Market Feedback Effect on CEO Pay: Evidence from Peers' Say-on-Pay Voting Failures

C.S. Agnes Cheng
University of Oklahoma Steed School of Accounting, Price College of Business, agnescheng@ou.edu

Iftekhar Hasan
Fordham University, Gabelli School of Business, ihasan@fordham.edu

Feng Tang
University of Macau, Faculty of Business Administrationm fengtang@um.edu.mo

Jing Xie
University of Macau, Department of Finance and Business Economics, jingxie@um.edu.mo

Abstract

This paper shows that when a compensation peer firm experiences a significant failure in its say on pay (SOP) voting, the focal firm's stock price is adversely affected, resulting in reduced CEO pay in the subsequent period. This pay-reduction effect is amplified when the board is more powerful, when proxy advisors express concerns about CEO pay, and when the compensation consultant lacks quality. Directors who react to the price drop and cut the CEO's pay receive higher votes in future director elections, implying a market feedback effect for directors of the focal firm triggered by their peers' SOP voting failure.

For helpful comments and discussions, we thank Thomas Chemmanur, Wenli Huang, Peter Truc Do, conference participants at the 2019 Annual Congress of European Accounting Association (EAA), the 2020 Canadian Academic Accounting Association (CAAA) Annual Conference, the 2022 Financial Management Association (FMA) Annual Conference, and seminar participants at the National Chengchi University. An earlier version of this paper was circulated under the title, "Spillover Effects of the Mandated Say-on-pay Votes."

1. Introduction

In various ways, stock prices influence the decisions CEOs make (Bond, Edmans, and Goldstein, 2012). Prior literature refers to this phenomenon as the market feedback effect, whereby decision-makers learn private information from changes in stock prices. This information is not available to the firm's management (Chen, Goldstein, and Jiang, 2007; Dittmar, Duchin, and Zhang, 2020; Zuo, 2016). However, it remains unclear whether short-term fluctuations in firm stock prices around corporate-governance events affect subsequent corporate policies. Our research investigates whether the market feedback effect applies to boards when they determine CEO pay.

Public opinion, including that of investors, is important to directors when deciding on CEO compensation. For instance, Kuhnen and Niessen (2012) demonstrate that public opinion, as reflected in media coverage, can influence the level and structure of CEO pay. In a recent survey, Edmans, Gosling, and Jenter (2023) found that 60% of directors offer less CEO pay to avoid the risk of investor opposition, highlighting the significance of shareholder opinion. Thus, we propose that the market feedback effect may prompt boards to consider shareholder sentiment when setting CEO pay. Board directors have an incentive to do so, as failing to take such feedback into account could harm their reputations; incorporating it, on the other hand, could improve their standing and support from investors.

Previous research explores the positive relationship between CEO pay and firm performance over the course of one year or longer (Jensen and Murphy, 1990; Rajgopal, Shevlin, and Zamora, 2006; Daniel, Li, and Naveen, 2020). However, this relationship is not a typical feedback effect, as long-term stock returns mainly reflect changes in company fundamentals stemming from multiple events. In this study, we instead investigate a particular type of governance event that triggers short-term stock price movements, which in turn reflect shareholder attitudes toward the

specific event. We then examine whether boards learn from this market feedback. Our analysis of abnormal stock returns around governance events is motivated by Becker, Bergstresser, and Subramanian (2013), who infer that shareholders value the access to proxy voting.

One empirical challenge is identifying governance events that do not directly affect a company's future cash flows but that influence investors' views on CEO pay. To address this challenge, we measure a firm's abnormal returns around compensation peers' say-on-pay (SOP) voting failures (i.e., negative votes are over 50%) and examine whether these abnormal returns affect the firm's future CEO pay.

We focus on compensation peers' SOP voting events for four reasons. First, these events are more exogenous to a firm's future cash flows than are the firm's own governance events. Second, peer firms' SOP voting failures may prompt a focal firm's investors to question whether their own CEO is overpaid and express their opinions through the firm's stock price. Third, directors of focal firms are responsible for choosing peers against which to benchmark CEO pay and, therefore, they care how investors view those peers. Finally, SOP voting failures are salient governance events that attract significant investor attention. According to Semler Brossy's report (2019), the percentage of Russell 3000 firms with SOP votes that fail is very low (only 2.6% in 2018). The rarity of such failures suggests their importance.

