October 12, 2018

Mr. Brent J. Fields,
Secretary Securities and Exchange Commission
100 F Street, NE Washington, DC 20549-1090

via SEC internet submission form

Re: File No. 4-725 -SEC Staff Roundtable on the Proxy Process

Dear Mr. Fields,

We are Nadya Malenko, Associate Professor of Finance at Boston College, Carroll School of Management, and Yao Shen, Assistant Professor of Finance at Baruch College, Zicklin School of Business.

We are aware that the SEC is seeking responses on the role of proxy advisors in the proxy voting process. Motivated by multiple discussions and controversy about the influence of proxy advisory firms, and ISS in particular, we have written a research article titled "*The role of proxy advisory firms: Evidence from a regression-discontinuity design*," published in the *Review of Financial Studies* in 2016.

In our paper, we examine the effect of ISS recommendations on shareholder voting outcomes. There has been considerable disagreement about the extent of proxy advisors' influence on shareholder votes. While the literature finds a strong positive *correlation* between proxy advisors' recommendations and voting outcomes, estimating the true *causal* influence of proxy advisors is difficult. This is because if a proxy advisor recommends voting against a proposal, and that proposal is subsequently defeated, it does not necessarily imply that the proxy advisor's negative recommendation was the reason for the defeat. Instead, it may be that the proposal is inherently detrimental to the firm, leading, simultaneously, to shareholders voting against it and to a negative recommendation from the proxy advisor.

This difficulty in distinguishing *correlation* from *causality* has been widely recognized both in the academic literature and among industry participants, and was discussed at the SEC roundtable on proxy advisors in December 2013. As a result, many observers believe that the influence of proxy advisors is significantly overstated.

In our paper, we provide causal estimates of the effect of ISS on voting outcomes for say-on-pay proposals over 2010-2011. Our empirical design uses a well-known econometric technique called *Regression Discontinuity*, by exploiting a cutoff rule that was used by ISS in its recommendations on say-on-pay proposals. The logic behind the Regression Discontinuity approach in our paper is the following:

When analyzing say-on-pay proposals over 2010-2011, ISS used a cutoff rule to perform an initial screen: if a firm's 1-year and 3-year Total Shareholder Return (TSR) were below certain cutoffs (industry median TSRs), ISS performed a deeper analysis of the firm's compensation practices and, as a result, was more likely to give a negative recommendation. In general, whether TSR is above or below the cutoff could be correlated with the quality of the firm's compensation practices: for example, poorly performing firms could be more likely to have poor compensation contracts. However, whether a firm is just above the cutoff (i.e., in the 51^{st} percentile of TSR) or just below the cutoff (i.e., in the 49^{th} percentile of TSR) is essentially random and is uncorrelated with proposal quality. This randomness introduces an exogenous shock to ISS recommendations and allows us to provide a causal estimate of ISS influence. We therefore compare firms that were just above the cutoff to those that were just below, leading to the following main findings:

- Relative to a positive recommendation, a **negative ISS recommendation leads to a 25 percentage point decrease in voting support for say-on-pay proposals** over 2010-2011. In other words, ISS moves about a quarter of the votes in our sample.

- The influence of ISS is stronger in firms in which institutional ownership is larger and less concentrated and in which there are more institutions that have high turnover or small positions. This evidence is consistent with the idea that smaller and more short-term shareholders have stronger incentives to rely on ISS instead of performing independent governance research.

- We discuss the generalizability of our causal estimates beyond our sample and discuss that, under certain assumptions, the 25% effect could be generalized to other firms and to subsequent years.

Overall, our findings indicate that ISS recommendations have a strong effect on voting outcomes, especially in firms whose shareholders do not have incentives to conduct independent governance research. While our paper does not evaluate whether this influence is good or bad, it emphasizes that, contrary to frequent claims, the influence of ISS does not seem overstated, suggesting that the consideration of various regulatory proposals is warranted.

We attach a copy of our paper, which contains the research underlying the above conclusions.

Sincerely,

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The Role of Proxy Advisory Firms: Evidence from a Regression-Discontinuity Design*

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Abstract

Proxy advisory firms have become important players in corporate governance, but the extent of their influence over shareholder votes is debated. We estimate the effect of Institutional Shareholder Services (ISS) recommendations on voting outcomes by exploiting exogenous variation in ISS recommendations generated by a cutoff rule in ISS voting guidelines. Using a regression discontinuity design, we find that from 2010 to 2011, a negative ISS recommendation on a say-on-pay proposal leads to a 25 percentage point reduction in say-on-pay voting support, suggesting a strong influence over shareholder votes. We also use our setting to examine the informational role of ISS recommendations. (*JEL* G34, D72, J33, M12)

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Shareholder voting plays a key role in corporate governance. It has become especially important because of the increase in institutional ownership, the rise in shareholder activism, the shift to majority voting for director elections, and the introduction of mandatory say-on-pay. A significant development in recent years has been the growth of proxy advisory firms. Proxy advisors counsel investors on how to vote their shares on major corporate decisions, such as mergers and acquisitions, director elections, executive compensation, and corporate governance policies. The largest proxy advisor, Institutional Shareholder Services (ISS), covers almost 40,000 meetings in 115 countries and has over 1,600 institutional clients.

Over time, regulators and market participants have become increasingly concerned with the influence proxy advisors allegedly have on investors' votes. According to the SEC commissioner Michael Piwowar, at the 2013 SEC roundtable on proxy advisors, "proxy advisory firms may exercise outsized influence on shareholder voting" and the "Dodd-Frank provisions, such as mandatory say-on-pay votes, make proxy advisory firms potentially even more influential." Proxy advisors' influence is potentially concerning because their recommendations are frequently criticized for inaccuracies, a one-size-fits-all approach to governance matters, and conflicts of interest stemming from the consulting services to corporations.

Motivated by these concerns, the SEC has held several discussions about the role of proxy advisors; these discussions culminated in the release of Staff Legal Bulletin No. 20 (SLB 20) in June 2014. The main goal of the bulletin has been to provide guidance on investment advisors' use of proxy advisors and on proxy advisors' responsibilities in dealing with conflicts of interest. However, many market participants, including regulators themselves, feel that the guidance provided by SLB 20 is insufficient and that more stringent regulation may be necessary.[1]

To understand whether increased regulation is warranted, we must ultimately understand the extent of proxy advisors' influence over shareholder votes. There is disagreement about whether the impact of their recommendations is as strong as is sometimes claimed. On the one hand, regulators' concerns about proxy advisors', and especially ISS's, outsized influence are consistent with the

[1] For example, the SEC commissioner Daniel Gallagher writes "While these reforms are much-needed, I am concerned that the guidance does not go far enough" (Gallagher 2014). See the Online Appendix for a more detailed discussion of the regulatory debate on proxy advisors.

strong positive correlation observed between ISS recommendations and voting outcomes. On the other hand, assessing the actual influence of ISS has been difficult because of the omitted variable problem: the same unobservable firm and management characteristics that lead ISS to give a negative recommendation can also lead shareholders to withdraw their support for the proposal, leading to an upward bias in the estimates of the ISS effect. This issue has been widely recognized in the academic literature and by many industry participants and has been discussed at the SEC roundtable on proxy advisors in December 2013. Prior literature concludes that ISS recommendations move at least some fraction of the votes, but whether this fraction is large or small remains unclear (e.g., Iliev and Lowry 2015; Ertimur, Ferri, and Oesch 2013; Larcker, McCall, and Ormazabal 2015).[2] As a result, many observers believe that the influence of proxy advisors is significantly overstated and hence believe that stringent regulation may do more harm than good (e.g., Edelman 2013). Even ISS itself has used this argument to alleviate regulators' concerns about its oversized influence and prevent further regulation (ISS 2012): "As many investors can be expected to share a general approach to assessing corporate governance practices, it is perhaps not surprising that correlations can be found between ISS recommendations and shareholder voting outcomes. However, this does not prove causality, nor are those correlations consistent. In our view, it is more logical to interpret broad correlation as indicating that ISS policies, analyses and recommendations are based on principles and approaches which are shared by many investors."

In this paper, we address this empirical challenge and quantify the causal effect of ISS by exploiting exogenous variation in ISS recommendations due to a cutoff rule employed by ISS in its 2010-2011 guidelines on say-on-pay proposals. Specifically, ISS used to conduct an initial screen of companies focusing on their one- and three-year total shareholder returns (TSRs) relative to certain cutoffs and only performed a deeper analysis of the company's executive compensation practices for companies below the cutoff. For example, the ISS 2012 white paper "Evaluating Pay for Performance Alignment" notes "In the last few years, the approach has utilized a quantitative methodology to

[2] For example, Iliev and Lowry (2015), Ertimur, Ferri, and Oesch (2013), and Larcker, McCall, and Ormazabal (2015) show that sensitivity to ISS recommendations is weaker for shareholders that are larger and have lower turnover, and Ertimur, Ferri, and Oesch (2013) use this differential sensitivity to put a lower bound on the causal effect of ISS.

identify underperforming companies – i.e., those with both 1- and 3-year total shareholder return (TSR) below the median of peers in their 4-digit Global Industry Classification Standard (GICS) group. Underperforming companies then received an in-depth qualitative review, focused primarily on factors such as the year-over-year change in the CEO's total pay, the 5-year trend in CEO pay versus company TSR, and the strength of performance-based pay elements." More specifically, as we discuss in Section 2.1, the cutoff for a given company is calculated as the median TSR of all firms that are both in the company's four-digit GICS industry group and in Russell 3000, where the TSRs are computed on the last day of the calendar quarter closest to the company's fiscal year-end. In the rest of the paper, we refer to this rule as the ISS "cutoff rule".

The ISS cutoff rule allows us to use a regression discontinuity (RD) design to estimate the causal effect of ISS recommendations on say-on-pay voting outcomes. Specifically, the rule implies that firms below the cutoff undergo more scrutiny to achieve a positive recommendation from ISS than firms above the cutoff, and hence the probability of a negative ISS recommendation should increase discontinuously just below the cutoff. Indeed, we show that relative to firms just above the cutoff, there is a 15% increase (from 10% to 25%) in the probability of a negative say-on-pay recommendation for firms just below the cutoff. This jump is large given that the average probability of a negative say-on-pay recommendation in our sample is 12.7%. At the same time, the somewhat arbitrary nature of the ISS cutoff suggests that being just above or below the cutoff is locally random, that is, firms around the cutoff are similar across all characteristics, except for, potentially, the ISS recommendation. Thus, any discontinuous decrease in voting support below the cutoff can be attributed to the discontinuity in ISS recommendations; this allows us to implement a fuzzy RD design to estimate the causal effect of ISS (Imbens and Lemieux 2008; Roberts and Whited 2012).

Our analysis shows a strong effect of ISS recommendations on say-on-pay voting outcomes: we find that relative to positive recommendations, negative ISS recommendations lead to a 25 percentage point decrease in voting support for say-on-pay proposals from 2010 to 2011. In other words, ISS moves about a quarter of the votes in our sample. This effect is economically significant: dissent above 20% is viewed as an indication of substantial dissatisfaction (e.g., Del Guercio, Seery, and Woidtke 2008; Ferri and Maber 2013) and leads companies to change their compensation practices

(Ferri and Maber 2013; Ertimur, Ferri, and Oesch 2013). Our main specification is a local linear regression estimated on a 5% bandwidth, and our estimates are robust to using multiple bandwidths and flexible polynomial functions and to controlling for various firm characteristics. We also show that the influence of ISS is stronger in firms in which institutional ownership is larger and less concentrated and in which there are more institutions that have high turnover or small positions, consistent with the hypothesis that such shareholders have stronger incentives to rely on ISS instead of performing independent governance research (e.g., Iliev and Lowry 2015).

