and pressure on the CEO to improve performance, given that a negative Say-on-Pay vote could have significant consequences on the support for the CEO within the firm. These consequences provide implicit incentives through a broad "career concerns" motive (Fama, 1980) that may involve several channels such as turnover, long term earnings or reputation. Second, Say-on-Pay can potentially affect the current level and structure of executive pay such that it is more closely tied to performance.

Our results suggest that Say-on-Pay has a positive impact on firms' accounting and operational performance in the years following the vote (beyond the short-term market reaction). Firms that vote to adopt Say-on-Pay have higher return on assets and operating assets one year after the vote. They also show a reduction in overheads and capital expenditure growth, one year after the vote.

As for the effect of Say-on-Pay on compensation, we find no systematic change in the level or structure of CEO compensation, or the probability of the CEO leaving the firm after a positive Say-on-Pay vote. While there are significant changes in the composition of pay, these are not consistent across measures or over time, although the lack of an average effect on the level or structure of compensation may mask the possibility that different firms adjust compensation along different (and maybe opposing) dimensions. In short, we find no evidence that Say-on-Pay leads to statistically significant changes or across-the-board reductions in executive compensation.

Our results suggest that Say-on-Pay serves to monitor and incentivize CEOs to deliver better firm performance by providing a clear mechanism for shareholders to voice their opinions, as confirmed by major improvements in shareholder value and firm performance among the firms in our sample. This improvement in performance cannot be ascribed to a particular change in compensation or firm policies common to all firms, implying that "one size does not fit all" and that optimal responses may vary from one firm to another.

Given the evidence that Say-on-Pay significantly benefits shareholders, why don't all firms embrace it? Within our sample, management is systematically opposed to Say-on-Pay, yet our results suggest that where the proposal narrowly failed shareholders would have benefited from it passing, yet it did not muster sufficient support, implying a possible misalignment of objectives between management, boards and shareholders. Earlier research has documented reasons for such misalignment, for example some shareholders may have other stakes in the firm that make them vote in ways that do not maximize shareholder value.[4] Moreover, strategic voting can lead to contested votes even when a large majority of shareholders hold similar opinions.[5] Such 'deviations' from shareholder value maximization show that relevant proposals with well-defined value implications can still be contested. They also underscore how hard it is for minority shareholders to bring about change. Another possibility is that views about the value of Say-on-Pay are heterogeneous; those represented by the aggregate vote in firms differ from those of the marginal investor setting stock prices.

Since our identification strategy is based on a regression discontinuity that by design yields a local estimate, we must necessarily be cautious when extrapolating such effects to a broader population. In particular, given the high transaction costs of having a reasonable chance of winning a shareholder vote, some of the higher returns may be confined to firms in our sample which proposed to adopt Say-on-

[4] For example, banks and insurance companies tend to side with management by voting against the proposals, while mutual funds, unions, advisors, and pension funds tend to support the proposals (Brickley, Lease, and Smith (1988), Agrawal, (2008). Mutual funds also determine their vote according to whether they have other lucrative business relationships with the firms that they invest in Cvijanovic, Dasgupta, Zachariadis (2014).

[5] The strategic voting theory was introduced by Austen-Smith and Banks (1996) and Feddersen and Pesendorfer (1998). Maug and Rydqvist, (2009) show empirical evidence of strategic voting in corporate voting.

Pay – and hence the returns to implementing the proposal were largest. The fact that we find relatively large point estimates for the effect of Say on Pay may be an indication that the effects are largest in this sample. To assess the external validity of our estimates, we first compare the observable characteristics of firms around the discontinuity to firms voting a Say-on-Pay proposal with vote outcomes outside the discontinuity. We find there are no differences in observable characteristics between firms around and outside the discontinuity. We also note that 67% (35%) of all observations have votes that fall within 10 (5) percentage points of the majority threshold. This suggests that it may be possible to extrapolate the results to the set of firms outside the discontinuity – with caution. (See Appendix Table A1) Second, we compare our firms to the set of firms in the S&P 1500 that were not targets of Say-on-Pay proposals. Similar to the findings of Cai and Walkling (2011), the main difference between our firms and the rest of the S&P1500 sample is size (the difference in operating ratios or other variables is significantly reduced or disappears once size is controlled for). This fact needs to be taken into account when extrapolating our results to a broader set of firms.

Our findings contribute to the debate about the appropriate role of government regulation and shareholder activism in shaping corporate governance structures. Say-on-Pay is compulsory in many countries (e.g. U.S., Netherlands, Norway, Switzerland, U.K.). However, the controversy around Say-on-Pay continues.[6] Since this paper provides evidence that Say-on-Pay can have substantial positive effects on firm value and performance, it should help to guide the debate.

[6] While the Dodd-Frank Financial Regulation Act made Say-on-Pay compulsory as of 2011, the Jumpstart Our Business Startups (JOBS) Act of 2012 eliminated the requirement for firms with gross annual revenues of less than $1 billion.

2. Background

2.1. Say-on-Pay Policies

Say-on-Pay policies are the result of a general trend in favor of greater executive accountability, transparency and shareholder rights, which has emerged following an increase in the number of shareholder proposals on compensation-related matters submitted to a vote at annual meetings (see Ertimur, Ferri and Muslu, 2011 for an analysis of shareholder activism and pay).

Our data consist of 250 shareholder proposals filed with the SEC between 2006 and 2010 to give shareholders an advisory vote on executive pay (see Table 1). Firms that adopt Say-on-Pay commit to giving shareholders a frequent regular vote on whether executive pay is commensurate with firm performance. Companies such as Motorola, Target, Raytheon and Pfizer were all ‘targets’ of Say-on-Pay proposals in that period.[7] The increasing focus on Say-on-Pay in the U.S. culminated with its incorporation in the Dodd-Frank Act (July 2010) that regulates the governance and disclosure practices of public companies. Among other provisions, it gave shareholders the right to a regular advisory vote on current and future executive compensation. As of 2011 it has been mandatory for all U.S. listed firms.[8]

Proponents of the bill claim that Say-on-Pay strengthens the relationship between the board, executives and shareholders, ensuring that board members fulfill their fiduciary duty. Critics insist that Say-on-Pay does not effectively

[7] A noteworthy case was the Verizon Say-on-Pay proposal in 2007, which was approved by a narrow majority of 50.18%. The board decided to implement it starting 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 increased voice, in the form of increased "feedback" and "influence" was an important goal of the proposal. The proposal also stated that Say-on-Pay would "...encourage shareholders to scrutinize the new, more extensive disclosures required by the SEC," suggesting that the incentive for shareholders to monitor increases when they have better tools to take action (a recurrent argument in Hirschman, 1970).

[8] The Dodd-Frank Act required an additional vote regarding the frequency of the compensation approval vote: to occur every 1, 2, or 3 years.

monitor compensation and is an intrusive measure that undermines the board's authority.

On average, shareholders voted 43% in favor of adopting Say-on-Pay proposals (Table 1). This is larger than the average vote on corporate governance shareholder proposals (36%) and relative to all other compensation proposals (23%).

2.2. Expected effects of Say-on-Pay

Given that Say-on-Pay votes are non-binding, it could be argued that it should have no effect on executive or director behavior, and hence firm outcomes. However, given the potential costs associated with it (e.g. legal costs, cost of managing the relationship with investors), the net effect of putting Say-on-Pay in place may well be negative even if it has no effect on behavior. It may also be detrimental in other respects. For example, since the board of directors is more informed about the company than the average shareholder, it should be better placed to make decisions. Likewise, directors and CEOs may have access to information that is best withheld from the market; hence restricting their freedom to decide may be value-destroying for shareholders.

There are a number of channels through which Say-on-Pay may positively affect firm performance. A popular view is that Say-on-Pay curbs excessive executive pay, although the potential gains from the point of view of shareholder value are modest relative to total firm value. A slightly different mechanism operates via a better alignment of pay with performance: any improved incentives resulting from Say-on-Pay should make CEOs more effective at generating higher profits. Finally, Say-on-Pay allows shareholders to express dissent. Where adopted, it becomes an established part of the votes that shareholders cast at annual meetings that is likely to be as prominent as the election of new directors.

Since it is the only regular vote on the link between pay and performance, it is akin to a referendum or vote of confidence in the CEO – empowering shareholders by providing a mechanism through which they can disapprove of a CEO for poor performance.[9] Even though the outcome of the vote is purely 'advisory' (rather than binding), it aggregates shareholder opinion into a simple, visible metric and can be a means to coordinate action to remove management or board members. It gives shareholders a "voice" (Hirschman, 1970) with which they may discipline managers, making their monitoring (and the incentive to monitor) more effective.

2.3. Related Literature

The existing empirical evidence on Say-on-Pay mainly exploits announced or effective legal changes – which are arguably exogenous to the firm – combined with different ex-ante classifications of which firms should be more or less affected by the legislation. Cai and Walkling (2011), using an event study methodology, find that the Say-on-Pay bill passed in the House of Representatives in April 2010 created value for firms with inefficient executive compensation and with weak governance. Larcker, Ormazabal and Taylor (2011) examine a broader set of legislative events on several aspects of pay (including Say-on-Pay) and found no consistent pattern in market reactions to such events. Ferri and Maber (2013) examine the implementation of Say-on-Pay regulation in 2002 in the

[9] The evidence on the actual Say-on-Pay votes, once the policy is implemented is still small although the existing studies show some evidence that is consistent with the use of Say-on-Pay as some form of vote of confidence. Iliev and Vitanova (2014) show increased directorship support among firms that implement Say-on-Pay. They also present anecdotal evidence of the disciplining effects of losing or closely wining a Say-on-Pay vote. Cotter, Palmiter and Thomas (2013) explore the determinants of Say-on-Pay vote outcomes. They find that firm performance is the main determinant of Say-on-Pay votes, while the effects of CEO pay are in general small or non-existent. Ferracone and Harris (2011) also show that performance is focal to the Say on Pay votes and identify that 92% of the votes against management Say on Pay are due to a pay to performance disconnect.

United Kingdom and find a positive market reaction to the regulation in firms with weak penalties for poor performance. They also find some evidence that legislation increased the sensitivity of CEO pay to poor accounting performance (but not to stock performance), that is, it curbed "pay for failure" problems. To date, however, there is no evidence on the impact of Say-on-Pay on the detailed components of pay in the U.S. or on long-term firm performance in any of these countries.