Our paper is related to Denis, Jochem, and Rajamani (2020). They find that firms cut CEO pay when their compensation benchmarking peers receive weak shareholder support in SOP voting. They suggest the effect travels through two channels: learning from peers' voting outcomes and maintaining compensation targets at the peer pay level. We offer a novel explanation for the spillover effect beyond these two proposed channels. Particularly, we differentiate the information sources for learning from price versus learning from peers' voting outcomes. We provide direct

evidence that learning from price has an incremental effect, beyond the other two channels, in explaining board pay-cut decisions.

We obtain data from the Incentive Lab database for compensation peers and from Semler Brossy's annual say-on-pay reports for the 2011-2018 proxy seasons. We identify 686 focal firms (1,351 firm-event pairs) whose compensation peers' SOP votes failed. To measure the impact of these events on focal firms, we calculate the focal firm's cumulative abnormal market-adjusted return (CAR) for the three days around the dates of the SOP-failed peer's annual shareholder meeting. We consider a voting failure as severe for focal firms if either the percentage of negative votes the SOP-failed peer receives or the proportion of SOP-failed peers in the focal firm's compensation benchmarking group is in the top quintile of the sample distribution. We find that increasing the severity of the compensation peers' SOP voting failure events decreases focal firms' announcement returns. This suggests that focal firms' shareholders are attentive to inappropriate pay-setting among compensation peer firms and that they express dissatisfaction with the resulting biased executive pay. Furthermore, we find that the R^2 of the prediction model, which regresses focal firms' CAR on peers' voting outcomes and other control variables, is low (13.4%), indicating that investors have private information about the focal firm's compensation that peers' voting failures or other characteristics do not fully convey. Hence, boards may learn additional information from CAR.

Our study examines whether boards of focal firms recognize that a decline in stock prices indicates investor dissatisfaction, which prompts them to reduce CEO compensation (i.e., the feedback effect). We focus on firms with compensation peer SOP failures and analyze their CEO compensation in the year following the peer's annual meeting relative to the prior year. Our analysis reveals that firms experiencing more negative CAR after their peers' voting failures show

a more significant decrease in CEO pay. This finding supports the feedback-effect hypothesis. Specifically, we find that firms experiencing a negative market reaction reduce CEO pay by more than 12.54% (approximately $1.08 million) in the subsequent year, compared to firms with no adverse market spillover effect.

To explore further how boards learn from prices, we present three tests based on variations in board learning incentives. First, we hypothesize that when a board has more bargaining power than the CEO, the feedback effect is stronger, as a more powerful or independent board has greater incentives to consider and incorporate investor views into the CEO pay decision.

Second, we posit that the feedback effect is stronger when the board is under pressure from proxy advisory firms. These firms can influence shareholder voting on board elections, affecting directors' careers. We use data from Institutional Shareholder Services, Inc. (ISS), the world's largest proxy advisor. We find that the feedback effect is stronger if ISS expresses a negative view of the focal firm's CEO pay in the previous year. This suggests that pressure from ISS raises boards' incentives to be attentive to investors' opinions conveyed through prices.

Third, we posit that the feedback effect is stronger when boards lack high-quality compensation-related information from other sources. Boards often seek compensation-related information from professional consultants whose quality is inconsistent. To the extent that consultant opinions and information from prices substitute for each other, we predict that board members learn more from prices when their consultants cannot offer high-quality opinions. We find that the feedback effect is stronger for focal firms that use smaller consulting firms (i.e., consultants with fewer clients).

To strengthen the causal inference in our study, we conduct a series of placebo tests. First, we analyze price movements around randomly selected pseudo dates. Specifically, we analyze

focal firms' abnormal returns around a set of pseudo-SOP-event dates randomly selected from within 50 days before to 50 days after the actual SOP-event dates. Second, we analyze a sample of focal firms without SOP-failed compensation peers but with SOP-failed industry peers that are not compensation peers. In all these placebo tests, we find that abnormal returns around the pseudo-event dates do not lead to a significant decrease in focal firms' CEO compensation, indicating that the information related to SOP events is what is most important in the feedback effect. Altogether, the placebo tests help establish a causal link between the SOP events and subsequent CEO compensation changes observed in our study.