The key assumption of our RD design is that whether a firm falls just above or below the cutoff is locally random. We perform several tests to verify this assumption and show that our results are not driven by differences in firm characteristics around the cutoff. First, we redo our analysis on several samples for which the ISS cutoff rule does not apply: say-on-pay voting in 2012 (the year in which ISS stopped using this rule), voting for the board as a whole, and voting for compensation committee members. In all these samples, voting support is continuous around the cutoff, suggesting that the discontinuity in votes in our main sample exists because of the corresponding discontinuity in ISS recommendations. Second, we show that the distribution of various elements of CEO compensation and other firm characteristics is smooth around the cutoff. Third, we alleviate the concern that firms manipulate their TSRs to move above the cutoff by performing the McCrary (2008) test and showing that the density of the forcing variable is smooth around the cutoff. Such manipulation is indeed unlikely, given that the cutoff depends on the TSRs of all firms in the industry and the TSRs are determined by stock price movements. Fourth, we consider several placebo cutoffs and show continuity in voting support around them.

We also use our results to discuss the informativeness of ISS recommendations relative to the information that shareholders possess independently. To study this question, we compare our estimates of ISS's influence to the estimates obtained via ordinary least squares (OLS) and find that the two are very close to each other. As we discuss in Section 5.1, this result suggests that if large long-term shareholders perform their own governance research and vote based on it (as prior literature suggests), then ISS recommendations are uncorrelated with these shareholders' information. For example, this could be the case if large shareholders perform firm-specific research, while ISS follows a one-size-fits-all approach to governance matters.

Finally, we discuss the generalizability of our results beyond our sample. The RD design does not allow us to estimate the causal effect of ISS for other types of proposals or for firms away from the cutoff and after 2011, so one should be cautious in extrapolating our estimates to the general effect of ISS recommendations. However, we examine the OLS estimates of the ISS effect on say-on-pay voting outcomes and find that they are very stable across different subsamples from 2010 to 2011 (ranging between 23% and 25%) and over time (ranging between 25% and 29%). Assuming that the omitted variable bias in OLS estimates remains small in these other samples, this suggests that the 25% effect could be generalized to other firms and to subsequent years.

Our paper contributes to the literature on shareholder activism and the role of institutional investors in firms' corporate governance.[3] In particular, it is related to the literature on shareholder voting. Prior research shows that shareholder voting has a significant impact on firms' policies and value, even when votes are nonbinding.[4] Cuñat, Gine, and Guadalupe (2012, 2015) find that relative to proposals that fail by a small margin, proposals that pass by a small margin yield an abnormal return between 1.3% and 2.4%, depending on the proposal type. In the context of say-on-pay voting, Ferri and Maber (2013) show that about 80% of U.K. firms with substantial voting dissent respond by removing controversial compensation practices, and Ertimur, Ferri, and Oesch (2013) find similar evidence for U.S. firms.[5] Given the significance of shareholder voting, it is important to understand which factors affect investors' voting decisions. Our paper shows that the recommendations of proxy advisory firms are a major factor affecting shareholder votes.

The literature on proxy advisors documents a significant positive association between ISS recommendations and shareholder support on various voting issues.[6] Many papers point out that the

[3]For example, Hartzell and Starks (2003), Aghion, Van Reenen, and Zingales (2013), Mullins (2014), Boone and White (2015), Appel, Gormley, and Keim (2016), and Crane, Michenaud, and Weston (2016). See Karpoff (2001), Gillan and Starks (2007), and Brav, Jiang, and Kim (2009) for reviews of the literature on shareholder activism.

[4]See Ferri (2012) for a survey of nonbinding voting and Levit and Malenko (2011) for a theoretical analysis.

[5]Relatedly, Schwartz-Ziv and Wermers (2015) show that low say-on-pay support is followed by a decrease in excess compensation and better selection of peer firms when ownership is relatively concentrated. See also Ertimur, Ferri, and Muslu (2011) and Cai and Walkling (2011).

[6]See, for example, Alexander et al. (2010), Bethel and Gillan (2002), Cai, Garner, and Walking (2009), Morgan et al. (2011), Ertimur, Ferri, and Oesch (2013, 2015), Aggarwal, Erel, and Starks (2015), Iliev and Lowry (2015), and Larcker, McCall, and Ormazabal (2015).

association between recommendations and vote outcomes might not be causal and might be explained by shareholders and ISS independently reaching the same conclusions and/or by ISS being influenced by institutional investors' opinions. To assess causality, Iliev and Lowry (2015), Ertimur, Ferri, and Oesch (2013), and Larcker, McCall, and Ormazabal (2015) show that sensitivity to ISS recommendations is stronger for shareholders that are smaller and have higher turnover, consistent with these shareholders having weaker incentives to perform independent research. Using this differential sensitivity to ISS, Ertimur, Ferri, and Oesch (2013) estimate that, under certain assumptions on blockholder versus nonblockholder voting, the lower bound on the causal effect of ISS is 5.7%, while the upper bound is 25%, their OLS estimate.[7] Thus, prior literature suggests that ISS moves at least some fraction of the votes, but it is unknown whether this effect is small (in the order of magnitude of 5%) or large (in the order of magnitude of 25%). Our main contribution to this literature is to estimate the magnitude of the causal effect of ISS. We show that ISS recommendations move 25% of say-on-pay votes; this is evidence of rather strong influence. In addition, this effect is similar to the estimates obtained via OLS, suggesting that at least based on our sample of 2010-2011 say-on-pay votes, the influence of ISS does not seem overstated. In a contemporaneous study, Bach and Metzger (2015) use the passing of shareholder proposals as an instrument for ISS recommendations on directors and estimate the ISS effect to be 25% as well. Motivated by the evidence on ISS's influence, Malenko and Malenko (2016) provide a theoretical analysis of the effect of proxy advisors on voting outcomes.

1 Methodology

In this section, we discuss how we use the regression discontinuity design to estimate the causal effect of ISS recommendations on say-on-pay voting outcomes.

According to the ISS cutoff rule, companies identified as "underperforming", that is, whose both

[7] The lower bound is calculated under the assumptions that all institutional blockholders do their own research and cast votes independently of ISS and that all shareholders (blockholders and nonblockholders) doing their own research reach, on average, the same conclusions. In a similar spirit, Choi, Fisch, and Kahan (2010) study the interaction terms between ISS recommendations and individual and institutional investor holdings. Assuming that voting of individual investors is a perfect proxy for how institutions would vote if ISS did not exist, they conclude that the causal effect of ISS is between 6% and 10%.

one- and three-year TSRs are below the respective median TSRs of their four-digit GICS groups, receive an in-depth qualitative review of their compensation practices from ISS. This rule suggests that ISS is likely to give a positive say-on-pay recommendation without conducting deep analysis if the firm is not identified as "underperforming", but will carefully scrutinize the firm's compensation practices before giving it a positive recommendation if the firm is "underperforming". As we show in Section 3.1, this leads to a discrete jump in the probability of a negative recommendation for "underperforming" firms. We therefore implement a fuzzy RD design by instrumenting a negative ISS recommendation with an indicator variable *BelowCutoff*, which equals one if the firm's one- and three-year TSRs both fall below their respective industry medians, and zero otherwise (Imbens and Lemieux 2008; Roberts and Whited 2012). Formally, for firm i in year t, $\mathit{BelowCutoff}_{it}$ is given by

$$\mathit{BelowCutoff}_{it} = \begin{cases} 1 & \mathit{if}\ \ MaxTSR_{it} < 0, \\ 0 & \mathit{otherwise}, \end{cases} \tag{1}$$

where $MaxTSR$ is the forcing variable, measured in percentage points and defined as

$$MaxTSR_{it} = \max(\mathit{TSR}_{it}^{(1)} - \mathit{MedianTSR}_{it}^{(1)}, \mathit{TSR}_{it}^{(3)} - \mathit{MedianTSR}_{it}^{(3)}), \tag{2}$$

$TSR_{it}^{(n)}$ is the n-year TSR of firm i computed in year t, and $MedianTSR_{it}^{(n)}$ is the median n-year TSR in year t computed across all Russell 3000 firms in the same four-digit GICS group as firm i.

The identification assumption is local continuity, which implies that firms around the cutoff are comparable, so that the relation between voting support and the variable $MaxTSR$ would be smooth around the cutoff in the absence of differential ISS recommendations. This assumption is plausible because the cutoff used by ISS is somewhat arbitrary. First, it is based on the TSRs of a specific group of firms (those both in Russell 3000 and in the firm's four-digit GICS group), and second, the TSRs are calculated on a specific date (the last day of the quarter closest to the firm's fiscal year-end). We further discuss and formally test this identification assumption in Sections 4.2-4.4.

We conduct the two-stage least-squares (2SLS) procedure by estimating the following two-equation system. The first (second) equation corresponds to the first (second) stage:

$$\begin{aligned} NegRec &= \gamma_0 + \gamma_1 BelowCutoff + f_1(MaxTSR) + BelowCutoff \cdot f_2(MaxTSR) + \gamma X + u, \\ Votes &= \alpha_0 + \alpha_1 \widehat{NegRec} + g_1(MaxTSR) + BelowCutoff \cdot g_2(MaxTSR) + \alpha X + \varepsilon, \end{aligned} \tag{3}$$

where *Votes* is the percentage of votes in favor of a say-on-pay proposal; *NegRec* is an indicator variable equal to one if the ISS recommendation is negative and zero if positive; $\widehat{NegRec}$ is the fitted value of *NegRec* from the first-stage regression; f_1, f_2, g_1, and g_2 are continuous functions of $MaxTSR$; and X is a vector of control variables. As is standard in the literature (e.g., Imbens and Lemieux 2008), we estimate a linear probability model for the first stage. The key coefficient of interest is α_1, which captures the local average treatment effect of a negative ISS recommendation on voting support. Standard errors are computed as the usual 2SLS standard errors.

Our main specification is a local linear regression, that is, f_i and g_i are linear functions, and the regressions are estimated on a small bandwidth around the cutoff, $-h < MaxTSR < h$. Section 4.1 shows that results are robust to including higher-order polynomials. Following the practical considerations in Imbens and Lemieux (2008), we focus on the rectangular kernel and use the same bandwidth for both stages. The trade-off in choosing the bandwidth is that a larger bandwidth increases precision by including more observations, but introduces an additional bias. In the main analysis, we use the bandwidth of 5%. Section 4.1 shows that our estimates are robust to bandwidth choice. We also apply the cross-validation procedure, which is commonly used to determine the optimal bandwidth (see the Online Appendix for details). The cross-validation procedure yields the optimal bandwidth between 4% and 5%, consistent with our baseline bandwidth of 5%.

In model 1 of Tables 2 and 3, we restrict the slope of the linear regression so that it is the same on the two sides of the cutoff, and in models 2-5, we allow for different slopes around the cutoff. Note that as long as the covariates are continuous around the cutoff (the assumption that we verify for various firm characteristics in Section 4.3), fuzzy RD design does not require the inclusion of control variables other than the forcing variable to produce consistent estimates (Imbens and Lemieux 2008). Nevertheless, we include several firm characteristics that may affect support for say-on-pay proposals, such as characteristics of the executive pay package, the firm's ownership structure, and other firm characteristics, as well as year and industry fixed effects, and show the robustness of our results.