Relative to the regulatory event-study based evidence, our approach has the advantage of allocating firms to treatment and control groups using small differences in votes, which randomly assign them around the majority threshold. This generates treatment and control groups that are comparable in observable and unobservable characteristics. Conversely, regulatory event studies require the researcher to choose these groups ex-ante and assume common trends and the absence of spillovers. A second advantage is that we study the pre-mandatory period (2006-2010) and focus explicitly on Say-on-Pay votes at the *firm level*, generating events that are idiosyncratic and at different points in time. Hence our results are unlikely to be confounded by various items, news or information being released to the market on the same date as the legislative event. Legislative events also generate aggregate changes in the overall market for CEOs and spillovers on *non-treated* firms. This is particularly relevant in this setting, in which legislative changes are often bundled, or may contain information about future policy changes. As discussed below, our estimation strategy (the regression discontinuity design) deals with this problem and actually estimates a causal effect.

Cai and Walkling (2011) also analyze firm-level proposals of shareholder Say-on-Pay between 2006 and 2008. In contrast to the findings in this paper, they find generally insignificant effects, and actually negative effects for proposals by labor unions. These are associated with negative abnormal returns when announced, and positive when defeated. A relevant methodological difference between our paper and Cai and Walkling (2011) is that they compare all the proposal announcements and outcomes (pass/fail), which could be correlated with omitted firm characteristics (such as the strength of unions within the firm). Instead, we focus on proposals around the voting majority threshold to use a form of exogenous variation.

3. Data and identification strategy

3.1. Data description

We obtained data on Say-on-Pay proposals from Riskmetrics. The dataset includes information on all the proposals voted on in the S&P1500 universe and an additional 500 widely held firms. Our sample consists of 250 shareholder-sponsored proposals voted on at annual meetings from 2006 until the 21st of July of 2010 to implement Say-on-Pay provisions.[10] Riskmetrics provides information on the company name, the date of the annual meeting and the percentage of votes in favor of the proposal[11].

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 proposals passed increased from 12% in 2007 to 26% in 2010. Our identification strategy relies on proposals with a close-call outcome. 67% (35%) of the proposals in our sample fall within ten (five) percentage points to each side of the majority threshold.

We used additional information from a number of sources: security prices from

[10] The end date of the sample is chosen to match the date in which the final bill that makes Say-on-Pay compulsory was signed. The last observation in the sample corresponds to the 11th of June of 2010. A total of 254 proposals were filed with the SEC in the sample period. To avoid the risk that our results are driven by a few proposals, throughout the paper, we drop observations with extreme abnormal returns (firms above the top and below the bottom 1%) on the day of the vote. We also drop those firms with missing abnormal returns on the day of the vote. This leaves us with a sample of 250 observations. The overall results in the paper are qualitatively similar if we use all observations.

[11] Two observations were reported to have exactly 50% of the votes in favor, so we checked whether they were considered to have passed and they did not. We therefore code them as "fail" with 49.9% of the vote.

CRSP were used to calculate daily abnormal returns with a standard unrestricted OLS market model and also with the three Fama-French factors plus a momentum factor as in Carhart (1997).[12] Financial information came from Compustat and executive compensation from Execucomp. Table 2 presents descriptive statistics of our sample and defines all the variables used in the paper.

3.2. Identification strategy

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

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

The effect of interest is captured by the coefficient θ, while the error term u_{ft} represents all other determinants of the outcome. However, this regression is unlikely to give a consistent estimate $\hat{\theta}$, for instance because passing a proposal is correlated with omitted variables that are themselves correlated with y_{ft}, or in the presence of reverse causality, such that $E(D_{ft}, u_{ft}) \neq 0$.

To obtain a causal estimate of the effect of Say-on-Pay proposals we use a regression discontinuity estimate, which exploits the fact that in an arbitrarily small interval around the discontinuity (the threshold v^*) whether the proposal passed or failed is akin to a random outcome. Lee (2008) shows that as long as there is a (possibly small) random component to the vote, the assignment to "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

[12] The estimation period is 200 days, ending two months prior to the event date.

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 θ 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^*. To do this one needs to control for the relationship between the dependent variable and the vote share in a flexible way, through some function $F(v_{ft}, \gamma)$. Adding year dummies α_τ gives us:

$$y_{ft} = D_{ft}\theta + F(v_{ft}, \gamma) + \alpha_\tau + u_{ft} \quad (2)$$

In what follows, we present two different strategies for this. First, following Lee and Lemieux (2010), we approximate the underlying relationship between y_{ft} and v_{ft}, 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 $P_l(v_{ft}, \gamma^l)$ of the threshold, such that $F(v_{ft}, \gamma) = P_r(v_{ft}, \gamma^r) + P_l(v_{ft}, \gamma^l)$. The polynomials capture any continuous relationship between y_{ft} and v_{ft}, in particular, the effect of any confounding factors that are correlated both with the vote and firm characteristics in a continuous way.[13] At the same time, θ captures the discrete changes in y_{ft} at the majority threshold, and is a consistent estimate of the causal effect of the passing of a proposal on y_{ft}.[14] 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 θ captures the weighted average

[13] We are considering other events at the annual meeting as part of the regression noise. This is correct as long as other unexpected events are not correlated with a close-call pass or fail. We confirmed that a close-call pass on Say-on-Pay does not predict whether a close-call vote on other proposals in the same meeting will pass or fail. An alternative approach is to estimate a discontinuity model for all the proposals in a meeting simultaneously, as in Cuñat, Giné, Guadalupe (2012). The results for this method yield very similar results as can be seen in Table A3.

[14] Say-on-Pay proposals are not binding. However, they have well-defined majority rules, which we use to calculate our vote margins. Given this, we are in a "fuzzy discontinuity design" setting and are estimating an Intent-to-Treat effect. To obtain the Treatment on the Treated, we could instrument whether the proposal is implemented with the vote outcome. However, as we show in Section 4.2, while discontinuous at the threshold, the vote does not predict implementation with strong enough significance to have a good first stage. Hence the paper shows Intent to Treat effects (ITT), although we provide an estimate of the treatment on the treated for the market value response by rescaling the ITT effect over the change in the probability of implementation at the discontinuity.

effect across all firms, where more weight is given to those firms in which a close election was expected. We report a polynomial of order three to each side of the discontinuity. We also checked that overall the results are robust to using polynomials of order four and five.

Second, we use the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014) (CCT). This approach approximates the regression function on either side of the majority threshold by a second order weighted polynomial regression. The weights are computed by applying a kernel function on the distance of each observation's score to the cutoff. θ is then estimated as the difference between these non-parametric regression functions on either side of the majority threshold. Calonico, Cattaneo and Titiunik (2014) improve on earlier non-parametric estimators by calculating the optimal bandwidth that overcomes limitations of earlier non-parametric RD estimators (which tend to lead to bandwidths that are too large). As we will see, the results are fairly similar for the two estimators.

Note that the combination of a regression discontinuity design with an event study setting has some additional desirable properties that are absent from traditional event studies. First, to the extent that the market can predict the vote, votes that win or lose by large margins will already be incorporated into prices prior to the vote, and thus we expect no significant price reaction far from the discontinuity. The closer the actual vote is to the discontinuity, the higher the ex-ante uncertainty that is resolved by the outcome of the vote (whether the proposal effectively passes or fails). Hence we expect the largest market response around the discontinuity. 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.[15] Second, the prior expectations of the market

[15] See Figure A4 for a more detailed explanation and Figures 3 and 4 for an empirical confirmation of these properties.

about the implementation of the proposal are identical on both sides of the discontinuity, as two firms that pass or reject the proposal by a small margin, will on average have the same prior expectation of passing it. The combination of an event study with a regression discontinuity design naturally takes care of any anticipated events prior to the vote. Appendix Figure A4 shows the predicted change in abnormal returns as a function of the vote share: it is highest (lowest) just to the right (left) of the threshold, and is closer to zero the further away the vote share is from the threshold.[16]

3.3 Sample characteristics, external validity and pre-existing differences

In this section we investigate two selection issues that are important to understand the external and internal validity of our results. The first is to assess whether the firms identifying our effect are representative of a broader population of firms. The second relates to the selection of firms within our sample into treated and non-treated firms. To the extent that the exact vote outcome around the threshold is random, our identification strategy implies there is no selection into treatment around the discontinuity, that is, firms that pass a Say-on-Pay provision by a few votes should ex-ante be comparable to firms that reject a Say-on-Pay provision by a small margin. We run a number of tests to evaluate the validity of this assumption.

First, since the Riskmetrics sample only includes the subset of firms targeted by votes on Say-on-Pay, we compare those to the population they are sampled from (S&P 1500 firms). Appendix Table A1 presents detailed summary statistics of firm characteristics for firms in our sample as well as for the universe of

[16] Cuñat, Giné, and Guadalupe (2012) give a more detailed account of these properties and show that the regression discontinuity estimate captures the expected value of the proposal (given implementation probabilities) after a positive vote. More generally, they show the conditions under which the value of implementing a proposal can be recovered in an event-study setting from the regression discontinuity estimate.

S&P1500 firms, both in 2005. A systematic difference between them appears to be firm size. Larger firms are significantly more likely to hold a Say-on-Pay vote: they have higher total market value, more employees, higher total CEO pay and less dispersed ownership[17] – all characteristics of large firms. As is common among larger firms, they also have higher leverage, and, accordingly, higher return on equity. However, once one looks at other profitability ratios that control for size and leverage, the differences become smaller or disappear (as is also shown in Cai and Walkling, 2011). Similarly, total annual CEO pay is larger in our sample relative to the whole of S&P1500 in Execucomp (an average of $15m and $5m respectively). However, if we compute the residual of total CEO pay after controlling for firm size (assets) and stock returns, the difference in pay falls below $400k and it is not statistically significant.[18] These sample characteristics, do not affect which firms are treated within our sample, and hence do not bias our estimate of the treatment effect. However, the fact that the sample tends to include the larger and more prominent firms of the S&P1500 should be taken into account when generalizing the results to a broader population of firms.[19]

Second, in Appendix Table A1 we also compare the characteristics of firms around the discontinuity (within ten percentage points of the majority threshold) to the whole sample. We find that the subsample around the discontinuity is very similar to the whole population. All the variable means are statistically the same except for a small significant difference in top-5 ownership concentration (22%

[17] In the bottom panel of Table A1 one can see the typical structure of votes in our sample. Institutional investors have on average 72% of the votes, although these are quite dispersed among them. There are on average two shareholders with holdings above 5% and the top five investors accumulate on average 24% of the votes. In none of our observations do the top 5 shareholders accumulate enough votes to constitute a majority of votes. On average, a substantial number of votes are held by dispersed shareholders, which reduces the ex-ante predictability of the vote. Note that we restrict the comparison to 2005 to avoid that the year stratification or the effects of the adoption of Say-on-Pay itself could drive the results.