We also perform various robustness tests to ensure the validity of our findings. We add matched firms with non-SOP-failed peers as additional benchmark firms, employ an alternative sample of firms whose SOP voting dates do not overlap with the event dates of SOP-failed peers, and use an alternative dependent variable (excess CEO compensation). We also test a set of alternative ways of defining large price drops and alternative definitions of pre- and post-event windows. In all cases, we find that the feedback effect remains robust, further supporting the validity and generalizability of our results.

To investigate the impact of learning on directors' future careers, we examine firms that experience negative price reactions around peers' bad governance events. We find that when the focal firm's board reduces CEO pay in response to shareholder feedback via adverse price movements, shareholder satisfaction increases and, subsequently, shareholders vote more favorably for incumbent directors. In a director-firm-level analysis, we show that directors, particularly those on compensation committees, who are at firms that cut CEO pay receive significantly stronger shareholder support in elections than do directors at other focal firms. This suggests that directors who are responsive to shareholder opinions enhance their career prospects.

Our study contributes to the literature in several ways. First, it expands the research on the feedback effect on corporate-governance decisions. Prior studies do not explore how boards learn from shareholder responses to specific events. We contribute to this literature by showing that the feedback effect is relevant for boards making critical decisions such as setting CEO pay. We demonstrate that boards pay attention to shareholder dissatisfaction (expressed in the market's response to SOP-failure news from a compensation-peer firm), and this feedback effect drives them to reduce CEO pay. Our paper is also related to Ferreir, Ferreira, and Raposo (2011), who show that price informativeness and board monitoring are substitutes.

Second, our paper contributes to the literature by enriching the understanding of information transfers between focal firms and compensation peers. We show that investors use news about compensation peers to infer valuable compensation-related information, similar to traditional connections such as industry or supply-chain relations. We investigate focal firms' immediate market reactions surrounding the shareholder meeting dates of their SOP-failed peers and find that news of a peer's SOP failure has a negative externality on focal firms' stock prices. This effect is significant only for noteworthy news. Our findings are consistent with Francis et al. (2016), who show that CEOs learn from compensation peers. Our paper is related to but also differs from Denis, Jochem, and Rajamani (2020). We provide direct evidence that learning from price has an incremental effect, beyond the severity of the compensation peers' SOP failure, in explaining board pay-cut decisions. Moreover, we show that learning from price benefits directors in that they receive stronger shareholder support in future elections.

Third, our study also contributes to the literature on director reputation and effectiveness (e.g., Duchin, Matsusaka. and Ozbas, 2010; Iliev et al. 2015; Fos, Li, and Tsoutsoura, 2018; Aggarwal, Sandeep, and Prabhala, 2019). Specifically, we find that directors who respond to

shareholder dissatisfaction by cutting CEO pay receive greater shareholder voting support in future director elections. This result suggests that directors benefit from building shareholder-friendly reputations, which is consistent with prior research on the importance of director reputation (Coles and Hoi, 2003). Additionally, our study highlights the effectiveness of board governance and director elections in incentivizing directors to respond to shareholder concerns and make decisions that align with shareholder interests. Overall, our findings provide valuable insights into how market feedback shapes board decision-making and the effectiveness of corporate-governance mechanisms in promoting shareholder value.

The remainder of this paper is organized as follows. Section 2 reviews the related literature and develops testable hypotheses. Section 3 presents the empirical analysis of the abnormal stock return around peer firms' SOP events and their impact on focal firms' CEO pay. Section 4 presents the impact of board learning on shareholder voting outcomes in future director elections, and section 5 concludes the paper.

2. Related Literature and Hypothesis Development

2.1 Impact of the Feedback Effect on Corporate Policy

To understand how stock prices affect corporate decisions, previous research examines the real effects of the stock market on various corporate policies such as investment decisions (Baker, Stein, and Wurgler, 2003; Chen, Goldstein, and Jiang, 2006; Campello and Graham, 2013; Edmans, Jayaraman, and Schneemeier, 2017; Bird et al., 2020; Jayaraman and Wu, 2020), takeovers (Edmans, Goldstein, and Jiang, 2012; Baker, Pan, and Wurgler, 2012), financing decisions (Dittmar, Duchin, and Zhang, 2020; Hovakimian and Hu, 2020), and voluntary disclosure decisions (Zuo, 2016). This is because stock prices provide valuable information to which CEOs

do not otherwise have access. We use the terms "learning" and "feedback" interchangeably because, in a broader sense, market prices can confirm underlying information, even if decision-makers do not learn new information. Prior studies analyze the effects of feedback on corporate policies, and we extend this literature by examining the impact of feedback on board governance decisions, specifically the decision to set CEO pay.