2 Data and Variable Construction

The data on ISS recommendations and voting outcomes come from the ISS Voting Analytics database, which covers Russell 3000 firms. For each firm and each proposal on the agenda, the database provides the ISS recommendation, the percentage of votes for, votes against, and abstentions, and whether the proposal passed or failed. There is variation across firms in the way voting support is calculated. While the numerator is always the number of votes in favor of the proposal, the denominator, captured by the variable "base", is different across firms. Base can be the sum of the votes in favor and against (51.83% of the sample), the sum of the votes in favor and against plus the number of abstentions (47.77% of the sample), or the number of shares outstanding (0.40% of the sample). We use the appropriate denominator to calculate *Votes*, the percentage voting support for each company.

We obtain the data on TSRs and firm characteristics from Compustat, the data on institutional ownership from Thomson Reuters 13F, the data on executive compensation and insider ownership from GMI Ratings (formerly Corporate Library), and the list of Russell 3000 firms from Bloomberg. We first match the ISS sample to Compustat and CRSP by ticker and company name.[8] We then merge the sample with GMI Ratings by ticker and name, and with Thomson Reuters 13F by CUSIP, name, and ticker. The Appendix contains the definitions of the variables used in the paper.

For most of the analysis, except for the falsification tests in Section 4.4, we focus on say-on-pay proposals voted on in 2010 and 2011, the years in which ISS applied its cutoff rule. Even though the cutoff rule might have been used prior to 2010, we did not find a formal mention of this rule in the 2007-2009 ISS guidelines. We do not lose many observations by excluding prior years because say-on-pay proposals were relatively rare before 2011, at which point they were made mandatory by the Dodd-Frank Act.

[8] We conduct two rounds of matching to ensure that the match is correct. The first approach is to merge the data with Compustat to obtain GVKEY (and CUSIP) and then merge with the CRSP/COMPUSTAT Merged link table to get PERMNO (and CUSIP). The second approach is to first merge the data with the stock names file from CRSP to get PERMNO and then merge with the link table to obtain GVKEY. We cross-check the observations after the two rounds of matching.

2.1 Constructing the TSR cutoffs

The Online Appendix presents a detailed description of the ISS cutoff rule, and we summarize the relevant information here.

According to the ISS guidelines, "ISS utilizes S&P's Compustat database for TSR calculated values. The Total Return concepts are annualized rates of return reflecting price appreciation plus reinvestment of dividends (calculated monthly) and the compounding effect of dividends paid on reinvested dividends." We therefore also use Compustat's Total Return concept to calculate TSRs. In particular, we multiply the current month's adjusted close price by the current month's Total Return Factor provided by Compustat, divide the result by the product of the adjusted close price multiplied by the Total Return Factor from the prior period (one or three prior years for $TSR^{(1)}$ and $TSR^{(3)}$, respectively), and annualize the three-year return. The Total Return Factor is a multiplication factor that reflects monthly price appreciation and reinvestment of monthly dividends and cash equivalent distributions and the compounding effect of dividends paid on reinvested dividends.

The one-year (three-year) TSR cutoff for each firm is calculated as the median one-year (three-year) TSR of Russell 3000 firms in the same four-digit GICS industry. ISS guidelines specify the following dates to compute TSRs. TSRs are downloaded at the end of each calendar quarter, that is, on the last day of March, June, September, and December. For a given firm and year, the relevant download date is the last day of the calendar quarter closest to the fiscal year-end of the subject firm. For example, if a firm's fiscal year-end is March 31, TSRs for firms in the same industry group are calculated on March 31. Besides manually calculating the median industry TSRs, we obtain the list of median TSRs from the ISS Web site. For illustration, the Online Appendix presents a screenshot of the Web page with median TSRs for each industry group downloaded from 2014 to 2015. We obtain similar tables for most periods in our sample and find that they mostly match our manually calculated cutoffs. Because the medians from the ISS Web site more precisely capture the cutoffs used by ISS, we use these medians for all quarters for which they were available on the ISS Web site and use our manually calculated medians for the remaining quarters. For robustness, we repeated the analysis using the manually calculated medians for all quarters and obtained similar results.

ISS guidelines do not specify whether a given firm's TSR, which is compared to the industry median cutoff, is downloaded on the firm's fiscal year-end date or, similarly to the corresponding industry median, at the end of the calendar quarter closest to its fiscal year-end. The two approaches coincide for firms whose fiscal year-end falls on the end of the calendar quarter; this constitutes 90% of the sample. To follow the ISS methodology precisely, we restrict our sample to these 90% of firms. We also note that regardless of the company's TSRs, ISS always gives it a positive say-on-pay recommendation if the total dollar value of CEO compensation is sufficiently small, in particular, below the fifth percentile of firms in that year. This implies that the fuzzy RD setting is not directly applicable for this group of firms, so we restrict our sample to observations with the total value of CEO compensation above the fifth percentile in that year. Our results are very close if we keep these observations, and the only difference is that at the first stage, the discontinuity in the probability of a negative ISS recommendation is slightly smaller.

2.2 Descriptive statistics

Our final sample covers 1,932 firms and 2,020 say-on-pay proposals in 2010 and 2011: 106 in 2010 and 1,914 in 2011, when say-on-pay became mandatory for a large number of firms.[9] The number of observations below the ISS cutoff (that is, with $MaxTSR < 0$) is 613; this constitutes 30% of the sample. Panel A of Table 1 presents descriptive statistics of the sample firms. The average market capitalization is \$5.5 billion; the average institutional ownership is 72%; and the average value of executive compensation is \$4.8 million. Panel A also presents descriptive statistics for our main subsample of firms in a 5% bandwidth around the cutoff (we discuss the comparison between the two samples in Section 4.6). Panel B presents voting outcomes depending on the ISS recommendation. The probability that ISS gives a negative recommendation is 12.7% (256 observations out of 2,020). The average voting support is 93.2% if ISS gives a positive recommendation, and 68.9% if ISS gives a negative recommendation. While all 1,764 proposals with a positive ISS recommendation received more than 50% voting support and thus passed, 29 out of 256 proposals with a negative

[9] Say-on-pay proposals were not mandatory in 2010, but this does not affect our methodology: the factors that lead a say-on-pay proposal to be included in the agenda do not invalidate the RD design as long as they are continuous around the cutoff.

recommendation failed. These statistics are consistent with the strong positive association between ISS recommendations and voting outcomes documented in prior studies. Next, we use the RD approach to examine which part of this association is causal.

[TABLE 1 HERE]

3 Effect of ISS on Voting Outcomes

We first present the graphical analysis and then the results of the 2SLS estimation. Figure 1 plots the distribution of ISS recommendations and voting support for say-on-pay proposals on a 10% bandwidth around the cutoff. Visual inspection of the graphs reveals a discontinuity in both variables around the cutoff: there is about a 20 percentage point increase in the probability of a negative ISS recommendation and about a 5 percentage point decrease in say-on-pay voting support for firms just below the cutoff relative to firms just above the cutoff. Given the identification assumption, we attribute the discontinuity in votes to the causal effect of ISS recommendations. For comparison, we find no similar discontinuity in votes for samples in which the ISS cutoff rule is not used, suggesting that shareholders do not use the same cutoff for their voting decisions (see Section 4.4).

In a fuzzy RD design, the estimate of the causal effect of treatment is the ratio of the two discontinuities, that is, the difference in expected outcomes around the cutoff divided by the difference in the probability of treatment around the cutoff. Hence, our rough estimate of the causal effect of ISS based on the graphical analysis is $\frac{5\%}{20\%}$, or 25 percentage points. In the next sections, we formally estimate the effect of ISS by conducting the 2SLS analysis in a narrow bandwidth.

[FIGURE 1 HERE]

3.1 Discontinuity of ISS recommendations around the cutoff

We start by formally showing the discontinuity in the probability of a negative ISS recommendation. Table 2 reports the first-stage regression estimated on a 5% bandwidth. This sample contains 403 observations, with 175 of them corresponding to firms below the cutoff. Models 1 and 2 control for the forcing variable with and without the interaction term, and model 3 adds year and industry fixed effects. In models 4 and 5, we add additional control variables that have been used in the literature on say-on-pay and proxy advisors (e.g., Ertimur, Ferri, and Oesch 2013). Specifically, in model 4, we control for governance characteristics that are likely to affect ISS recommendations and say-on-pay support: several characteristics of the executive compensation package, as well as institutional and insider ownership.[10] In model 5, we control for other firm characteristics, such as firm size, return on assets (ROA), and market-to-book ratio. In all specifications, the coefficient on *BelowCutoff* is about 0.15, that is, the probability of a negative ISS recommendation for firms just below the cutoff is 15 percentage points higher than for firms just above the cutoff. In unreported results, we also estimate the probit model for the first-stage regression. The marginal effect of the coefficient on *BelowCutoff* is 0.13, which is consistent with the estimates from the linear probability model.

[TABLE 2 HERE]

The magnitude of the first-stage discontinuity is substantial. Indeed, negative ISS recommendations are relatively rare: according to Table 1, the sample average probability of a negative recommendation is 12.7%. Therefore, the 15% jump around the cutoff is economically significant. In Section 4.5, we examine the strength of this instrument in more detail.

3.2 Effect of ISS recommendations on voting support

By using the discontinuity in ISS recommendations as an instrument, we next analyze the effect of ISS on voting outcomes. Table 3 presents the second-stage estimates for several specifications corresponding to those in Table 2. Importantly, our estimates are not biased by any omitted variables as long as these variables are continuous around the cutoff.

In models 1 and 2, the coefficient on *NegRec* is about -25 and is significant at the 5% level, suggesting that relative to a positive recommendation, a negative ISS recommendation reduces say-on-pay voting support by 25 percentage points. As expected under the identification assumption,

[10] Since the say-on-pay vote is to approve executive compensation over the preceding fiscal year, we use compensation variables from the fiscal year preceding the fiscal year of the shareholder meeting.

the effect is quantitatively similar and remains significant once we include year and industry fixed effects and other control variables: the coefficient on *NegRec* in model 5 is -26.6 and is significant at the 5% level. In Section 4.1, we verify the robustness of this effect to the choice of the bandwidth and the inclusion of higher-order polynomial controls.

It is instructive to compare the RD coefficients on *NegRec* to the coefficients from the corresponding OLS regressions of *Votes* on *NegRec*. We present the OLS estimates, standard errors, and p-values of the Durbin-Wu-Hausman test in the last three rows of Table 3. Interestingly, the OLS estimates are very close to the RD estimates: they are about 25% across all specifications, which is similar to the estimate in Ertimur, Ferri, and Oesch (2013) for say-on-pay proposals during the same time period. The Durbin-Wu-Hausman test does not reject the hypothesis that the RD and OLS estimates are equal. Thus, our estimate of the causal effect of ISS is very close to the upper bound of 25% that was proposed by Ertimur, Ferri, and Oesch (2013) based on their evidence. We discuss the implications of this finding in Section 5.1.