[18] We compute abnormal pay as a residual of a regression of total pay on second order polynomials on assets and market returns that are allowed to vary by industry (variable labeled "abnormal pay" in Table A1).

[19] Iliev and Vitanova (2014) find results consistent with ours among smaller firms. They use an event study methodology paired with cross sectional regulatory differences to examine the effect of Say-on-Pay on firms of sizes with share float between $40 million and $110 million. This additional evidence on quite small companies complements ours and contributes to establishing the external validity of the effects found here.

vs. 24%). Overall, there is no evidence of selection into the discontinuity.

Third, in Appendix Table A2 we investigate whether there are any systematic pre-existing differences between firms that pass Say-on-Pay and those that do not. We find some differences when we compare all firms that pass Say-on-Pay to all those that do not, indicating that the decision to adopt Say-on-Pay is endogenous to firm characteristics. However, these differences mostly disappear around the discontinuity, i.e. when we estimate our main specification using firm characteristics prior to the vote as the dependent variable (a detailed discussion of the table can be found in the Appendix). This absence of observable differences around the discontinuity lends support to our identification strategy.

Finally, we analyze 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% of the observations fall within 10 percentage points of 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, Figures 1 and 2 show that the distribution of votes is continuous at the 50% threshold, suggesting that there is no strategic voting or withdrawal of proposals for close-call votes. [20]

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, allowing us to estimate a causal effect. It also shows that the main distinguishing difference between firms in our sample and the sampling universe is firm size, which should therefore be

[20] The formal continuity test in Figure 2 (see McCrary 2008) rejects the discontinuity of the distribution at the majority threshold. Cuñat, Giné and Guadalupe (2012) show a similar lack of strategic behavior around the discontinuity for all shareholder-sponsored proposals, while Listokin (2008) documents that strategic withdrawal of proposals is a real issue for management-sponsored proposals (which implies this analysis should not be done on management proposals). Note that, as long as agents are not able to precisely predict the vote, any form of strategic voting would not affect the results at the discontinuity. This applies, among others to ISS recommendations and block holders. The results in Figures 3 and 4 confirm that the market is not able to predict the vote outcomes with such precision.

taken into account when generalizing the results to a broader population of firms.

4. Results

4.1. The effect of Say-on-Pay on abnormal returns

To evaluate the impact of Say-on-Pay provisions on shareholder value we first examine the market reaction to passing a Say-on-Pay proposal. Table 3 reports estimates of the difference in abnormal returns between Say-on-Pay proposals that pass and those that do not. Columns 1 to 5 present non-parametric estimates. To isolate the causal effect of Say-on-Pay on value, under our identification strategy, we estimate θ as the difference in abnormal returns between proposals that pass and those that do not pass for increasingly small intervals around the majority 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 -0.00210 that is not statistically different from zero) reflecting that for proposals that pass or fail by a large margin, the market has already incorporated the expected vote outcome in prices. Columns 2 and 3 restrict the sample to within ten percentage points and five percentage points of the threshold, respectively. As we narrow the margin of votes around the pass threshold, we begin to see a small increase in the estimates, though the standard errors are still large. For votes within two and half percentage points of the threshold (column 4), we observe an estimate of 1.39% abnormal return that is significant at the 5% confidence level. Finally, if we narrow the window to within one and half percentage points, we observe that the estimate still follows an increasing pattern, reaching a statistically significant abnormal return of 1.88%.

Column 6 shows our main specification for the entire sample, when we allow for a discontinuous jump at the majority threshold, and control for two polynomials of order three in the vote share on each side of it. The results are consistent with the non-parametric ones: the abnormal return of firms that pass a Say-on-Pay proposal is 2.41% 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.

Finally, in Column 7 we estimate the non-parametric CCT RD estimate and find a 2.73% higher abnormal return for proposals that pass. This estimate is not statistically different from the ones in Columns 6 and 7.

Panel B of Table 3 shows the same set of regressions using as an alternative benchmark the four factor model. 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 or the CCT estimator we obtain an estimate of the differential abnormal return of 1.76% and 2.24% respectively.

Another way of visualizing these results is to plot the abnormal returns on the day of the meeting. Figure 3 shows 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 (results are similar with the four factor model). The graph plots the average daily abnormal return for the day of the meeting (t = 0) when the information of the vote is revealed; it is smoothed using a local linear regression approach. 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. Comparing those to proposals that fail by a small margin gives us the effect of passing Say-on-Pay on abnormal returns. For votes further away from the threshold the abnormal return is indistinguishable from zero. One might be concerned that outliers could drive the shape of the figure, so in Figure 4 we replicate the exercise using medians: each point in the graph computes median (instead of mean) abnormal returns of the 20 nearest vote outcomes and shows very similar results.[21]

In our data, proposals that pass with a very small margin of victory (up to 3%) have a positive abnormal return, and this decreases sharply with the distance to the threshold, suggesting that the market is able to predict votes that pass by large margins. Similarly, proposals that fail by a small margin have a negative abnormal return, and the return is decreasing in the vote share to the left of the threshold.

Even if a substantial part of the information about the vote is released on the day of the meeting, we explore any further gains (or potential reversals) beyond the day of the vote.[22] Table 4 reports the regression discontinuity estimate for abnormal returns computed in different event windows around the day of the vote using both the polynomial and non-parametric CCT estimators.

First, in column 1 the dependent variable is abnormal returns the day before the vote. The small and statistically insignificant coefficient indicates that the effect of Say-on-Pay is not foreseen by the market the day before the vote for any of the benchmarks in Panels A (Market Model) and B (Fama French and Momentum). The estimates are similar across estimation methods in each of the panels. Column 2 shows the effect on the day of the vote (identical to column 6 of Table 3). Next, in Column 3 onwards we find that passing a Say-on-Pay proposal delivers abnormal returns beyond the day of the vote. Column 3 shows the impact on a two-day window that includes the day of the vote and the following day. The coefficients are between 2.2% (CCT estimator) and 2.4% (polynomial) for the market model and 2% (CCT estimator) 2.1% (polynomial) for the four factor model, which are close to the ones on the day of the vote and statistically

[21] Each point in the y axis represents the median abnormal return (on the day of the vote) of the ten nearest votes along each side of the x axis. The discrete jumps in the graph correspond to changes in the median observation as the window changes. The advantage of this approach is that the results are not sensitive to the presence of outliers or driven by a few observations.

[22] Say-on-Pay proposals are closely followed by the media. Moreover, a variety of channels such as newswires and real-time broadcasts disclose the vote outcome on the day of the annual meeting.

significant. Column 4 displays similar estimates for the two-week window: 2% to 2.5% for the market model and 2.4% to 3.7% for the four factor model. Finally, Column 5 shows growing estimates between 3.2% and 7.2%, for cumulative returns up to six weeks; indicating that there is no reversal six weeks after the vote. Standard errors are much larger (and estimates not significant) in longer windows, since there are many other events driving stock prices and creating noise, although the fact that there is no reversal in the estimated coefficients suggests that the Say-on-Pay effect is persistent.

In principle, the only characteristic that changes discontinuously at the majority threshold is the probability of implementing Say-on-Pay. Given that Say-on-Pay is a relatively homogeneous policy, this allows us to identify Say-on-Pay as a specific channel for value creation; which is an important advantage relative to pooling different proposals as in Cuñat, Giné and Guadalupe (2012). To further understand that the captured effect is not common to all compensation-related shareholder proposals or mechanically associated with shareholders winning a close call vote we evaluate the market response to other pay related proposals. Columns 1 and 2 of Table 5 show the results of running a similar regression discontinuity design on the population of compensation-related shareholder proposals (excluding Say-on-Pay) in 1997-2010 (where data are available, in column 8) and in 2006-2010 (same sample period as the Say-on-Pay proposals, in column 9). The effect of these proposals at the discontinuity is statistically insignificant, and typically has a negative sign. This heterogeneity in the results suggests that Say-on-Pay is perceived as being quite different from other compensation-related proposals by the market: The market doesn't just respond positively to a proposal passing, but to a specific type of proposal passing. It is not the case that any positive vote triggers a positive market reaction.

Finally, to ensure that the changes we are finding are effectively related to the 50% threshold and do not occur at other points in the distribution, in Table 5 we show four placebo tests for the RD design where instead of the majority threshold we use the median vote share within the sample with vote share > 50% (column 3), the median vote share within the sample with vote share <50% (column 4), a vote cutoff of -5% (column 5) and +5% cutoff (column 6). We find there is no discontinuity in returns at those thresholds (or at any other that we tested but do not report here), confirming that there is something distinct about the actual majority threshold, as per our maintained assumption.

Overall, we find that the large positive market reaction to passing a Say-on-Pay proposal is sustained and even increases following the vote. We perform two robustness checks in Appendix table A3 related to the presence of other proposals and the dynamics of abnormal returns.[23] First, we directly control for other governance proposals that are voted on in the same annual meeting by adding them linearly to equation (2) with polynomials, and including another polynomial in the vote share for those proposals. The results on Say-on-Pay are similar (See columns 1 and 3 of Table A3) after including these controls (there is a positive effect of other proposals that is driven by anti-takeover provisions, as in Cuñat, Gine and Guadalupe 2012). Second, we use a dynamic RD estimator that estimates the effect of the vote on all periods simultaneously and also controls for other proposals (and their vote share polynomial) (see Columns 2 and 4 in Table A3). The results are again very similar. This confirms that the vote outcome of other proposals is not systematically related to the outcome of the Say-on-Pay vote around the discontinuity. In the following sections we go beyond the stock market reaction and explore the different channels that may be driving this market reaction.

4.2. Implementation

[23] The methodology in Table A3 follows Cuñat, Giné and Guadalupe (2012) and is described in the Appendix.

This section documents how much the implementation probability of a Say-on-Pay proposal changes at the majority threshold, with three main objectives in mind. First, given that the vote outcome on shareholder proposals is typically non-binding it is important to establish whether passing a proposal has an impact on implementation. Second, our identification strategy relies on a discontinuity (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.[24] Finally, while we have established the market reaction to passing a proposal, 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 re-scale the market reaction, dividing by the discrete change in the probability of implementation around the vote threshold between passing and not passing.