Recent studies employ abnormal stock returns surrounding a specific corporate event to measure private investor information not known to managers. This method ensures that stock-price changes reflect investors' private information about a particular issue, which improves the accuracy of the information managers can learn from stock prices. For instance, Jayaraman and Wu (2020) show that managers increase (decrease) annual capital expenditures in response to positive (negative) stock-market reactions to capital expenditure forecasts. Previous research indicates that the market reacts positively to news related to SOP that enhances corporate governance (Cai and Walkling, 2011; Illiev and Vitanova, 2019).

2.2 Spillover Effects of Shareholder Activism

Although prior literature examines the spillover effects of shareholder activism, little research explores the spillover effects of a mandated SOP vote.[1] One notable exception is the study by Denis, Jochem, and Rajamani (2020), who investigate a sample of S&P 1500 firms and their compensation benchmarking peers that receive weak SOP voting support (less than the 10th percentile of the vote distribution for Russell 3000 firms) from 2011 to 2012. Their findings

[1] Ferri (2012) claims that it is cost-effective for shareholder activists to generate the "market-wide adoption of best practices" from their initiatives. Several studies find that spillover effects are present for shareholder activism that takes place through shareholder proposals and shareholder votes on director elections. Ferri and Sandino (2009) investigate 153 shareholder proposals from 2003 to 2004 requesting that firms expense employee stock options (ESO). They find that when a firm is targeted by an ESO proposal, the probability that its peers (firms in the same four-digit SIC industry) will expense their ESO increases. Ferri and Sandino (2009) also show the effects of ESO proposals on target firms. The firms targeted by these shareholder proposals are more likely to expense ESO, and proposal approval leads to a decrease in CEO pay of around $2.3 million. Ertimur, Ferri, and Stubben (2010) analyze 620 advisory corporate-governance-related shareholder proposals supported by majority votes (MV proposals) among S&P 1500 firms from 1997 to 2004. They show that firms are more likely to implement a MV proposal if its peers (firms in the same two-digit SIC industry) implement a similar proposal in the past two years.

suggest that non-target firms reduce executive pay following their peers' voting failure. The authors identify two channels through which this effect occurs: First, boards directly learn governance information from peer firms' voting outcomes; second, when compensation peers reduce CEO pay, focal firms' boards follow suit to maintain the compensation benchmarking.

2.3 Hypothesis Development

In the context of the SEC's SOP rule, if a spillover effect exists, the voting failure of a compensation benchmarking peer should lead to dissatisfaction among the focal firm's shareholders about executive pay based on an improper benchmark and possible overstatement. This argument aligns with Gleason, Jenkins, and Johnson (2008), who observe negative market reactions among the peers of restated firms due to investor suspicions about financial reporting quality. Dissatisfied investors may sell the focal firm's shares upon hearing about their compensation peers' SOP failure. The focal firm's abnormal return around compensation peers' SOP news should be lower when the latter's voting failure is more severe, as investors are more likely to follow news about other firms they consider important. The spillover effect on the focal firm should be stronger when a peer firm experiences a more substantial abnormal return, as it partially reveals the event's severity. Hence, we present our first hypothesis as follows:

H1: When the severity of a compensation peer's SOP voting failure increases, the focal firm's CAR around the announcement date of that voting failure decreases.

Prior research suggests that managers can learn private information from investors. Specifically, when a focal firm experiences a negative market response to its compensation peers' SOP voting failure, it indicates shareholder dissatisfaction with the focal firm's existing CEO compensation. This private information can help the board appropriately adjust the CEO compensation.