[TABLE 3 HERE]

The 25% effect is economically important. Indeed, practitioners and prior academic studies consider voting dissent of above 20% to be an indication of strong shareholder dissatisfaction (e.g., Del Guercio, Seery, and Woidtke 2008; Ferri and Maber 2013). According to a 2011 investor survey, 72% of investor respondents believe that voting dissent above 30% warrants an explicit response from the board regarding improvements in pay practices, and 20% is the most commonly cited dissent level that should trigger such a response (see "2011-2012 Policy Survey Summary of Results" by ISS). Accordingly, say-on-pay dissent above 20%-30% prompts most firms to change their compensation policies. Ferri and Maber (2013) examine U.K. firms and show that about 80% of firms with more than 20% say-on-pay dissent remove controversial pay practices, such as generous severance contracts and problematic performance-based vesting conditions in equity grants. Similarly, Ertimur, Ferri, and Oesch (2013) find that more than 70% of U.S. firms with at least 30% voting dissent change their compensation policies following the vote. Schwartz-Ziv and Wermers (2015) find that companies with low say-on-pay support are likely to decrease excessive compensation and pick peer firms that more closely resemble their own company when ownership is concentrated.

Why do firms respond to low say-on-pay support? In the past, low support levels, and especially failed votes, have led to shareholder lawsuits, negative media attention, and damage to firms' reputation. In its guidelines, ISS notes that if a company fails to "adequately respond to the company's previous say-on-pay proposal that received the support of less than 70 percent of votes cast," ISS may recommend to vote against the members of the compensation committee and potentially the full board. Similarly, Glass Lewis places extra scrutiny on firms with less than 75% approval. These consequences, combined with the strong influence of ISS on voting outcomes that we document, suggest that proxy advisors play an important role in firms' governance practices.

3.2.1 Variation in the effect of ISS across firms

The 25% estimate captures the impact of ISS on aggregate shareholder support and does not distinguish between different shareholders. In this section, we examine whether ISS has a particularly strong influence on certain types of investors by studying how the effect of ISS varies with the firm's ownership structure. Of course, because ownership structure is determined endogenously, we should be cautious about the interpretation of the cross-sectional results in this section.

We start by examining whether ISS has a smaller effect on shareholders that are ex ante more likely to perform independent governance research. Iliev and Lowry (2015), Ertimur, Ferri, and Oesch (2013), and Larcker, McCall, and Ormazabal (2015) show that sensitivity to ISS recommendations is weaker for institutions that are larger, have a larger stake in the firm, and have lower turnover, consistent with these investors having particularly strong incentives to invest in independent research. To study similar questions in our data, we examine how the effect of ISS varies with several ownership structure characteristics. First, in models 1-3 of Table 4, we look at several variables that measure shareholder size or concentration: institutional ownership Herfindahl-Hirschman index, defined as the sum of squared share ownership over all institutional investors; ownership by institutional blockholders (that is, institutions with more than a 5% stake); and ownership by the top ten institutional shareholders. The larger each of these variables is, the more likely that many shareholders have incentives to perform independent research, so we expect the impact of ISS to be smaller. Second, we follow Larcker, McCall, and Ormazabal (2015), who use the Bushee and Noe (2000) classification of institutional investors into three types – "transient", "quasi-indexer", and "dedicated". Larcker, McCall, and Ormazabal (2015) hypothesize that "dedicated" institutions have stronger incentives to acquire their own information, because of their large ownership blocks and low turnover, compared with the two other institution types. They thus define an institution as "passive" (in its reliance on ISS) if it is either "transient" or a "quasi-indexer" and, consistent with their hypothesis, show that the effect of ISS is stronger when the ownership by "passive" institutions is higher. Therefore, we also calculate the percentage of shares held by "passive" institutions and study its effect in model 4 of Table 4.

For each ownership characteristic, we restrict the sample to observations in a bandwidth around the cutoff and calculate the median value of this ownership characteristic in the resulting sample. Next, we divide this sample into two subsamples, based on whether the ownership characteristic falls below or above the median, and repeat the RD analysis on each subsample. Because the sample size drops twice when we cut the sample into these subsamples, we focus on a 10% bandwidth to avoid losing power and to keep the size of each subsample similar to the sample size in our main tests. The results are presented in Table 4. The coefficient on *BelowCutoff* in the first stage is consistent across subsamples and is close to the 15% coefficient in Table 2. This is expected, given that the ISS cutoff rule does not vary with the firm's ownership structure. In contrast, the coefficient on *NegRec* in the second stage is quite different across subsamples and is consistent with the hypothesis that ISS has a weaker effect on shareholders who are more likely to do independent research. For example, the effect of ISS is 41% and 21% for the subsamples with low and high institutional ownership concentration, respectively. Similarly, the effect of ISS is 38% and 19% for firms with high and low ownership by "passive" institutions, respectively. These results are in line with the findings of Iliev and Lowry (2015), Ertimur, Ferri, and Oesch (2013), and Larcker, McCall, and Ormazabal (2015).

Finally, in model 5 of Table 4, we examine the difference between institutional investors and other types of shareholders, such as retail investors or insiders. We expect ISS to have a particularly strong influence on the votes of institutional investors, who are its main clients. Consistent with this hypothesis, the coefficient on *NegRec* is -24.1 and -32.3 for the low and high institutional ownership subsamples, respectively.

[TABLE 4 HERE]

4 Validity of the RD Design and Robustness

In this section, we perform additional tests to show the validity of our RD setting. Section 4.1 analyzes the robustness of the estimates, Sections 4.2–4.4 present tests of internal validity, Section 4.5 examines the strength of the instrument, and Section 4.6 discusses external validity.

4.1 Robustness

We start by showing the robustness of our results to alternative bandwidths and specifications. The first two rows of Table 5 present the estimate and standard error of the coefficient on *BelowCutoff* in the first-stage regression, and the third and fourth rows present the estimate and standard error of the coefficient on *NegRec* in the second-stage regression. In columns 1-8, we repeat the analysis of the third specification from Tables 2 and 3 on bandwidths ranging between 3% and 10% and show that the estimates for both the first and second stages are quantitatively similar across bandwidths (results for other specifications are similar and omitted for brevity). An alternative to estimating a local linear regression on a narrow bandwidth is to use a larger sample but include higher-order polynomials of the forcing variable (e.g., Roberts and Whited 2012). Hence, in columns 9-11, we estimate regressions with different degree polynomials of $MaxTSR$ on a 20% bandwidth. The table shows that using higher-order polynomials does not affect our estimates either.

[TABLE 5 HERE]

4.2 No manipulation of the forcing variable

Our approach relies on the assumption that whether a firm is just above or just below the cutoff is random. In particular, we assume that firms do not manipulate their TSRs in a way that pushes them just above the cutoff. This assumption is plausible: it is not that easy for a firm to manipulate its TSR on a specific date given that it depends on stock price movements. Moreover, the cutoff for a given firm is a function of TSRs of all other firms in the industry; this cutoff is difficult to predict.

To verify the assumption of no manipulation formally, we perform the procedure proposed by McCrary (2008); the procedure tests for a discontinuity in the density of the forcing variable. Figure 2 plots the estimated density of $MaxTSR$ in a 5% bandwidth and shows that the distribution is smooth. The absolute value of the McCrary test statistic is 0.84, which is not statistically significantly different from zero at any conventional level. Thus, we cannot reject the null hypothesis that the density of the forcing variable is smooth around the cutoff.

[FIGURE 2 HERE]

4.3 Continuity of covariates

To further test the assumption of random assignment to treatment, we examine the distribution of ex ante firm characteristics around the cutoff. Under the null hypothesis of random assignment, the distribution of characteristics unaffected by ISS recommendations should be continuous. We look both at general firm characteristics, such as size, market-to-book ratio, ROA, leverage, and institutional and insider ownership, and at various characteristics of the executive compensation package, such as its total value, its percentile in the industry, its growth over the previous year and over the previous three years, and several measures of its pay-performance sensitivity. By analyzing executive compensation characteristics, we can also examine whether firms below the cutoff are more likely to preemptively change their compensation practices if they realize that they face a higher probability of an in-depth ISS review (e.g., Larcker, McCall, and Ormazabal 2015).

We perform two sets of tests, which are summarized in Table 6. First, we perform the RD analysis using each firm characteristic as the outcome variable and regressing it on *BelowCutoff* and control variables. For each characteristic, *BelowCutoff* is not statistically significant, consistent with the distribution being smooth around the cutoff. Second, we compare the average value of each characteristic in two narrow intervals: $-5\% < MaxTSR < 0$ and $0 < MaxTSR < 5\%$. The p-values for the difference in means test confirm that the means of each firm characteristic on the two sides of the cutoff are not statistically significantly different. Table A.1 in the Online Appendix repeats both sets of tests for several additional characteristics and shows similar results.

Finally, in unreported results, we verify continuity in the recommendations of Glass Lewis for 2010-2011 say-on-pay proposals around the cutoff. Thus, our estimates should be attributed to the causal effect of ISS, rather than to the combined effect of both proxy advisory firms.

[TABLE 6 HERE]

4.4 Tests on alternative samples and placebo cutoffs

We further confirm the validity of our RD setting by repeating the analysis on several alternative samples. In particular, instead of looking at say-on-pay proposals in 2010-2011, which is our main sample, we study say-on-pay proposals in 2012 and director elections. (Other types of proposals are much less common and hence do not provide enough observations in a narrow bandwidth around the cutoff.) The rationale for these falsification tests is that, as discussed below, the ISS cutoff rule does not apply to these samples, that is, the probability of a negative recommendation is continuous around the cutoff. Hence, if our exclusion restriction is valid and being above or below the cutoff only affects 2010-2011 say-on-pay voting outcomes through ISS recommendations, then voting outcomes for these alternative samples should be continuous around the cutoff.

First, as we discuss in the Online Appendix, ISS significantly changed its say-on-pay guidelines in 2012 and, among other things, stopped using its 2010-2011 cutoff methodology. We formally confirm this in Table A.2 of the Online Appendix by repeating the first-stage estimation on the 2012 say-on-pay sample and showing that the coefficient in the regression of *NegRec* on *BelowCutoff* is insignificant. Accordingly, Figure 3A below shows that voting support for the 2012 say-on-pay sample is smooth around the cutoff. The results of the regression analysis verify the absence of discontinuity in votes at any conventional level of significance and are omitted for brevity.

Similarly, ISS does not apply its cutoff rule for director elections: the 2010-2012 guidelines that describe ISS policies on director elections do not mention the use of the cutoff rule for those recommendations. We verify this in Tables A.3-A.5 of the Online Appendix by looking at ISS recommendations for directors in general (both from 2010 to 2011 and in 2012), and for members of the compensation committee in particular, and showing continuity around the cutoff.[11] Accordingly, Figures 3B, 3C, and 3D show that voting support for the corresponding samples is also continuous around the cutoff. Moreover, when we examine the 2010-2011 director elections in Figures 3B and 3C, we restrict attention to those firms in each year that had a say-on-pay proposal in that year and hence are included in our main sample of say-on-pay votes. In this way, we ensure that our say-on-pay sample and the sample of 2010-2011 director elections feature the exact same investors voting in the exact same firms and at the same points in time, just for different proposals. Hence, continuity of voting outcomes in Figures 3B and 3C provides strong evidence that the only reason for the discontinuity in 2010-2011 say-on-pay voting outcomes is the discontinuity in ISS recommendations.