We collected complete implementation data from SEC filings for all voted proposals in our sample. In particular we recorded whether the proposal was implemented before the following annual meeting. The graph in Figure 5 illustrates the empirical probability of implementing a proposal using a flexible function of the vote on each side of the discontinuity.[25] The probability of implementation increases almost monotonically in the vote share, but we observe a discrete jump at the majority threshold. Table 6 estimates the size of the jump at the discontinuity. Column 1 shows that for the whole sample, a proposal that passes has a 52.5% higher probability of being implemented than one that does not. This is an average estimate for all vote outcomes, whereas we seek to estimate whether the probability of implementation changes just around the

[24] The focus on a homogeneous and well-defined policy like Say-on-Pay, joint with detailed information about implementation helps to establish that the actual policy is the main channel of the reported effects.
[25] In particular, we use a series of local linear regressions with a bandwidth of 5.2% votes as suggested by the CCT approach.

discontinuity. To do so we replicate the analysis in Table 3 and estimate how passing a proposal changes the probability of implementation for increasingly small vote intervals around the majority threshold. Intuitively, passing should lead to a lower differential probability of implementation as we narrow the interval. However, around 1.5% of the majority threshold (Column 5), the differential probability of implementation is still quite high (45.8%) and statistically significant. Columns 6 and 7 display, respectively, the polynomial based and the CCT estimates: We obtain a coefficient between 39% and 52.5%, depending on the estimation method.

With this estimate 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 3 of between 1.76% and 2.7%, and re-scaling by a probability of implementation around the threshold of 52.5%, the value of adopting a Say-on-Pay proposal is estimated to be about 3.4% to 5.1%.[26]

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 may reflect market perceptions of the potential cost-savings and managerial efficiency gains as a result of the Say-on-Pay provision. As described in Section 2, there are at least two channels by which Say-on-Pay can deliver better firm performance: first, through a stricter alignment of pay with performance; second, through more efficient monitoring and the risk of the CEO being dismissed if the outcome of the regular Say-on-Pay votes is very negative. Given that a negative outcome on the subsequent Say-on-Pay votes sends a very

[26] This re-scaling gives an approximation of the actual effect of implementation. It is equivalent to the point estimate of an IV regression. Although within our sample we cannot estimate the first stage of an IV regression with enough precision, we show in Table 3 that the jump in implementation is statistically different from zero. This is consistent with the previous governance literature that also finds that there is a jump in implementation at the majority threshold of non-binding shareholder proposals.

negative signal, the CEO may change behavior out of concern for his/her career. In this section we evaluate the effects of Say-on-Pay proposals that may result from closer monitoring and better contractual incentives.

Table 7 shows 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 show results using the non-parametric CCT estimator (results using third order polynomials on each side of the majority threshold can be found in Table A4). Each column corresponds to a different dependent variable 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. Annual meetings are held between two fiscal year ends, which is when the variables used in this and the following sections are recorded.[27] Therefore we define the time periods such that there are 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 pre-treatment months and at least six of the first post-treatment months. The dependent variables in the first panel measure changes in the variables from *t-1* to *t*. In the second they measure changes from the end of the year of the vote *t* until the first full year after the Say-on-Pay vote (*t+1*). Estimating real effects in an RDD design can be challenging in small samples for several reasons that we tackle in the following way: First, cross sectional heterogeneity could affect the results, for this reason all variables are expressed either in first differences or growth rates. Second, ratios and transformed variables could be subject to the presence of outliers, so variables are

[27] Most of the proxy season takes place between April and June - 88% of the proposals in our sample take place before June.

winsorized at the 5% level[28]. Finally, the significance threshold may require large point estimates, so tables should be read as a whole, taking into account the implications of small sample sizes on our estimates.

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 as changes within the firm to identify the within-firm effect of Say-on-Pay. Overall, Table 7 shows that there are no significant effects of Say-on-Pay between t-1 and t, but significant increases in profitability between t and t+1. More specifically, firms passing Say-on-Pay have a 5.1% higher return on assets, and a 4.7% higher return on operating assets between t and t+1. All these effects are significant and economically quite large (in relative terms, the size of the effect is 0.7 standard deviations in both cases), which is consistent with the large market value effects found earlier. They also have higher Tobin's Q (16%) and return on equity (7.5%), although these are not significant at standard levels.

How is this better performance attained? We investigate whether Say-on-Pay led to reductions in costs, which should be higher in poorly governed firms with less efficient monitoring. Column 5 of Table 7 shows a reduction in capital expenditure growth, and column 6 shows a reduction in overheads (SG&A) as a result of passing a Say-on-Pay proposal. Note that these are marginally statistically significant in Table 7 using the CCT estimator, but they are in Appendix Table A4 using the polynomial estimator. Overall, this is suggestive of an improvement in the firms' efficiency of operation and potentially of higher CEO discipline, but unfortunately we have no power to provide more than suggestive evidence.

In Figures 6 and 7 we report the graphical representation of out RD estimates,

[28] We ran all our results winsorizing at 2% with similar results; we chose to keep the more conservative cut-off (5%), to make sure our results were not driven by outliers since the 2% cut is more likely to retain extreme observations, which are more problematic when the total number of observations is relatively small.

along with 0.5% bins of data averages. We do this for the overheads and returns on assets variables, where we found significant RD results. The graphs show that the RD result seems to be quite "global" on the left hand side of the graph (i.e. the effect to the left of the threshold seems to be quite constant even as we move away from the discontinuity), but quite local on the right hand side (within 3% to 5% of the discontinuity. We think this is partly due to the fact that 60% of the observations to the right of the discontinuity fall within the 0 to 5% interval, and there are few observations beyond 5%. It may also be the case that firms that pass a proposal by more than 5% are special in some sense (e.g. shareholders are very disgruntled with management or past performance has been abnormally poor), so there are important omitted factors that are being captured by the continuous function. In any case, this highlights the fact that our estimates are local and that we need to be cautious when extrapolating.

In sum, CEOs and executives seem to be reacting to the Say-on-Pay provision by delivering better earnings and returns to shareholders. This performance improvement seems to be accompanied by improved efficiency. However, we must keep in mind the lack of power in our estimates may not allow us to estimate more significant effects.[29]

The estimates on performance are economically quite significant, which suggests that the changes in behavior accompanying Say-on-Pay around the threshold are significant and consistent with the market value response, although we cannot rule out that some of these effects are short lived or the result of earnings manipulation. The effects identified arise mainly 6 to 18 months after the Say-on-Pay proposal is passed – we cannot analyze a longer time period because in 2011 all firms were subjected to Say-on-Pay. Moreover, since these are local

[29] We looked at other potential outcomes as drivers of changes in performance measures, but were not able to identify a consistent story. For instance, we looked at whether SoP increased total payout or share repurchases, and found estimates that are not statistically different from zero.

effects, we must be cautious about extrapolation – i.e., whether we can expect such large effects to apply to firms outside our sample or far from the discontinuity. However, at the very least our results suggest that firms where proposals failed by a small margin would have benefited from it passing.

4.4. The effect of Say-on-Pay on CEO compensation

The main objective of Say-on-Pay proposals is to affect CEO pay, either by reducing it or improving the alignment of CEO incentives with firm objectives. 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 8 we report the effect of Say-on-Pay at the discontinuity threshold on changes in different elements of CEO compensation. We measure all the monetary variables in percentage growth rates so that the reported effects can be interpreted as the differential growth in the variable between firms that approve or reject Say-on-Pay by a close margin. Column 1 reports the effect on total CEO compensation. We do not observe a statistically significant change in the growth rates of CEO compensation in the two 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, CEOs may respond by performing better, offsetting the increased monitoring and lowering the chance of being dismissed. The estimates for the effect on the probability of turnover are not significant, so CEO exit is comparable between firms that pass Say-on-Pay and those that do not (one cannot accurately distinguish between voluntary and forced departures with the existing data).

Next we look into the changes on CEO compensation within firms that do not change their CEO. Column 3 reports a similar pattern to Column 1, and the estimates are again not statistically different from zero. Taken together, the results in Columns 1 to 3 show no significant effects of Say-on-Pay on total CEO compensation or turnover.

We now turn to the different components of CEO pay. Columns 4 to 8 report the impact of passing Say-on-Pay on different components of total compensation. Column 4 shows a significant change in salary after the passing of the Say-on-Pay proposal, in line with the results in Table A5 using the polynomial estimator. Column 5 reports the effect on increases in 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 to 8 focus on options and stocks. The results suggest an increase in the growth of the option portfolio (column 6), the stock portfolio (column 7) and the delta of the stock and option portfolio in the period (i.e. its sensitivity to firm value) immediately following the vote, followed by a significant increase in some of those three variables between *t* and *t+1* (column 8).[30] Note that the increase in performance-pay sensitivity could be induced by higher grants of options and shares, or more 'mechanically' through changes in the share price of firms.

Overall, the results in this section show no systematic or sustained effects of Say-on-Pay on CEO compensation. Total pay does not change significantly (other than a small decline in salary), and the different components of compensation do not change in an identifiable and consistent manner. While some results might be suggestive of a shift from fixed pay to more variable pay (consistent with the stated objectives of most Say-on-Pay proposals) this conclusion is not robust across different measures. The absence of a significant effect on pay levels or pay structure can result from Say-on-Pay having no effect on pay, but could also be explained by adjustments in pay packages that are heterogeneous across firms.

[30] The total delta of the portfolio measures the change in the dollar value of the stock and option portfolio per dollar change in the value of the firm stock and is calculated following Core and Guay (1999).

Even if there is room for improvement in CEO pay packages, the deviation from the optimal contract may vary across firms: If each firm responds in a different way or requires a different treatment, this would induce imprecise estimates of the average effect of Say-on-Pay.[31] In any case, we do not find a significant and systematic reduction in compensation across firms as a result of Say-on-Pay.

5. Conclusion

Say-on-Pay gives shareholders a mechanism to express their voice, their view on how the firm is run and on whether CEO pay policies are aligned with performance. Therefore it can affect firm value through several channels – i.e., by affecting firm performance through better designed pay structures that motivate CEOs. It also lowers the shareholder cost of expressing dissent, and therefore makes monitoring by shareholders more attractive and effective. Conversely, it can have a negative effect on performance if it imposes large costs on firms.

We find that adopting Say-on-Pay can generate substantial value for shareholders. The use of a regression discontinuity design on the outcomes of shareholders proposals to adopt a Say-on-Pay policy allows us to deal with the presence of prior expectations in an event study setting, and to estimate the causal effect of adopting the policy. Say-on-Pay proposals that pass yield, on average, an abnormal return of 1.8% to 2.7% relative to those that fail on the day of the vote. We estimate the actual value of adopting a Say-on-Pay proposal to be around 5% of firm value, an economically sizeable effect that potentially arises through different channels.

In our sample of firms that were target of Say-on-pay proposals, those that passed Say-on-Pay display stronger performance outcomes. CEOs seem to be

[31] Given our identification strategy, unfortunately the sample size does not allow us for a thorough exploration of these heterogeneous effects.

reacting to having a Say-on-Pay provision in place by delivering stronger profitability. We also find that Say-on-Pay leads to higher labor productivity and reductions in overheads and capital expenditure. In short, Say-on-Pay provisions appear to lead to more efficiency and better firm performance.

We find no significant or systematic effect of Say-on-Pay on total CEO compensation or pay composition. Despite this, we cannot rule out the idea that adjustments to pay packages may be heterogeneous across firms. Even if there is room for improvement in CEO pay packages, not all firms necessarily respond in the same way. If each firm requires different treatment, this would lead to imprecise estimates of the effect of Say-on-Pay.

We interpret the sizable and significant effects of Say on Pay on firm value and performance as potentially reflecting the fact that losing a Say-on-Pay vote is a very negative signal that increases the likelihood of future shareholder actions. These may include dismissal and can have damaging effects for CEO's reputation and careers. This "career concerns" motive creates an incentive to improve performance (Fama, 1980). Note that, many of these reputational concerns may operate off equilibrium; so, in equilibrium, we need not see any firings or shareholder actions, since the threat of Say on Pay should make CEOs take the appropriate actions to improve firm performance and reduce shareholders' concerns. However, consistent with a career concerns motive, during the 2011-2013 proxy seasons losing the Say on Pay vote triggered the dismissal of CEOs in several instances. Some salient examples are Leo Apotheker at Hewlett-Packard, David Brennar at AstraZeneca, Andrew Moss at Aviva and Vikram Pandit at Citigroup. In other cases, the failure to approve the Say on Pay vote led shareholders to file lawsuits against management to demand further changes in the executive team.

So, overall we interpret our results as reflecting the fact that Say-on-Pay empowers shareholders by offering a mechanism (a "vote of confidence") through which they can express dissent towards poorly performing CEOs, as a result of which CEOs take actions to improve firm performance. This interpretation is consistent with mounting evidence that failed Say on Pay votes in the post Dodd Frank period follow poor firm performance (Ferracone and Harris, 2011; Cotter, Palmiter and Thomas, 2013) and that Say on Pay increases the voice of small shareholders (Schwartz-Ziv and Wermers, 2014).

Thus, our paper provides evidence of the potentially large and positive effects of Say on Pay on firm value and performance which complements existing work (e.g. for UK firms in Ertimur, Ferri and Muslu 2011; for smaller US firms in Iliev and Vitanova 2014). Spelling out the precise mechanisms through which Say on Pay operates is an important topic for future research.

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Figures and Tables

Figure 1: Distribution of Votes

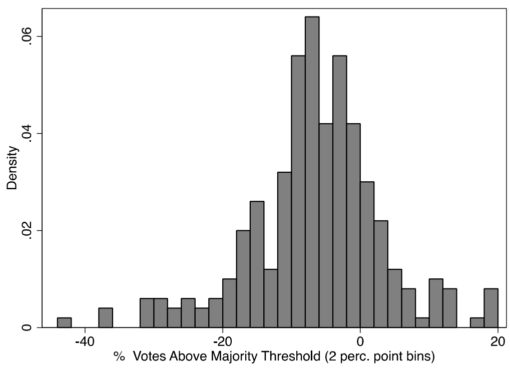

Figure 2: Continuity of Votes

Following (McCrary 2008)

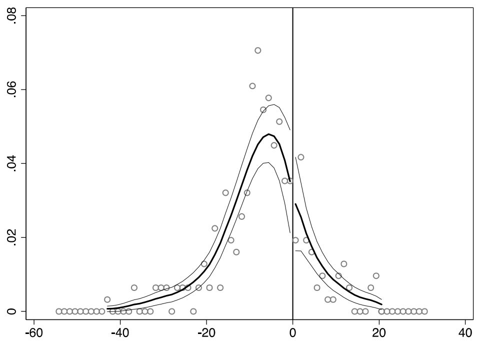

Figure 3: Abnormal Returns - Day of the Vote

Non-parametric local linear regression of market model returns using an Epanechnikov weight and a bandwidth according to CCT. Bullet points show averages in 0.5% bins.

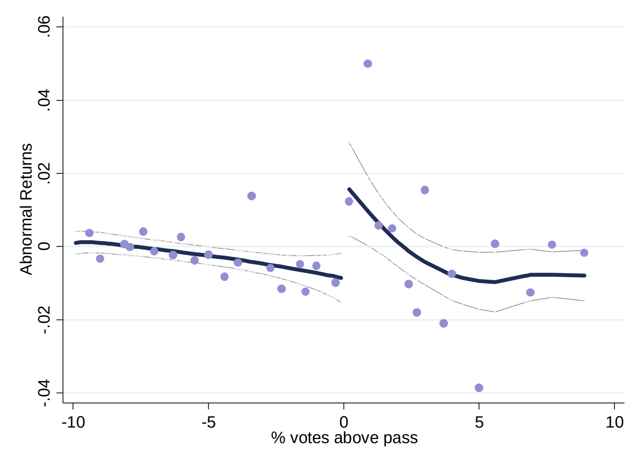

Figure 4: Abnormal Returns – Day of the Vote Medians

Median returns of market model on a window of twenty observations

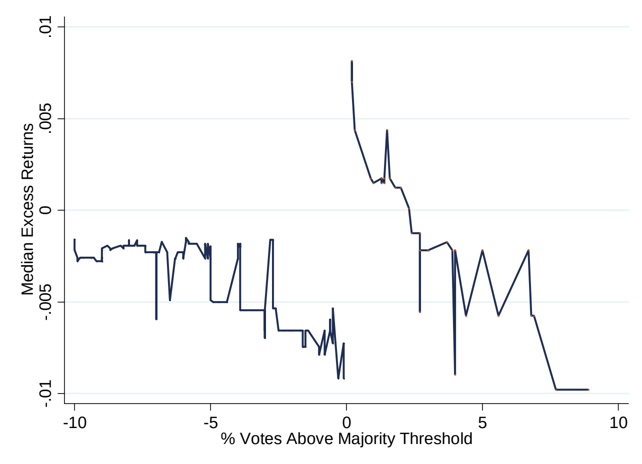

Figure 5: Probability of Implementation

Non-parametric local linear regression of the probability of implementation within one year using an Epanechnikov weight and a bandwidth according to CCT and 10% confidence intervals. Bullet points show averages in 2% bins.

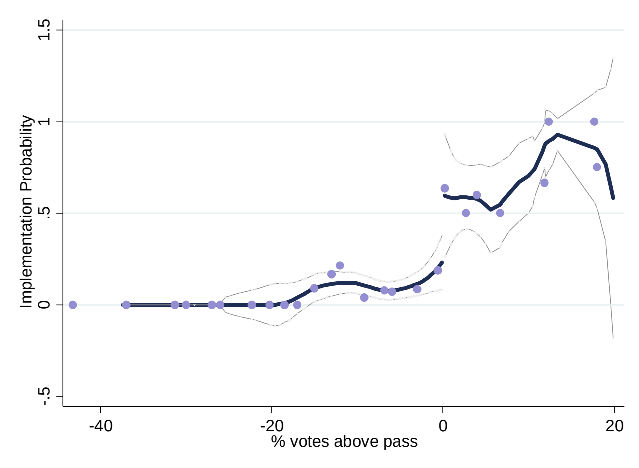

Figure 6: Overheads: t to 1+1

Non-parametric local linear regression of Overheads Change
using an Epanechnikov weight and a bandwidth according to CCT.
Bullet points show averages in 0.5% bins.

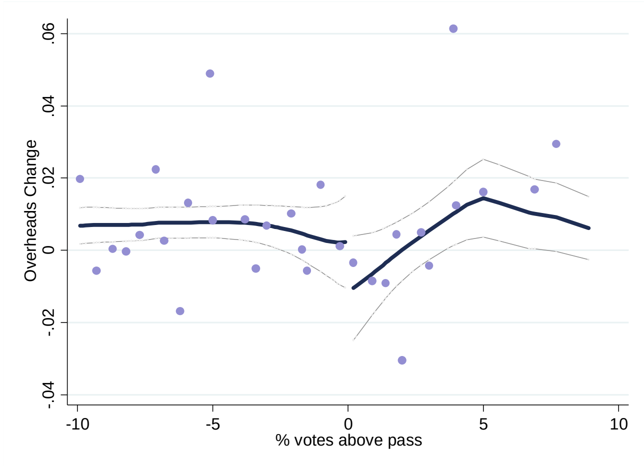

Figure 7: Return on Assets: t to 1+1

Non-parametric local linear regression of Return on Assets
using an Epanechnikov weight and a bandwidth according to CCT.
Bullet points show averages in 0.5% bins.

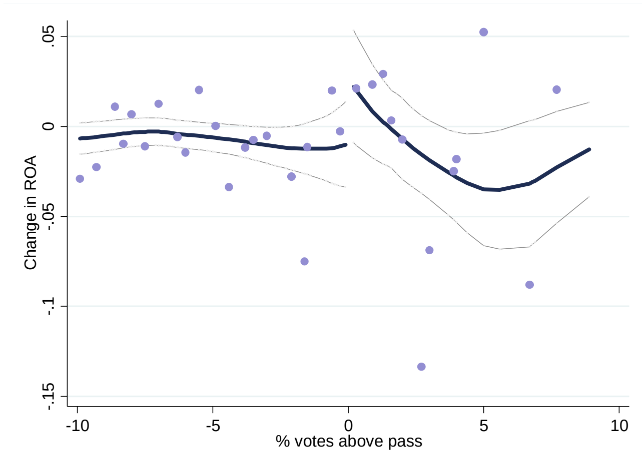

TABLE 1

Shareholder Say-on-Pay Proposals

This table displays the frequency of Say on Pay voted proposals, the percent of passed and the average support over time. Data is collected by Riskmetrics on all shareholders Say on Pay proposals from 2006 until 2010 for all S&P 1,500 companies plus an additional 500 widely held firms. For all of our observations the threshold to pass a proposal is 50%.