These findings suggest that investors do not independently react to the same cutoff as ISS when making their voting decisions. This is plausible, given that the cutoff used by ISS is somewhat arbitrary. First, all TSRs are calculated on the last day of the calendar quarter closest to the company's fiscal year-end, which is a reasonable date, but is not the only reasonable choice. More importantly, the cutoff is based on a rather specific set of firms, which is the intersection of two sets – the firm's four-digit GICS group and the Russell 3000 index. According to the investor survey by Bew and Fields (2012), investors use different peer groups from those used by proxy advisors. One reason for that is that ISS's peer group selection has been criticized for including firms engaged in a different business than the company and not including the company's publicly recognized competitors (see, e.g., the proxy statement amendment of Allegheny Technologies on April 29, 2011).

[FIGURE 3 HERE]

Finally, we repeat the analysis on our original sample of 2010-2011 say-on-pay proposals, but use several placebo cutoffs: $MaxTSR = c$ for $c = -3\%$, 3%, -5%, and 5%. The results, presented in the Online Appendix, show continuity in voting support around these placebo cutoffs.

[11] Negative say-on-pay recommendations likely do not translate into negative director recommendations because ISS seems to give the boards a "grace period" of one year to respond to the say-on-pay vote. For example, its 2012 guidelines explicitly state that after giving a negative say-on-pay recommendation, ISS may recommend against the company's board or compensation committee members one year later if the company does not make adequate changes to its compensation package.

4.5 Instrument strength

Next, we analyze the strength of the instrument *BelowCutoff* in the first-stage regression. The jump around the cutoff is quite large economically, but it is important to understand whether the instrument is also strong statistically. We start by analyzing the first-stage F-statistic. Because our main specification features one endogenous regressor and one instrument, we use the Staiger and Stock (1997) rule of thumb and compare the F-statistic to 10. As the last row of Table 2 demonstrates, the F-statistic for the 5% bandwidth is only around 4.5, suggesting that the null hypothesis that the instrument is weak is not rejected. Given that the magnitude of the first-stage jump is rather large, this small F-statistic could be due to the small sample size for the 5% bandwidth. We therefore also examine the F-statistic for larger bandwidths, where the magnitude of the first-stage jump is very similar, but the sample is slightly larger. Table 7 shows that the F-statistic for all bandwidths starting with 7% exceeds 10, and thus the test rejects the presence of weak instruments. Recall that according to Section 4.1, both the estimates of the first-stage jump and, importantly, the 2SLS estimates of the ISS effect are quite similar between the 5%-6% bandwidths and the 7%-10% bandwidths. Thus, while the low value of the F-statistic for smaller bandwidths is a potential concern, the fact that our results for these bandwidths are similar to the results for larger bandwidths, where the weak instrument problem is less likely, is reassuring.

We also follow Stock, Wright, and Yogo (2002) and perform the Anderson and Rubin (1949) test, which is robust to the presence of weak instruments.[12] Table 7 reports the p-values of the Anderson-Rubin statistic for various bandwidths; the 5% and 6% bandwidths are particularly important given the low values of the F-statistic. The results show that even according to fully robust inference, the second-stage coefficient is significant at the 5% level for the 6% bandwidth and at the 10% level for the 5% bandwidth, which provides further support for our estimates. Finally, Table 7 also presents the reduced-form estimates and shows that the magnitude of the jump in voting support around the

[12]The estimate of the ISS effect from the second stage of the 2SLS estimation remains the same, but the Anderson-Rubin test allows us to correctly analyze the significance of this second-stage estimate even when instruments are potentially weak. In particular, while the standard inference used in Table 3 is based on the asymptotic normal approximation of the t-statistic, which is not valid if instruments are weak, the inference based on the Anderson-Rubin statistic is valid even under weak instruments.

cutoff is similar across bandwidths and is statistically significant.

[TABLE 7 HERE]

4.6 External validity

In this section, we discuss whether we can extrapolate our findings to the general effect of ISS recommendations. While the RD design has strong internal validity, its external validity is usually limited because the estimation is based on a narrow bandwidth around the cutoff. Unfortunately, our empirical design does not allow us to estimate the causal effect of ISS for firms away from the cutoff. We can, however, check whether the OLS estimate of the ISS effect is stable across subsamples. Imbens and Lemieux (2008) point out that if the RD and OLS estimates are close, and if the OLS estimate is relatively stable across subsamples, one would be more confident in both estimates. This is what we observe in the data. First, as shown in Table 3, the RD estimate of the ISS effect is very close to its OLS estimate. Second, Table A.6 in the Online Appendix shows that the OLS estimate is, in turn, very stable: it varies between 23% and 25% across various subsamples. As long as the OLS estimate remains close to the causal effect in these other subsamples, this suggests that our results are generalizable to other firms.

Another way to examine the generalizability of the results is to compare firms around the cutoff to those further from the cutoff. By construction, the cutoff for a given firm is calculated as the median TSR in the firm's industry, and hence each firm close to the cutoff has similar stock performance to a median industry firm. Thus, the sample of firms in a narrow bandwidth around the cutoff is likely representative of the whole sample of firms. This conjecture is consistent with panel A of Table 1, which presents the distribution of ex ante characteristics for firms in the full sample and in the 5% bandwidth: firms around the cutoff are similar to the full sample across a number of characteristics, including operating performance, leverage, institutional ownership, ownership concentration, and the proportion of stock-based compensation. There are some systematic differences as well: firms around the cutoff are on average larger (and, accordingly, have higher executive compensation) and have lower market-to-book ratios. To understand how the differences in these characteristics could affect the results, we perform the cross-sectional RD analysis of Section 3.2.1 for market-to-book and firm size. Table A.6 of the Online Appendix shows that the effect of ISS is slightly stronger for firms that are larger and have higher market-to-book ratios, but the differences are not statistically significant. Importantly, the ISS effect is at least 24.6% in each of these subsamples, suggesting that the 25% estimate is unlikely to significantly underestimate the influence of ISS for an average firm. Nevertheless, these differences need to be taken into account when extrapolating our estimates to a broader sample of firms.

It is also useful to understand whether the results can be generalized to other time periods. The majority of our sample is in 2011, the first year when say-on-pay votes became mandatory. Hence, shareholders' sensitivity to ISS could potentially change in subsequent years, when say-on-pay became a more routine issue. While we cannot repeat the RD analysis after 2011 because ISS stopped using its cutoff methodology, we can, again, analyze the OLS estimates of the ISS effect after 2011. In addition, we can examine other characteristics of say-on-pay votes to see whether there were important structural changes after 2011. Table A.6 of the Online Appendix shows that the distributions of say-on-pay voting support and ISS recommendations are similar between 2011 and 2012, and that the OLS estimates of the ISS effect for 2012 are very similar to those in Table 3 for 2010-2011. In unreported results, we also examine 2013 and 2014 and find that the OLS estimate of the ISS effect is quite persistent: it is 28.7% in 2013 and 27.7% in 2014. Assuming that the omitted variable bias in OLS estimates remains small in subsequent years, this suggests that the effect of ISS from 2012 to 2014 is close to the 25% effect estimated on the 2011 sample.

Finally, it is important to note that our results cannot be easily generalized to issues other than say-on-pay. Even those shareholders who follow ISS on executive compensation issues, could be more active and perform independent research for other types of proposals. Thus, the 25% of say-on-pay votes moved by ISS are not necessarily coming from passive voters, who blindly follow ISS in all cases.

5 Additional Analyses

In this section, we perform additional analyses to study the informational role of ISS and to understand its effect on ex post outcomes, such as price reaction to the vote and compensation practices.

5.1 Implications for the informational role of ISS

We start by examining the informativeness of ISS recommendations for the investors. The results so far show that ISS has a strong influence on shareholders' voting decisions, but this influence does not necessarily imply that ISS recommendations are informative. Indeed, even if ISS recommendations are not very informative, shareholders may have incentives to follow them if they are concerned about potential litigation or would like to coordinate their votes with other shareholders. For example, the 2003 SEC rule, which requires mutual funds to vote in their clients' best interests, explicitly states that an institution "could demonstrate that the vote was not a product of a conflict of interest if it voted client securities in accordance with a pre-determined policy, based upon the recommendations of an independent third party." Therefore, following ISS could allow shareholders to defend against potential criticism or litigation for their voting practices. In addition, Matvos and Ostrovsky (2010) show that mutual funds have a preference to vote similarly to other shareholders. If this preference is strong, ISS recommendations could serve as a coordination device, which would further encourage shareholders to follow them.

These arguments imply that estimating the effect of ISS on voting outcomes is not sufficient to understand its informational role. Recall, however, that our estimates of the ISS effect are very close to those obtained from the OLS analysis (Table 3). This finding turns out to be useful to derive implications for the informational role of ISS. To see this, consider the following stylized model.

Consider a shareholder deciding how to vote on a proposal and suppose that the shareholder and ISS each get a signal about the effect of the proposal: $Signal_{SH}$ and $Signal_{ISS}$, respectively, where each of the signals could be potentially uninformative. The signal of ISS affects whether or not it gives a negative recommendation on the proposal: *NegRec.* The shareholder makes his voting decision based on the combination of his signal and the ISS recommendation. For simplicity, suppose that the shareholder's voting decision is given by a linear model

$$Vote = \beta_0 + \beta_1 NegRec + \beta_2 Signal_{SH} + \varepsilon, \tag{4}$$

where β_1 captures the causal effect of ISS on the shareholder's vote. Because the shareholder's signal is unobserved by the econometrician, it is omitted from the OLS regression $Vote = \beta_{0,OLS} + \beta_{1,OLS} NegRec + u$. The omitted variable bias formula (e.g., Roberts and Whited 2012) implies that the OLS estimate converges to $\beta_1 + Bias_{OLS}$, where the omitted variable bias is given by

$$Bias_{OLS} = \beta_2 \times Corr(NegRec, Signal_{SH}) \times \frac{Std.Dev.(Signal_{SH})}{Std.Dev.(NegRec)}. \tag{5}$$

The similarity between the RD and OLS estimates in our sample implies that the omitted variable bias appears to be small. To simplify the subsequent discussion, suppose that it is exactly zero, $Bias_{OLS} = 0$. Expression (5) implies that this is consistent with two possibilities. The first is that the shareholder's signal $Signal_{SH}$ is uninformative about the value of the proposal. In this case, $Signal_{SH}$ is uncorrelated with the ISS recommendation and hence $Bias_{OLS}$ is indeed zero.[13] The second possibility is that the shareholder has an informative signal about the proposal and at least partly votes based on it. This implies $\beta_2 > 0$ and $Std.Dev.(Signal_{SH}) > 0$ (if the standard deviation is zero, the signal does not vary with the state and hence is uninformative). In this case, $Bias_{OLS}$ can only be zero if $Corr(NegRec, Signal_{SH}) = 0$, that is, if the ISS recommendation is uncorrelated with the shareholder's signal. This implies that either the ISS recommendation is uninformative, or that ISS and the shareholder acquire information about different aspects of the proposal.

We conclude that the small omitted variable bias in OLS estimates is consistent with two alternatives. One is that few shareholders vote based on their independent research about the proposal value. The other is that many shareholders vote based on their independent research, but ISS recommendations are only weakly correlated with their research. Our evidence alone does not allow us to distinguish between these two alternatives. Note, however, that Iliev and Lowry (2015), Ertimur,

[13] This alternative also includes the case in which the shareholder is biased and votes based on his preferences rather than to maximize firm value, for example, if he always votes with management.