Shareholder Proposal Summary Statistics

Year	**Voted Proposals**	**Passed Proposals**	**Percentage Passed Proposals**	**Average Vote Outcome**	**# -5, +5**	**# -10,+10**
2006	7	0	0%	40.11	0	5
2007	51	6	11.76%	40.9	13	31
2008	68	9	13.24%	41.35	21	43
2009	78	24	30.77%	45.97	35	54
2010	46	12	26.09%	44.93	19	35
Total	250	51	20.4%	43.33	88	168

TABLE 2

Descriptive Statistics

This table describes the Say on Pay sample of 250 voted proposals. All accounting variables are obtained from Compustat: Market Value (mkvalt_f), 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), Return on Equity (NI/(CEQ+TXDITC)), Return on Assets (NI/AT), OROA (Cashflow/Total Assets), Leverage ((DLTT+DLC)/AT), Overheads (XSGA/XOPR), Total Payout ((DVT+PRSTKC)/AT), Sales per Worker (SALE/EMP), Number of Employees (EMP). CEO Pay is defined as TDC1 in Execucomp. Abnormal Pay is defined as the absolute deviation of pay residuals. Variable compensation is the sum of options and stock awards. Option portfolio is the Black-Scholes value of the options including reloads. Stock Portfolio is the total value of shares excluding options. Delta Portfolio is the delta for both the option and stock portfolios. Ownership variables are generated from Thomson 13F database. All monetary values are in 2010 US$. Note that the number of observations may change due to missing values in some of the variables.

	Mean	**Median**	**Std. dev.**	**10th Per.**	**90th Per.**		**Mean**	**Median**	**Std. dev.**	**10th Per.**	**90th Per.**
Market Value ($mil)	54,877	30,648	59,002	2,805	160,612	CEO Pay (Thou.)	15,088	13,543	10,000	4,118	30,501
Tobin Q	1.59	1.35	0.66	0.96	2.71	Abnormal Pay	-532.6	-691.74	7,792	-10,383	11,397
Earnings per Share (EPS)	2.30	2.38	2.60	0.84	5.60	Salary (Thou.)	1,337	1,237	5,961	1,472	17,002
Return on Equity	0.12	0.134	0.211	-0.10	0.35	Variable Compensation	8,323	6,918	5,961	1,472	17,002
Return on Assets	0.11	0.12	0.07	0.01	0.22	Option Portfolio	40,814	20,260	52,744	1,375	104,769
OROA (Cashflow/Total Assets)	0.08	0.09	0.065	0.002	0.16	Stock Portfolio	63,734	21,499	103,496	3,156	186,479
Net Income	3,501	2,017	4,256	-107	11,917	Delta Portfolio	1,628	747	1,979	160	4,609
Leverage (Debt/Assets)	0.27	0.24	0.16	0.08	0.55	Share of Stock Awards	0.31	0.32	0.24	0	0.67
Total Payout	0.058	0.044	0.053	0.003	0.15	Shate of Option Awards	0.23	0.21	0.21	0	0.58
Overheads (SGA/Op.Exp.)	0.28	0.25	0.17	0.06	0.55	Share of Bonus	0.04	0	0.10	0	0.20
Capex/ Assets	0.042	0.032	0.34	0.002	0.096	Share of Perks	0.03	0.02	0.04	0.004	0.08
Number Employees (Thousands)	96.7	55.4	107.2	5.8	312.02	Share of Deferred Comp.	0.016	0.001	0.101	0	0.13
Sales per Worker	653	422	584	213	1,479	Ownership by Instit. Sh.	0.72	0.71	0.12	0.56	0.89
Total Sales	44,967	26,473	48,966	2,755	119,435	Ownership by Top 5 Sh.	0.24	0.22	0.06	0.16	0.35
Total Assets	115,486	39,437	211,754	4,399	260,303						

TABLE 3

Abnormal Returns around the Majority Threshold

This table presents regressions of the abnormal returns on the day of the meeting t=0, on whether the Say-on-Pay proposal passed. Abnormal returns are computed using two benchmarks: Market Model, and Fama French and momentum factors from Carhart (1997). Column 1 estimates are based on the whole sample. Column 2 restricts the sample to observations with a vote share within ten points of the threshold, column 3 to five points and so forth. Column 6 introduces a polynomial in the vote share of order 3 (Lee and Lemieux, 2010), one on each side of the threshold, and uses the full sample. Column 7 uses the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014). All columns control for year fixed effects; standard errors are clustered by firm. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Market Model						
	Polynomial order 3						CCT
	All votes	-10;+10	-5;+5	-2.5;+2.5	-1.5;+1.5	Full Model	SoP
Pass	-0.00210	0.000462	0.00433	0.0139**	0.0188**	0.0241***	0.0273***
	(0.00316)	(0.00381)	(0.00472)	(0.00603)	(0.00696)	(0.00889)	(0.009)
Bandwidth							4.406
Obs	250	168	88	43	28	250	250
R-sq/ Z	0.017	0.000	0.013	0.140	0.253	0.091	2.89

B. Fama French & Momentum

	Polynomial order 3						CCT
	All votes	-10;+10	-5;+5	-2.5;+2.5	-1.5;+1.5	Full Model	SoP
Pass	-0.00389	-0.00320	-0.000276	0.00864	0.0151**	0.0176**	0.0224***
	(0.00320)	(0.00393)	(0.00484)	(0.00598)	(0.00678)	(0.00861)	(0.0096)
Bandwidth							4.412
Obs	250	168	88	43	28	250	250
R-sq/ Z	0.028	0.007	0.000	0.059	0.179	0.078	2.33

TABLE 4

Abnormal Returns beyond the Day of the Meeting

This table presents the effect of passing a Say-on-Pay proposal on abnormal returns around different event windows. Column 1 reports the effect of pass one day before the meeting. Column 2 reports the effect on the day of the meeting. Column 3, 4 and 5 report the effect of pass on the cumulative abnormal returns for two days, one week and one month respectively. Abnormal returns are computed using two benchmarks: Market Model and Fama French and momentum factors from Carhart (1997). The specification is equation (2) using a polynomial in the vote share of order 3 on each side of the threshold (Lee and Lemieux (2010)) and the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014). All columns control for year fixed effects; standard errors are clustered by firm. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	(1)	(2)	(3)	(4)	(5)
	A. Market Model				
	day before vote	**day of vote**	**two days**	**two weeks**	**six weeks**
Polynomial order 3					
Pass	0.00552	0.0241***	0.0242*	0.0254	0.0516
	(0.0076)	(0.0088)	(0.0128)	(0.0323)	(0.0499)
R-squared	0.113	0.091	0.085	0.060	0.088
CCT					
Pass	0.007	0.0273***	0.221**	0.020	0.032
	(0.008)	(0.009)	(0.011)	(0.032)	(0.057)
Bandwidth	5.522	4.406	4.689	4.951	4.785
Obs	250	250	250	250	250
Z	0.9	2.89	1.82	0.62	0.56

	B. Fama French & Momentum				
	day before vote	**day of vote**	**two days**	**two weeks**	**six weeks**
Polynomial order 3					
Pass	0.00236	0.0176**	0.0211**	0.0240	0.0716
	(0.0078)	(0.0086)	(0.0106)	(0.0265)	(0.0478)
R-squared	0.074	0.078	0.088	0.047	0.030
CCT					
Pass	0.005	0.0224**	0.020**	0.037	0.067
	(0.009)	(0.0096)	(0.010)	(0.027)	(0.04)
Bandwidth	4.447	4.412	4.660	4.466	4.687
Obs	250	250	250	250	250
Z	0.56	2.33	1.95	1.37	1.44

TABLE 5

Other Compensation Proposals and Placebo Tests

This table presents the effect of passing other compensation proposals on abnormal returns on the day of the meeting t=0. Abnormal returns are computed using two benchmarks: Market Model and Fama French and momentum factors from Carhart (1997). Column 1 uses all compensation proposals from 1997 to 2010 and column 2 all compensation proposal during the same period as the Say-on-Pay sample. Columns 3, 4, 5 and 6 use the Say on Pay universe with different placebo cuts as thresholds: the median vote for all SoP votes below the majority threshold in column 3; the median vote for votes above the majority thresholdin column 4; -5% from the majority threshold in column 5 and +5% in column 6. All columns use the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014) and control for year fixed effects; standard errors are clustered by firm. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
A. Market Model						
Other Compensation Proposals			**Placebo tests for Different Vote Thresholds**			
	Comp 1997-2010	Comp 2006-2010	Median Vote [-50,0)	Median Vote [0,50]	-5	+5
Pass	-0.004	-0.023	-0.004	0.007	-0.0009	-0.0061
	(0.006)	(0.015)	(0.004)	(0.015)	(0.0042)	(0.0187)
Bandwidth	12.60	5.42	5.92	3.29	6.03	5.69
Obs	1022	315	250	250	250	250
Z	-0.68	-1.50	-1.00	0.471	-0.22	-0.327
B. Fama French & Momentum						
	Comp 1997-2010	Comp 2006-2010	Median Vote [-50,0)	Median Vote [0,50]	-5	+5
Pass	-0.0003	-0.022	0.0126	-0.0015	0.0009	-0.0007
	(0.005)	(0.012)	(0.0167)	(0.0043)	(0.0045)	(0.0191)
Bandwidth	10.93	4.90	3.21	6.86	6.28	5.67
Obs	1022	315	250	250	250	250
Z	-0.06	-1.71	0.75	0.36	0.20	-0.03

TABLE 6

The Effect of Pass on Implementation

This table presents the effect of passing a Say-on-Pay proposal on Implementation. Column 1 estimates are based on the whole sample. Column 2 restricts the sample to observations with a vote share within ten points of the threshold, column 3 to five points and so forth. Column 6 introduces a polynomial in the vote share of order 3 (Lee and Lemieux (2010)), one on each side of the threshold, and uses the full sample. Column 7 uses the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014). All columns control for year fixed effects; standard errors are clustered by firm. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	-10;+10	-5;+5	-2.5;+2.5	-1.5;+1.5	Full Model	CCT
Pass	0.525***	0.470***	0.482***	0.393**	0.458**	0.525**	0.39*
	(0.0810)	(0.0981)	(0.114)	(0.167)	(0.214)	(0.240)	(0.24)
Bandwidth							5.23
Obs	201	132	68	31	20	201	201
R-sq/Z	0.344	0.241	0.261	0.159	0.222	0.365	1.6