Ferri, and Oesch (2013), and Larcker, McCall, and Ormazabal (2015) show that the association between ISS recommendations and shareholder votes is weaker for shareholders that are larger, have a larger stake, and lower turnover (Section 3.2.1 shows similar patterns in our data as well). These papers conclude that their evidence is consistent with the hypothesis that larger and more long-term shareholders perform independent research and vote based on their private information. If, indeed, many large shareholders have informative private signals and vote based on them, then the second alternative above is more likely. In other words, either ISS and shareholders acquire information about unrelated issues, or ISS recommendations must be relatively uninformative. For example, proxy advisors are frequently criticized for following a one-size-fits-all approach to corporate governance, without taking into account the specifics of the company.[14] If votes of large shareholders are company-specific, while the recommendations of ISS are not, then the correlation between the two is likely to be small. This interpretation is consistent with Iliev and Lowry (2015), who conclude that ISS's one-size-fits-all approach contributes to the observed differences between their recommendations and the votes of actively voting funds.

Note also that our conclusions from the OLS-RD comparison are confirmed by an additional test, which focuses on firms with a positive ISS recommendation. For brevity, we present the details of this test in the Online Appendix and only briefly describe our findings here. According to the ISS cutoff rule, firms below the cutoff receive "an in-depth qualitative review" and only get a positive recommendation if the review shows that their compensation practices are appropriate. In contrast, firms above the cutoff are likely to get a positive recommendation without going through such a review. Consider two firms that both receive a positive recommendation, but one falls just below and the other just above the cutoff. If the in-depth review by ISS is effective at screening firms based on the quality of their compensation packages and if many shareholders do independent research, we would expect to see greater say-on-pay voting support for the firm below the cutoff, where the positive recommendation is more informative. Focusing on the subsample of firms with a positive

[14] In addition, proxy advisors are frequently criticized for basing their recommendations on materially false or inaccurate information. See, for example, Gallagher (2014) and the comment letter to the SEC from the Shareholder Communications Coalition on December 4, 2013. According to PwC's 2014 Annual Corporate Directors Survey, more than 80% of directors believe that proxy advisory firms use a one-size-fits-all approach to governance and that their policies do not align with company needs or investors' best interests.

ISS recommendation, we do not find differences between voting support for firms just above and just below the cutoff. This suggests that either the ISS in-depth review is not very informative or that most shareholders do not perform independent research and simply follow ISS recommendations, that is, the two alternatives that also follow from the comparison of OLS and RD estimates.

Two important caveats are in order here. First, even if ISS recommendations are not correlated with large shareholders' information, it may still be optimal for small uninformed shareholders to follow them: this may be more efficient than to always support management or to vote randomly. Second, our results are based on the sample of say-on-pay proposals in 2010-2011. It could be the case that ISS recommendations are more correlated with large shareholders' information for other types of proposals or for say-on-pay proposals in later years, when this issue became more routine.[15]

5.2 Ex post outcomes

Given that falling below the cutoff increases a firm's likelihood of receiving a negative ISS recommendation and low say-on-pay support, it is interesting to compare firms around the cutoff in terms of the ex-post outcomes. For example, is the market reaction to voting outcomes different for firms around the cutoff? Or are firms below the cutoff more likely to change their compensation practices after the vote? We examine these questions in Tables A.7 and A.8 of the Online Appendix. In particular, the outcome variables in Table A.7 are the cumulative abnormal returns calculated over one-day and three-day windows around the voting date. The outcome variables in Table A.8 are the level of executive compensation and the percentage of stock-based compensation in the two years following the vote (results for other aggregate measures, such as the proportion of compensation represented by incentive pay, are similar and omitted for brevity). The key explanatory variables are whether the firm is below the cutoff, the percentage of say-on-pay voting support, and the interaction between the two. Depending on the specification, we also control for the ISS recommendation and characteristics of the compensation package being voted on.

Across all specifications, the market reaction around the voting date is similar for firms above

[15]For example, in the context of proxy contests, Alexander et al. (2010) find evidence that ISS recommendations are informative about the ability of dissidents to add value.

and below the cutoff. There are two potential explanations for this similarity. First, the abnormal return around the shareholder meeting date reflects the market reaction to multiple pieces of news, which include, besides the say-on-pay vote, director elections and votes for various shareholder and management proposals. Second, because ISS gives its recommendation a few weeks before the shareholder meeting, and because some shareholders announce in advance how they plan to vote, there is often substantial anticipation of the voting outcome by the market.

Likewise, we do not see a significant coefficient on either *BelowCutoff* or *Votes·BelowCutoff* for any of the compensation characteristics in subsequent years. On the one hand, this could mean that firms do not react to negative ISS recommendations or low say-on-pay voting support. However, this is less likely given the results of Ertimur, Ferri, and Oesch (2013): the authors examine the 2012 proxy statements of firms with a negative ISS recommendation in 2011 and find that 55% of these firms, as well as more than 70% of firms with at least 30% voting dissent, changed their compensation policies after the vote. The more likely explanation is that firms tend to change more specific provisions of their compensation packages, which are not captured by our aggregate compensation measures. For example, according to Ertimur, Ferri, and Oesch (2013), the most frequent changes made in response to low say-on-pay support are introducing performance-based vesting conditions in equity grants, toughening of performance goals in incentive plans, reducing perks and tax gross-ups on perks, and modifying change-in-control severance agreements.[16]

6 Conclusion

Proxy advisory firms and ISS, in particular, have emerged as prominent players in the proxy voting process, but their role is highly controversial. Many market participants are concerned about the outsized influence ISS recommendations allegedly have on shareholder voting outcomes and call for increased regulation of proxy advisors. Others argue that stringent regulation can do more harm than good because the influence of proxy advisors is overstated: the strong correlation between ISS

[16]The absence of significant effects on aggregate compensation measures is consistent with Cuñat, Gine, and Guadalupe (2015), who compare firms in which proposals to adopt a say-on-pay policy barely passed versus barely failed. While they find a significant difference in stock price reactions to the vote, they do not find systematic differences in the level or structure of executive pay following the vote.

recommendations and shareholder votes could be due to ISS and shareholders relying on the same information and independently reaching the same conclusions.

In this paper, we analyze the effect of ISS on voting outcomes by using exogenous variation in ISS recommendations due to a cutoff rule in the ISS voting guidelines. Specifically, when giving recommendations on say-on-pay proposals, ISS used to conduct an initial screen of firms based on their one- and three-year TSRs and performed a deeper analysis of a firm's compensation policies if both TSRs fell below certain industry-related cutoffs. This rule leads to a discontinuous increase in the probability of a negative ISS recommendation for firms just below the cutoff. We therefore use a fuzzy regression discontinuity design to estimate the causal effect of ISS recommendations on 2010-2011 say-on-pay voting outcomes. We find that ISS has a strong effect on shareholder votes: relative to positive recommendations, negative ISS recommendations reduce the percentage of votes in favor of say-on-pay proposals by about 25 percentage points. The influence of ISS is particularly strong in firms with large institutional ownership, firms where institutional ownership is more dispersed, and where a larger fraction of shares is held by institutions with small stakes or high turnover. Our findings contribute to the ongoing debate on the role and economic impact of proxy advisory firms.

Appendix

Table A1

Variable definitions

Variable	Definition	Source
$TSR^{(1)}$ (%)	One-year TSR, defined as the one year percentage change in the adjusted close price multiplied by the total return factor: $TSR^{(1)}$=100·[(PRCCM$_t$·TRFM$_t$/AJEXM$_t$) / (PRCCM$_{t-1}$·TRFM$_{t-1}$/AJEXM$_{t-1}$) - 1].	CSRP/Compustat Merged
$TSR^{(3)}$ (%)	Three-year TSR, defined as the annualized three year percentage change in the adjusted close price multiplied by the total return factor: $TSR^{(3)}$=100·[[(PRCCM$_t$·TRFM$_t$/AJEXM$_t$) / (PRCCM$_{t-3}$·TRFM$_{t-3}$/AJEXM$_{t-3}$)]$^{1/3}$ - 1].	CSRP/Compustat Merged
MaxTSR (%)	Defined as $max(TSR_{it}^{(1)} - MedianTSR_{it}^{(1)}, TSR_{it}^{(3)} - MedianTSR_{it}^{(3)})$, where $TSR_{it}^{(n)}$ is the n-year TSR of firm i in year t, and $MedianTSR_{it}^{(n)}$ is the median n-year TSR in year t computed across all Russell 3000 firms in the same four-digit GICS group as firm i.	CSRP/Compustat Merged
BelowCutoff	Indicator variable that takes the value of one if MaxTSR is negative, and zero otherwise.	CSRP/Compustat Merged
Votes (%)	The percentage of votes in favor of a say-on-pay proposal.	ISS Voting Analytics
NegRec	The indicator variable that takes the value of one if ISS gives a negative ("Against") recommendation, and zero otherwise.	ISS Voting Analytics

Variable	Definition	Source
Market Value of Equity (in $ billion)	Market value of equity, which is calculated by multiplying the company's stock price by its number of outstanding shares as of its fiscal-year-end.	Compustat
M/B	The ratio of the market value of assets (equity market capitalization plus the book value of other liabilities) to the book value of assets.	Compustat
ROA	Net income divided by total assets.	Compustat
Leverage	(Long-term debt + Debt in current liabilities)/Total assets.	Compustat
CEO Total Compensation (in $ million)	The total compensation of the CEO (variable CEOTotSumComp from GMI Ratings) as reported in the company's proxy statement. It equals the aggregate total dollar value of each form of compensation quantified in the summary compensation table, including base salary, bonus, stock awards, option awards, non-equity incentive plan, change in pension value and nonqualified deferred compensation, and all other compensation.	GMI Ratings
Proportion of Stock-Based Compensation	The sum of stock and option awards divided by CEO total compensation: (CEOOptionAwards + CEOStockAwards)/CEOTotSumComp from GMI Ratings.	GMI Ratings
Growth in CEO Total Compensation	Defined as (CEO Total Compensation in year t - CEO Total Compensation in year t-1)/CEO Total Compensation in year t-1.	GMI Ratings
Institutional Ownership	Total institutional ownership in fraction of shares outstanding (Instown_perc from Thomson Reuters 13F).	Thomson Reuters 13F
Institutional Ownership HHI	Institutional ownership Herfindahl-Hirschman index, i.e., the sum of squared share ownership over all institutional investors (Instown_HHI from Thomson Reuters 13F).	Thomson Reuters 13F
Insider Ownership	The estimated fraction of shares held by top management and directors, as reported in the firm's most recent proxy statement (InsidersPctg from GMI Ratings).	GMI Ratings
Industry dummies	Industry fixed effects are based on four-digit GICS classification to make the definition of the industry consistent with that used by ISS.	Compustat

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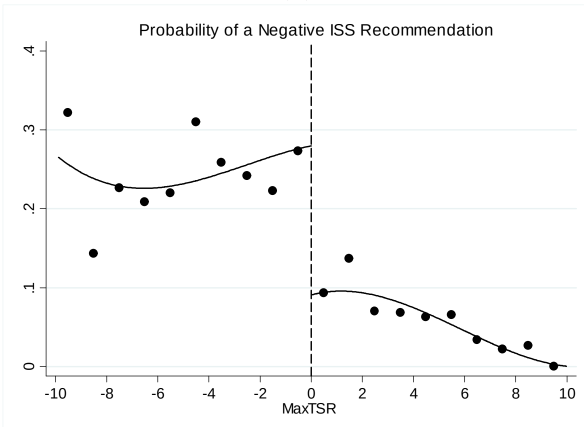
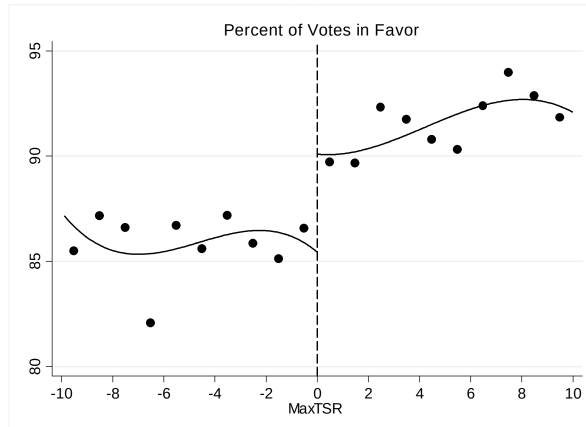
Figures and tables

Figure 1: Probability of a negative ISS recommendation and voting support

(a)

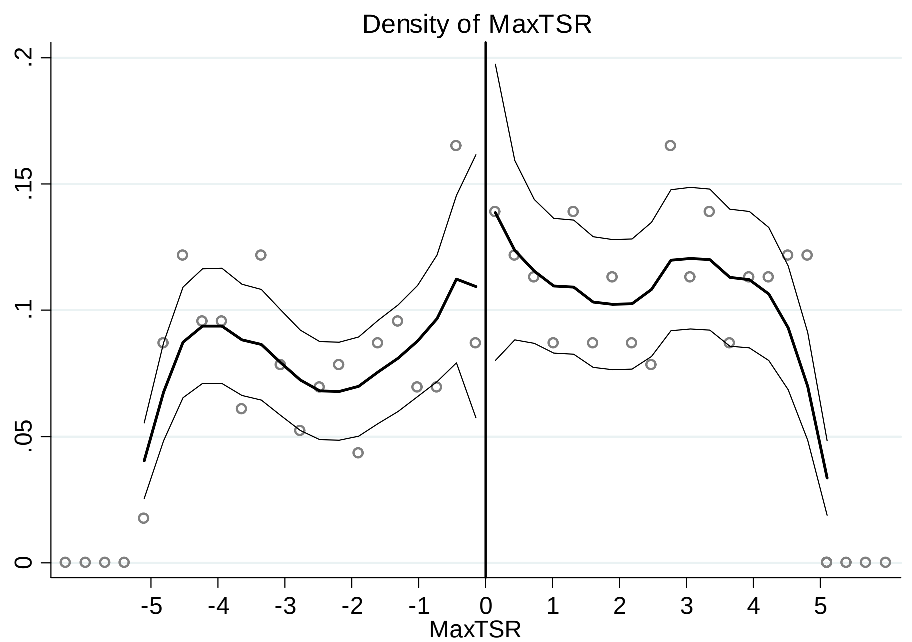


(b)



The figure plots the distribution of negative ISS recommendations and say-on-pay voting support around the cutoff. The x-axis presents the forcing variable $MaxTSR$, measured in percentage points, in a 10% bandwidth around the cutoff. The y-axis of Figure 1a corresponds to the probability of a negative ISS recommendation, measured in absolute values (from 0 to 1). The y-axis of Figure 1b corresponds to *Votes*, the percentage of votes in favor of the say-on-pay proposal, measured in percentage points. Each dot in Figure 1a (1b) represents the average probability of a negative ISS recommendation (percentage of votes in favor of the say-on-pay proposal) in bins of 1%. The solid lines represent the fitted values of a third-degree polynomial of $MaxTSR$ estimated on the interval $-10\% < MaxTSR < 10\%$.

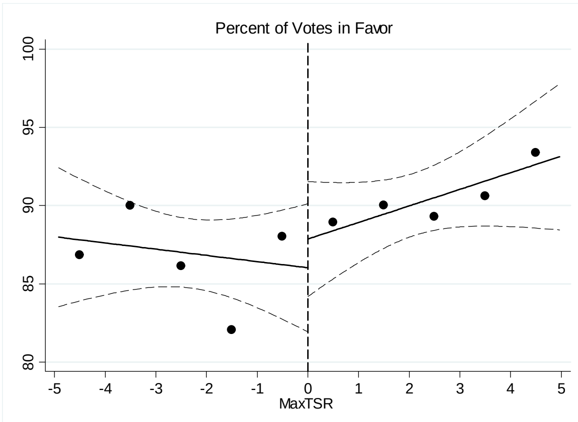
Figure 2: Density of the forcing variable



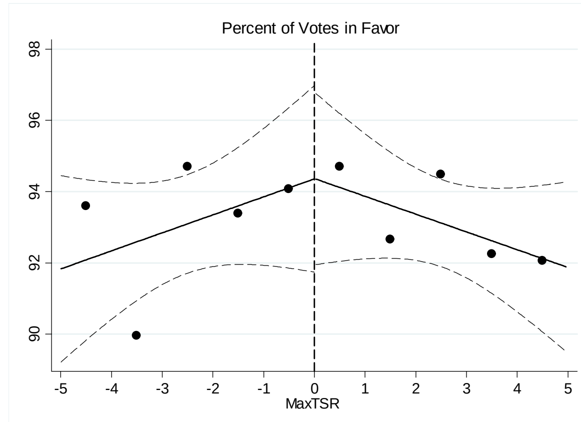
The figure confirms that firms do not manipulate their TSRs to push themselves above the cutoff by showing that the density of the forcing variable is continuous. The x-axis presents the forcing variable $MaxTSR$, measured in percentage points, in a 5% bandwidth around the cutoff. The y-axis corresponds to the density of $MaxTSR$, measured in absolute values. The figure shows the histogram, estimated density, and 95% confidence intervals of $MaxTSR$ in a 5% bandwidth. The absolute value of the McCrary (2008) test statistic is 0.84, which is not statistically significantly different from zero at any conventional level. Both the figure and the McCrary test statistic were generated using the code provided by J. McCrary on his website: http://eml.berkeley.edu/~jmccrary/DCdensity/.

Figure 3: Falsification tests on alternative samples

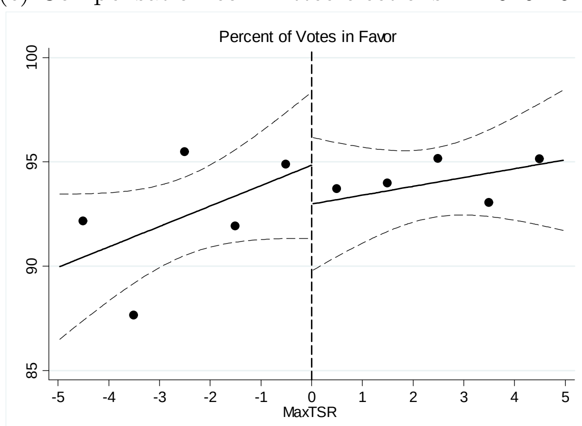
(a) Say-on-pay proposals in 2012



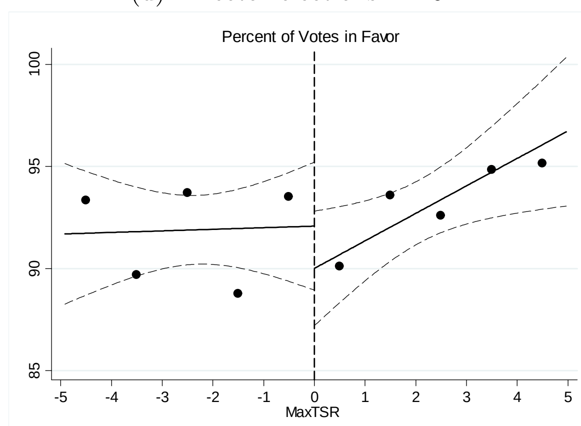
(b) Director elections in 2010-2011



(c) Compensation committee elections in 2010-2011



(d) Director elections in 2012



The figure shows that voting support is continuous for samples where the cutoff rule does not apply. Figure 3a considers say-on-pay proposals in 2012. Figure 3b considers director elections in 2010-2011, where to match our main sample, we restrict attention to those firms in each year that had a say-on-pay vote in that year. Figure 3c restricts the sample in Figure 3b to the elections of compensation committee members. Figure 3d considers director elections in 2012. Because each firm has many directors but only one say-on-pay proposal, we aggregate director election observations by firm-year-recommendation to make the sample consistent with our main sample: for each firm-year, we calculate the average voting support for all directors who received a positive (negative) recommendation. Samples a-d have 289, 470, 263, and 332 observations, respectively.

Table 1: Descriptive statistics

Panel A

	Full sample					5% bandwidth					Diff. in means
	Mean	25th	50th	75th	SD	Mean	25th	50th	75th	SD	p-val
Market Value of Equity	5.53	0.44	1.15	3.50	17.78	7.97	0.64	1.64	4.37	26.47	0.08
ROA	0.02	0.00	0.03	0.07	0.15	0.03	0.01	0.03	0.07	0.12	0.19
Leverage	0.20	0.04	0.16	0.31	0.20	0.22	0.06	0.18	0.32	0.19	0.15
Market-to-Book	1.90	1.04	1.35	2.06	1.68	1.59	1.02	1.26	1.78	0.92	0.00
Institutional Ownership	0.72	0.58	0.76	0.89	0.23	0.71	0.57	0.75	0.88	0.22	0.65
Inst. Ownership HHI	0.06	0.04	0.05	0.07	0.06	0.06	0.04	0.05	0.06	0.07	0.62
CEO Total Comp.	4.76	1.46	3.00	6.20	4.96	5.44	1.73	3.64	6.93	5.17	0.01
% of Stock-Based Comp.	0.40	0.23	0.42	0.57	0.27	0.42	0.27	0.45	0.57	0.23	0.29
Growth in CEO Comp.	0.47	-0.07	0.18	0.59	1.62	0.42	-0.10	0.12	0.48	1.55	0.58

Panel B

	Voting support					Number of observations by voting outcome		
ISS recommendation	Mean	10th	50th	90th	SD	Fail	Pass	Total
Against	68.9%	48.5%	68.9%	88.7%	15.2%	29	227	256
For	93.2%	83.3%	95.6%	99.1%	7.0%	0	1,764	1,764
Total (Against and For)	90.1%	73.5%	94.9%	99.0%	11.7%	29	1,991	2,020

Panel A presents the summary statistics of the variables used in the study for the full sample of 2,020 say-on-pay proposals in 2010-2011 and for the 404 observations in the 5% bandwidth around the cutoff. The last column in Panel A shows the p-values for the difference in means test between the full sample and the 5% bandwidth. Panel B presents the distribution of voting support and the number of fail vs. pass voting outcomes depending on the ISS recommendation, where a proposal fails if it receives less than 50% support. The last column presents the total number of observations with a given ISS recommendation. Variable definitions are provided in the appendix.