TABLE 7

Effect of Say-on-Pay Proposals on Firm Profitability

This table presents the effect of passing a Say-on-Pay proposal on firm profitability. The specification is equation (2) using the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014). The dependent variables are obtained from Compustat are all defined as changes. Column 1 reports changes in Tobin's Q, which 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). Column 2, 3 and 4 report the change in Return on Equity (NI/(CEQ+TXDITC)), in Return on Assets (NI/AT) and in the Operating Return on Assets (CashFlow /AT), respectivewly. Column 5 and 6 report changes in the Capex ratio (Capex/AT) and in Overheads (XSGA/XOPR). All dependent variables are winsorized at the 5th and 95th percentile. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Tobin Q Change	**ROE Change**	**ROA Change**	**OROA (CashFlow /AT) Change**	**Capex/ Total Assets Change**	**Overheads Change**
Effect from t-1 to t						
Pass	0.111	0.06	0.017	0.025	-0.013	0.006
	(0.114)	(0.058)	(0.02)	(0.01)	0.006	0.007
Bandwidth	5.59	4.35	4.08	4.29	4.92	4.76
Obs.	241	250	250	247	245	209
Z	0.97	1.05	0.70	1.27	-2.0	0.89
Effect from t to t+1						
Pass	0.157	0.075	0.051***	0.047***	-0.0085	-0.013*
	(0.12)	(0.059)	(0.017)	(0.017)	(0.0056)	(0.0078)
Bandwidth	8.47	6.14	4.78	4.57	6.53	5.08
Obs.	184	192	192	188	189	158
Z	1.09	1.3	2.77	2.49	-1.51	-1.69

TABLE 8

Changes in the Level of Compensation

This table presents the effect of passing a Say-on-Pay proposal on compensation measures. The specification is equation (2) using the non-parametric approach proposed by Calonico, Cattaneo and Titiunik (2014).The dependent variables are obtained from Execucomp. Column 1 reports growth in Total Compensation (TDC1), column 2 the change in CEO Turnover and column 3 growth in Total Compensation within CEO. Column 4 reports growth in Salary and column 5 growth in Variable Compensation (Stock_awards_fv+Option_awards_fv+Bonus+ Noneq_Incent). Column 6 and 7 report growth in Option and Stock Portfolio, respectively. Column 8 reports growth in Stock and Option Portfolio Delta. All dependent variables are winsorized at the 5th and 95th percentile. All columns control for year fixed effects. Standard errors in parentheses are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

	(1) **Total Compensation Growth**	(2) **Change in CEO (Turnover)**	(3) **Total Compensation Growth**	(4) **Salary Growth**	(5) **Variable Compensation Growth**	(6) **Option Portfolio Growth**	(7) **Stock Portfolio Growth**	(8) **Delta Growth Stock & Option Portfolio**
			Within CEO	*Within CEO*	*Within CEO*	*Within CEO*	*Within CEO*	*Within CEO*
Effect from t-1 to t								
Pass	0.016	0.099	0.030	0.0007	-0.008	0.147	-0.124	-0.13
	(0.132)	(0.065)	(0.128)	(0.016)	(0.150)	(0.479)	(0.254)	(0.228)
Bandwidth	4.72	4.77	6.33	6.59	5.41	5.61	4.91	5.69
Obs	233	238	210	208	201	194	204	201
Z	0.124	1.51	0.23	0.04	-0.05	0.30	-0.48	-0.57
Effect from t to t+1								
Pass	-0.43	-0.043	-0.435	-0.041*	-0.485	1.05***	0.212	0.448**
	(0.273)	(0.059)	(0.281)	(0.024)	(0.347)	(0.314)	(0.40)	(0.220)
Bandwidth	4.54	4.92	4.68	6.27	4.83	5.43	6.37	4.81
Obs	179	179	159	157	153	143	153	154
Z	-1.59	-0.73	-1.54	-1.68	-1.39	3.35	0.52	2.00

Appendix

TABLE A1

Sample Selection

This table compares the Say on Pay sample of 250 voted proposals with the SP1500 universe for the same years. As well, it compares the Say on Pay Close votes sample (within 10 percentage points) with the full Say on Pay sample. All accounting variables are obtained from Compustat: Market Value (mkvalt_f), 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), Return on Equity (NI/(CEQ+TXDITC)), Return on Assets (NI/AT), OROA (Cashflow/Total Assets), Leverage ((DLTT+DLC)/AT), Overheads (XSGA/XOPR), Total Payout ((DVT+PRSTKC)/AT), Sales per Worker (SALE/EMP), Number of Employees (EMP). CEO Pay is defined as TDC1 in Execucomp. Abnormal Pay is defined as the absolute deviation of pay residuals. Ownership variables are generated from Thomson 13F database. All monetary values are in 2010 US$. Note that the number of observations may change due to missing values in some of the variables.

	SoP vs. SP1500			**SoP Close (+10/-10) vs. SoP**	
	Mean SoP	**Mean SP1500**	**t-test**	**Mean Close**	**t-test**
Market Value ($mil)	54,877	6,749	12.8	65,326	0.55
Tobin Q	1.6	1.76	-3.8	1.67	0.60
Return on Equity	0.12	0.10	1.3	0.04	-0.13
Return on Assets	0.11	0.12	-0.83	0.119	0.64
OROA (Cashflow/Total Assets)	0.086	0.091	-1.22	0.084	0.60
Leverage (Debt/Assets)	0.27	0.20	6.5	0.26	-0.46
Total Payout	0.058	0.044	4	0.069	1.48
Overheads (SGA/Op.Exp.)	0.28	0.31	-2.0	0.27	-0.55
Number Employees (Thousands)	96.7	15.6	11.2	101	-1.25
Sales per Worker	653	488	4.4	832	0.86
CEO Pay (Thousands)	15,088	5,204	15.4	15,875	-0.26
Abnormal Pay	-532.6	-171	-0.72	-716	-0.54
Ownership by Instit. Shareholders	0.72	0.78	-7.8	0.71	-0.59
Ownership by Top 5 Shareholders	0.24	0.29	-11.4	0.22	-2.1
Number Shareholders own > 5%	2.0	2.7	-8.01	1.9	-1.51

TABLE A2

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

Table A2 tests whether passing a Say-on-Pay vote on the meeting date is systematically related to firm characteristics prior to the meeting. Note that in 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. Columns 1 and 2 (3 and 4) report the estimated effect of passing a vote on outcome variable levels (changes) the year before the annual meeting, t-1 (between t-2 and t-1). Columns 1 and 3 present estimates without controlling for a polynomial in the vote share and, therefore, estimate the average effect of passing relative to not passing. Columns 2 and 4 include the polynomial in the vote share of order 3 on each side of the threshold such that it effectively estimates the effect at the discontinuity. All columns control for year fixed effects and standard errors (in parenthesis) are clustered at the firm level. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	Before meeting (t-1)		**Change, from (t-2) to (t-1)**	
	(1)	(2)	(3)	(4)
A.				
Abnormal Return one day before Meeting, Car (-1,-1) OLS	-0.007*	0.006	-0.020	0.021
	(0.004)	(0.008)	(0.017)	(0.030)
Abnormal Return one day before Meeting, Car (-1,-1) FFM	-0.007*	0.002	-0.020	0.003
	(0.004)	(0.008)	(0.017)	(0.033)
B.				
Tobin Q	-0.179	-0.192	0.017	0.098
	(0.160)	(0.505)	(0.059)	(0.137)
Return on Assets	-0.047**	0.002	-0.033*	-0.021
	(0.023)	(0.057)	(0.017)	(0.045)
OROA --Cash Flow	-0.043**	0.016	-0.031**	-0.024
	(0.021)	(0.049)	(0.014)	(0.037)
Return on Equity	-0.478	-0.917	-0.421	-0.929
	(0.324)	(0.751)	(0.334)	(0.778)
Leverage/ Assets	-0.075***	-0.089	0.004	0.019
	(0.026)	(0.062)	(0.006)	(0.021)
Overheads (SGA/Op. Exp.)	-0.078**	-0.209**	0.000	0.003
	(0.036)	(0.091)	(0.004)	(0.009)
Earnings Per Share	-1.302*	-1.795	-0.376	-3.362
	(0.766)	(2.135)	(0.696)	(2.396)
Sales	-22,864.203*	42,287.107	145.589	9,261.578
	(12,607.828)	(30,612.852)	(1,573.873)	(6,311.584)
Number Employees (Thousands)	-84.706*	-61.275	2.326	5.703
	(44.568)	(92.892)	(2.771)	(5.134)
C.				
Ceo Pay (Thousands)	-4,768.8***	4,195.7	-2,120.6	302.1
	(1,767.3)	(4,094.9)	(2,283.9)	(5,326.8)
Ceo Stock Awards FV (Thousands)	-1,083.9	1,359.6	595.3	1,885.0
	(840.6)	(2,480.6)	(869.7)	(2,222.6)
Ceo Option Awards FV (Thousands)	-2,027.8**	1,234.7	-754.9	-4,437.7*
	(1,024.3)	(1,426.1)	(941.6)	(2,465.0)
D.				
Number Proposals	-0.370	0.686	n.a.	n.a.
	(0.233)	(0.894)		
Dummy Proposal Compensation	-0.130	0.100	n.a.	n.a.
	(0.088)	(0.279)		
Polynomial in the vote share	no	yes	no	yes

In Table A2 we examine whether there are any pre-existing differences at the majority threshold 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 three on either side of the threshold. Columns 1 and 2 refer to the variables in levels and 3 and 4 in growth rates.

Column 1 shows that, on average, firms that pass the proposal have different characteristics from those where the proposal fails. For instance, firms where the proposal is passed have on average lower prior return on assets than those where it fails. These are the kind of selection problems that would make the estimates of a standard OLS regression biased. In contrast, when we control for a polynomial in the vote share and estimate the effect at the discontinuity (in column 2 and 4), we find that these average differences across firms on each side of the threshold disappear. We do find some differences in the level of overheads and the growth rates of option grants although given the number of coefficients that we check it is expected that some of them would seem statistically different even if both samples are drawn from the same distribution.

In general, we do not find any systematic differences between firms on each side of the majority threshold.

TABLE A3

Abnormal Returns Controlling for Other Proposals

Abnormal returns are computed using two benchmarks: a market model (in Columns 1 and 2) and a four factor model (Fama French and momentum factors; Carhart,1997) (in Columns 3 and 4). Columns 1 and 3 include as controls the vote outcome of other proposals in the same meeting, third order vote polynomials to each side of the discontinuity different for SoP votes and other votes and year dummies. Columns 2 and 4 include a dynamic specification and firm fixed effects, similar to Cuñat, Gine and Guadalupe (2012). The sample includes all votes from 2006 until June 2010. We drop observations outside the top (bottom) 1% of abnormal returns of the full sample. All columns control for year fixed effects and standard errors (in parenthesis) are clustered at the firm level. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	(1)	(2)	(3)	(4)
	Market Model		**Fama French & Momentum**	
Say on Pay Proposals				
Day of the vote, *t*	0.021**	0.027***	0.014*	0.018*
	(0.009)	(0.010)	(0.008)	(0.010)
One day later, *t+1*		0.010		0.007
		(0.010)		(0.010)
Days *t+2* to *t+9*		-0.018		-0.013
		(0.024)		(0.021)
Other Proposals				
Day of the vote, *t*	0.006**	0.008**	0.004	0.006
	(0.003)	(0.004)	(0.003)	(0.004)
One day later, *t+1*		0.004		0.001
		(0.006)		(0.005)
Days *t+2* to *t+9*		0.020		0.016
		(0.013)		(0.011)
Observations	1,024	5,120	1,024	5,120
R-squared	0.044	0.025	0.034	0.012

The specification in columns (1) and (3) builds on the main specification in this paper (equation (2)) to control for the average effect of all other governance proposals voted in the annual meeting by adding a second RD structure to the specification that is identical in structure to the one used for Say-on-Pay proposals. The specification uses the universe of governance-related shareholder proposals in firms that hold a Say-on-Pay vote. We split them into Say-on-Pay proposals and other proposals and estimate:

$$y_{ft} = D^A{}_{ft}\theta^A + P^A{}_r(v^A{}_{ft}, \gamma^{Ar}) + P^A{}_l(v^A{}_{ft}, \gamma^{Al}) + D^B{}_{ft}\theta^B + P^B{}_r(v^B{}_{ft}, \gamma^{Br}) + P^B{}_l(v^B{}_{ft}, \gamma^{Bl}) + \alpha_\tau + u_{ft}$$

Where the superindex A refers to Say-on-Pay proposals and B refers to other governance proposals. The reported coefficients are θ^A and θ^B. Whenever there is more than one B type proposal in a given meeting, we aggregate them (and their vote shares) linearly, using the same method as in Cuñat, Giné and Guadalupe (2012).

Columns (2) and (4), additionally, allow for the possibility of dynamic effects and interactions between the abnormal returns at different time horizons. Following Cellini, Ferreira, and Rothstein (2010) we pool the abnormal returns at different time horizons and estimate the following regression.

$$y_{ft+T} = D^A{}_{ft+T}\theta^{AT} + P^A{}_r(v^A{}_{ft}, \gamma^{Ar}{}_T) + P^A{}_l(v^A{}_{ft}, \gamma^{Al}{}_T) + D^{B}{}_{ft+T}\theta^{BT} + \ldots$$
$$\ldots + P^B{}_r(v^B{}_{ft}, \gamma^{Br}{}_T) + P^B{}_l(v^B{}_{ft}, \gamma^{Bl}{}_T) + \lambda_{fT} + \alpha_\tau + u_{ftT}$$

Where the polynomials are allowed to vary at different horizons "T" and a dummy for the distance to the date of the vote (λ_{fT}) is added.

The methodology of this table closely follows Cuñat, Giné and Guadalupe (2012) and more details can be found in its section IIB.

TABLE A4

Effect of Say-on-Pay Proposals on Firm Profitability

This table presents the effect of passing a Say-on-Pay proposal on firm profitability. The specification is equation (2) using a polynomial in the vote share of order 3 one on each side of the threshold. The dependent variables are obtained from Compustat are all defined as changes. Column 1 reports changes in Tobin's Q, which 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). Column 2, 3 and 4 report the change in Return on Equity (NI/(CEQ+TXDITC)), in Return on Assets (NI/AT) and in the Operating Return on Assets (CashFlow /AT), respectivewly. Column 5 and 6 report changes in the Capex ratio (Capex/AT) and in Overheads (XSGA/XOPR). All dependent variables are winsorized at the 5th and 95th percentile. All columns control for year fixed effects. Standard errors in parentheses are clustered by firm. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and *** respectively.

	(1) **Tobin Q Change**	(2) **ROE Change**	(3) **ROA Change**	(4) **OROA (CashFlow /AT) Change**	(5) **Capex/ Total Assets Change**	(6) **Overheads Change**
Effect from t-1 to t						
Pass	0.0517	0.0552	-0.0121	0.00891	-0.00656	-0.0121*
	(0.0876)	(0.0767)	(0.0226)	(0.0176)	(0.00669)	(0.00725)
Obs.	241	250	250	247	245	209
R-sq.	0.259	0.065	0.083	0.060	0.173	0.053
Effect from t to t+1						
Pass	0.134	0.107	0.0583***	0.0511***	-0.0118**	-0.0260***
	(0.0865)	(0.0768)	(0.0172)	(0.0157)	(0.00568)	(0.00946)
Obs.	184	192	192	188	189	158
R-sq.	0.303	0.136	0.214	0.195	0.095	0.078

TABLE A5

Passing Say on Pay Proposals and Changes in the Level of Compensation

This table presents the effect of passing a Say-on-Pay proposal on compensation measures. We estimate the specification in equation (2) using a polynomial in the vote share of order 3 on each side of the threshold. The dependent variables are obtained from Execucomp. Column 1 reports growth in Total Compensation (TDC1), column 2 the change in CEO Turnover and column 3 growth in Total Compensation within CEO. Column 4 reports growth in Salary and column 5 growth in Variable Compensation (Stock_awards_fv+Option_awards_fv+Bonus+ Noneq_Incent). Column 6 and 7 report growth in Option and Stock Portfolio, respectively. Column 8 reports growth in Stock and Option Portfolio Delta. All dependent variables are winsorized at the 5th and 95th percentile. All columns control for year fixed effects. Standard errors (in parentheses) are clustered by firm. Significance at the 10%, 5%, and 1% levels is indicated by *, **, and *** respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Compensation Growth	**Change in CEO (Turnover)**	**Total Compensation Growth**	**Salary Growth**	**Variable Compensation Growth**	**Option Portfolio Growth**	**Stock Portfolio Growth**	**Delta Growth Stock & Option Portfolio**
			Within CEO	*Within CEO*	*Within CEO*	*Within CEO*	*Within CEO*	*Within CEO*
Effect from t-1 to t								
Pass	-0.155	-0.0259	-0.0212	-0.00893	-0.110	-0.328	-0.256	-0.316
	(0.143)	(0.111)	(0.124)	(0.0171)	(0.129)	(0.452)	(0.276)	(0.214)
Obs	233	238	210	208	201	194	204	201
R-sq	0.058	0.043	0.106	0.068	0.100	0.392	0.108	0.354
Effect from t to t+1								
Pass	-0.212	-0.0362	-0.173	-0.0443***	-0.197	0.599**	0.531	0.349**
	(0.194)	(0.0986)	(0.197)	(0.0167)	(0.239)	(0.264)	(0.414)	(0.137)
Obs	179	179	159	157	153	143	153	154
R-sq	0.038	0.045	0.054	0.073	0.058	0.335	0.191	0.375

Figure A4
Vote Distribution and Expected Abnormal Returns.

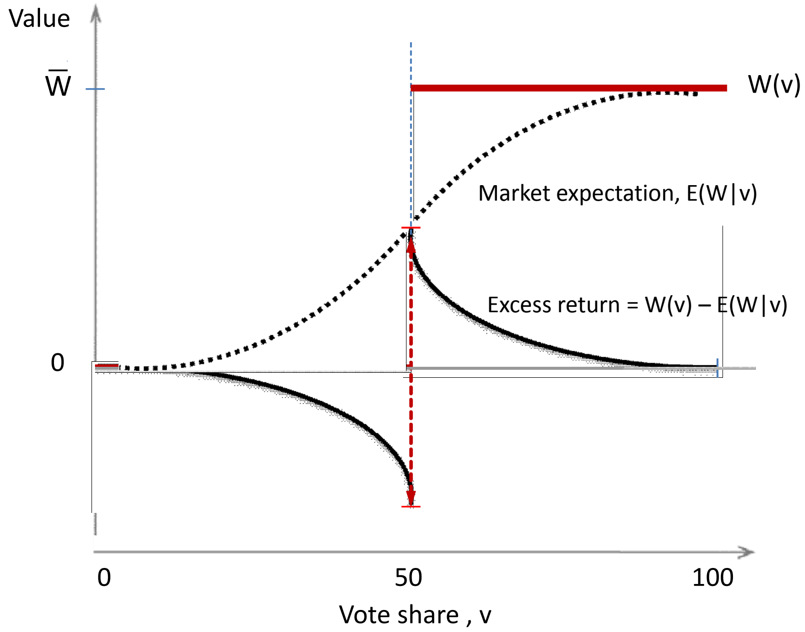

Figure A4 illustrates how one can recover the effect of passing a proposal on abnormal returns using a regression discontinuity. The figure uses 50% as the relevant majority threshold for all firms. The example assumes that the implementation of the proposal is binding; although extending the analysis to non-binding fuzzy discontinuity design is straightforward. Our identification strategy only requires that there is a discrete jump in the probability of implementation at the majority threshold (this is the "fuzzy" regression discontinuity setting as in Lee and Lemieux, 2010).

Suppose that the value of implementing a proposal is $\overline{W}$, that the majority threshold is 50% and that passing a proposal is binding (firms have to adopt it).[32] The step function W(v) represents the change in firm value as a function of the actual vote. Prior to the vote, the market has formed an expectation of the outcome of the vote and incorporated its expected impact on firm value into stock prices, E(w| v).

[32] Extending the analysis to non-binding proposals is straightforward. Our identification strategy only requires that there is a discrete jump in the probability of implementation at the majority threshold (this is the "fuzzy" regression discontinuity setting as in Lee and Lemieux, 2010).

This expectation is a smooth version of the previous step function. The abnormal return that we observe after the vote is the difference between the actual value of the proposal to the firm W(v) (which is either W or zero, depending on whether it passes) and its expected value before the vote E(W|v).

One can recover the value of the proposal by fitting two flexible functions of the vote to each side of the discontinuity and allowing for a discrete jump at the discontinuity. The combination of an event study with a regression discontinuity design solves the pervasive problem of assessing prior expectations in an event study. The value Z captures $(W(v)_r - E_r(W(v)|v)) - (W(v)_l - E_l(W(v)|v))$. Where $W(v)_r = \bar{W}$, $W(v)_l = 0$, $E_l(W|v)$ denotes the expected effect of the vote as the vote approaches 50 from the left and similarly $E_r(W|v)$ when the vote approaches 50 from the right. As long as prior expectations are the same to each side of the discontinuity (i.e. E(W(v)|v)) is continuous), their effect cancels out in the RD design.