Table 2: Probability of a negative ISS recommendation (first stage)

	(1)	(2)	(3)	(4)	(5)
	NegRec	NegRec	NegRec	NegRec	NegRec
BelowCutoff	0.145**	0.146**	0.146**	0.148**	0.142**
	(0.068)	(0.069)	(0.070)	(0.067)	(0.067)
MaxTSR	-0.008	-0.010	-0.017	-0.012	-0.014
	(0.012)	(0.016)	(0.016)	(0.016)	(0.016)
BelowCutoff·MaxTSR		0.004	0.013	0.009	0.010
		(0.024)	(0.024)	(0.023)	(0.023)
CEO Total Compensation				0.007*	0.018***
				(0.004)	(0.006)
Growth in CEO Total Compensation				0.062***	0.061***
				(0.012)	(0.012)
Proportion of Stock-Based Compensation				-0.064	-0.060
				(0.093)	(0.095)
Institutional Ownership				-0.007	0.004
				(0.096)	(0.096)
Insider Ownership				0.412***	0.360***
				(0.125)	(0.127)
Log(Market Value of Equity)					-0.043**
					(0.020)
ROA					-0.133
					(0.172)
M/B					0.024
					(0.023)
Year FE	No	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	Yes
Observations	403	403	403	394	393
R^2	0.062	0.062	0.076	0.184	0.198
F-statistic	4.49	4.52	4.33	4.85	4.45

The table presents the first-stage estimates and shows that the probability of a negative ISS recommendation increases discontinuously for firms below the ISS cutoff. All specifications are estimated on a 5% bandwidth. The outcome variable is *NegRec*, which equals one if ISS gives a negative recommendation, and zero otherwise. The main variable of interest is *BelowCutoff*, which equals one if the firm is below the cutoff ($MaxTSR < 0$), and zero otherwise. We estimate a linear probability model, and hence the coefficient on *BelowCutoff* measures the difference in the probability of a negative recommendation between firms just below and just above the cutoff. Standard errors are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Effect of ISS recommendations on voting outcomes (second stage)

	(1) Votes	(2) Votes	(3) Votes	(4) Votes	(5) Votes
NegRec	-24.620**	-25.111**	-29.001**	-28.717***	-26.555**
	(11.210)	(11.160)	(11.588)	(10.655)	(10.853)
MaxTSR	0.045	0.175	0.129	0.125	0.226
	(0.358)	(0.450)	(0.531)	(0.455)	(0.463)
BelowCutoff·MaxTSR		-0.294	-0.302	-0.198	-0.277
		(0.560)	(0.585)	(0.544)	(0.534)
CEO Total Compensation				-0.273**	-0.677***
				(0.120)	(0.241)
Growth in CEO Total Compensation				0.261	0.167
				(0.720)	(0.717)
Proportion of Stock-Based Compensation				-0.047	-0.210
				(2.228)	(2.218)
Institutional Ownership				-5.597**	-6.009***
				(2.246)	(2.186)
Insider Ownership				13.023**	14.370***
				(5.292)	(4.861)
Log(Market Value of Equity)					1.658**
					(0.665)
ROA					5.750
					(4.247)
M/B					0.353
					(0.585)
Year FE	No	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes	Yes
Observations	403	403	403	394	393
R^2	0.547	0.547	0.536	0.605	0.628
OLS coefficient on NegRec	-25.339***	-25.338***	-25.052***	-26.070***	-25.443***
	(1.185)	(1.186)	(1.227)	(1.234)	(1.222)
Durbin-Wu-Hausman test p-value	0.949	0.984	0.720	0.795	0.915

The table shows that a negative ISS recommendation causes a significant decline in say-on-pay voting support. The outcome variable is *Votes*, the percentage of votes in favor of a say-on-pay proposal, measured in percentage points. The main explanatory variable is *NegRec*, which equals one if ISS gives a negative recommendation, and zero otherwise. Estimation is conducted via 2SLS, where *NegRec* is the instrumented variable, the first stage is estimated in Table 2, and the second stage is estimated in this table. The last three rows present the coefficient and standard error on *NegRec* in the OLS regression of *Votes* on *NegRec* with the same set of regressors as in the corresponding 2SLS regression, and the p-value of the Durbin-Wu-Hausman test for equality of the OLS and IV estimates. All specifications are estimated on a 5% bandwidth. Standard errors are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Variation in the effect of ISS across firms

	(1) Inst.Ownership HHI		(2) % Inst.Blockholders		(3) % Top 10 Institutions		(4) % Passive Institutions		(5) Inst.Ownership	
	Low	High	Low	High	Low	High	Low	High	Low	High
BelowCutoff	0.164**	0.158**	0.126*	0.169**	0.163**	0.156**	0.201***	0.159**	0.176**	0.187***
(First Stage)	(0.071)	(0.066)	(0.070)	(0.073)	(0.070)	(0.066)	(0.070)	(0.066)	(0.068)	(0.069)
NegRec	-41.12***	-21.27*	-35.72**	-25.56**	-37.48***	-23.38**	-18.87**	-38.20***	-24.08***	-32.30***
(Second Stage)	(12.07)	(11.04)	(16.33)	(11.95)	(11.20)	(11.21)	(7.78)	(11.76)	(8.65)	(10.19)
MaxTSR controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	386	386	337	336	385	387	384	384	388	389

The table shows how the effect of ISS varies with the firm's ownership structure. *Inst.Ownership HHI* is the Herfindahl-Hirschman index, defined as the sum of squared share ownership over all institutional investors. *% Inst.Blockholders* is the percent of institutional shares held by blockholders, defined as the ratio of total ownership by institutional blockholders to total institutional ownership (instblockown/instown from Thomson Reuters). *% Top 10 Institutions* is the percent of institutional shares held by the top 10 largest institutions, defined as the ratio of ownership by the top 10 institutional investors to total institutional ownership (top10instown/instown from Thomson Reuters). *% Passive Institutions* is the percent of shares held by "transient" and "quasi-index" institutional investors in the classification of Bushee and Noe (2000). *Inst.Ownership* is total institutional ownership in fraction of shares outstanding. For each ownership characteristic, we first restrict the sample to observations with $MaxTSR$ within a 10% bandwidth and calculate the median value of the ownership characteristic in the resulting sample. Next, we divide this sample into two subsamples, based on whether the corresponding ownership characteristic falls below or above the median, and refer to the first and second subsample as the "Low" and "High" subsample, respectively. We then repeat the 2SLS procedure on each of the two subsamples. Each specification controls for $MaxTSR$, *BelowCutoff*$\cdot MaxTSR$, and year and industry fixed effects. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Robustness

	(1) 3%	(2) 4%	(3) 5%	(4) 6%	(5) 7%	(6) 8%	(7) 9%	(8) 10%	(9) 20% Quadratic	(10) 20% Cubic	(11) 20% Quartic
BelowCutoff	0.18**	0.17**	0.15**	0.19***	0.19***	0.19***	0.20***	0.18***	0.19***	0.19***	0.21***
	(0.09)	(0.08)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.08)
NegRec	-20.33*	-24.68**	-29.00**	-27.99***	-21.09***	-23.47***	-23.29***	-28.56***	-22.38***	-23.62***	-23.91**
	(12.01)	(10.26)	(11.59)	(8.33)	(7.81)	(7.08)	(6.37)	(6.93)	(7.07)	(9.16)	(10.18)
Observations	238	313	403	490	574	651	717	785	1,244	1,244	1,244

The table shows robustness to the choice of the bandwidth and the degree of the polynomial. The first two rows present the estimate and standard error of the coefficient on *BelowCutoff* in the first-stage regression. The third and fourth rows present the estimate and standard error of the coefficient on *NegRec* in the second-stage regression. Columns 1-8 present the linear specification on bandwidths between 3% and 10%. Columns 9-11 present the estimation of the second, third, and fourth-order polynomial functions of $MaxTSR$ on a 20% bandwidth, allowing for different functional forms around the cutoff. All specifications control for year and industry fixed effects. For example, the regressors in column 9 include *BelowCutoff*, $MaxTSR$, $MaxTSR^2$, *BelowCutoff*$\cdot MaxTSR$, *BelowCutoff*$\cdot MaxTSR^2$, and year and industry fixed effects. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Distribution of firm characteristics around the cutoff

	RD coeff. on BelowCutoff	(-5%, 0) Mean	(-5%, 0) Obs	(0, 5%) Mean	(0, 5%) Obs	Diff. in means p-val
Log(Market Value of Equity)	-0.02 (0.29)	0.71	173	0.58	230	0.39
M/B	-0.10 (0.15)	1.56	173	1.62	229	0.49
ROA	-0.03 (0.02)	0.03	173	0.03	230	0.82
Leverage	0.00 (0.04)	0.21	165	0.22	216	0.74
CEO Total Compensation	0.06 (0.96)	5.81	173	5.17	230	0.22
CEO Compensation Percentile	1.29 (5.21)	60.3	173	57.9	230	0.38
Proportion of Stock-Based Compensation	0.06 (0.04)	0.42	173	0.42	230	0.91
Proportion of Incentive Pay	0.02 (0.05)	0.53	170	0.55	226	0.51
1-year growth in CEO Total Compensation	0.06 (0.29)	0.44	172	0.41	227	0.85
3-year growth in CEO Total Compensation	-0.03 (0.04)	0.05	154	0.05	207	0.91
Institutional Ownership	-0.03 (0.04)	0.72	173	0.71	230	0.62
Insider Ownership	0.01 (0.03)	0.12	169	0.09	226	0.12

The table shows that the distribution of firm characteristics is smooth around the cutoff. For each characteristic in the first column, column "RD coeff. on BelowCutoff" presents the results of a local linear regression of this characteristic on *BelowCutoff*, $MaxTSR$, *BelowCutoff*$\cdot MaxTSR$, and year and industry fixed effects using a 5% bandwidth. The estimated coefficients on *BelowCutoff* are reported in the first row, and standard errors are reported in parentheses. Subsequent columns present the means of each characteristic in two intervals: $-5\% < MaxTSR < 0$ and $0 < MaxTSR < 5\%$, as well as the number of observations in these intervals. The last column presents the p-values for the difference in means test. CEO Compensation Percentile is the percentile of CEO Total Compensation in the firm's four-digit GICS group in that year. Proportion of Incentive Pay is the proportion of total compensation represented by incentive pay (variable CEOVariableTPM from GMI Ratings). 3-year growth in CEO Total Compensation is defined as (CEO Total Compensation$_t$ $-$ CEO Total Compensation$_{t-3}$)/CEO Total Compensation$_t$. Other variable definitions are provided in the appendix.

Table 7: Instrument strength

	5%	6%	7%	8%	9%	10%
First-stage F-statistic	4.33	8.55	10.46	12.71	16.06	14.32
Anderson-Rubin p-value	0.086	0.018	0.055	0.018	0.009	0.003
Reduced form regression	-4.22*	-5.20**	-3.91**	-4.45**	-4.63***	-5.07***
	(2.42)	(2.17)	(1.99)	(1.84)	(1.74)	(1.65)
Observations	403	490	574	651	717	785

The first row presents the first-stage F-statistic for bandwidths ranging between 5% and 10%. The second row presents the p-value of the Anderson-Rubin statistic for the second-stage coefficient on *NegRec*, which is obtained using the Stata package *condivreg*. The third and fourth rows present the estimate and standard error of the coefficient on *BelowCutoff* in the reduced form regression of *Votes* on *BelowCutoff*. In all the tests and for all bandwidths, we control for $MaxTSR$, *BelowCutoff*$\cdot MaxTSR$, and year and industry fixed effects. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.