We argue that boards take note of shareholder responses to peers' SOP news and use this information to inform future decisions. Therefore, if a firm experiences a more negative abnormal return following its compensation peers' SOP news and does not take any corrective action, the firm is more likely to face a voting failure and severe reputational damage. As a result, we expect these firms to be more motivated to repair their reputations and mitigate the risk of future voting failures by improving their executive pay practices, such as substantially decreasing CEO pay. Thus, we present our second hypothesis as follows:

H2: Focal firms that experience a lower CAR after a peer's SOP event cut CEO pay to a greater extent than firms experiencing a higher CAR after such an event.

We investigate three factors that affect the feedback effect. First, the feedback effect's strength depends on the relative power of the CEO and the board. When the board is more powerful than the CEO, a negative CAR should be more effective in reducing excessive CEO pay because the CEO is more likely to accept a pay cut. Additionally, board members may find it beneficial to establish a reputation for being shareholder-friendly (by cutting CEO pay) rather than management-friendly (by not cutting CEO pay), as both reputations exist in the board labor market (Levit and Malendo, 2016).

Second, the feedback effect depends on third-party intermediaries' attitudes toward the CEO pay and the availability of alternative information channels. Board members are more likely to respond to investor sentiment communicated through stock prices, and they are more likely to reduce CEO pay when third-party intermediaries such as proxy advisory firms express concerns.

Third, we examine the interaction between professional compensation consultants and information from stock prices. If consulting opinions and stock prices are substitutes, board members are more likely to rely on stock price when the consultant cannot provide reliable advice.

We predict that the feedback effect is stronger for firms that hire smaller compensation consultant firms (i.e., with fewer clients) because these firms are less likely to provide reliable advice. Hence, we present our third hypothesis, as follows:

H3: CEO pay after negative CAR is lower when the focal firm's board is more powerful, when ISS expresses a negative view of the firm, or when the firm hires a smaller compensation consultant.

Investors reward (or punish) directors who learn information about their opinions on CEO pay from stock prices. In particular, if directors cut CEO pay after adverse returns (around peers' SOP failure events), it indicates learning from investor opinions revealed in stock prices. Consequently, investors should perceive such directors as more capable and worthy of higher support in future director elections. We argue that the relation between CEO pay cuts and favorable voting focuses mainly on directors on compensation committees (the effect may exist to a lesser extent for directors not on compensation committees, because some investors may not have information to distinguish these two types of directors).

This prediction is consistent with Coles and Hoi (2003), who find that directors who demonstrate shareholder-friendly behavior and monitor management are more likely to gain additional directorships. The prediction is also consistent with Fos and Tsoutsoura (2014), who find that proxy contests have a significant adverse impact on incumbent directors. Moreover, anecdotal evidence supports our prediction, as some institutional investors explicitly state their intention to vote for directors who take into account investor concerns about executive pay.[2]

[2] For example, directors of the technology firm Oracle (ORCL) did not heed the "advice" investors gave them about excess CEO pay from 2012 to 2016. Investors responded by voting against certain board directors. Consequently, in 2016, only six of Oracle's 13 directors received greater than 90% support for reelection. Knowing their board seats were in jeopardy, Oracle's compensation committee members cut executive long-term equity grants in half for fiscal year 2018. See "Oracle's Road To Moving The Needle On 'Say On Pay' Votes," *Forbes*, February 26, 2018. https://www.forbes.com/sites/robinferracone/2018/02/26/oracles-road-to-moving-the-needle-on-say-on-pay-votes/?sh=654432f41348

Hence, we present our fourth hypothesis as follows:

H4: Among focal firms that experience a negative event CAR, directors (especially those on compensation committees) at firms that cut CEO pay receive significantly stronger shareholder support in elections than do directors at other firms.

3. Empirical Results of Board Learning from Stock Prices

3.1 Data and Sample

We obtain data from Incentive Lab on the composition of the peer groups used for compensation benchmarking. The vendor provides detailed data on executive compensation for S&P 500 firms and for the majority of S&P 400 firms. To attract and retain competent managers, firms typically benchmark executive pay against comparable peers that are competitors for labor-market talent (Faulkender and Yang, 2010; Bizjak, Lemmon, and Nguyen, 2011; Albuquerque, DeFranco, and Verdi, 2013). Starting in 2006, the SEC requires public firms to disclose in the CD&A, "any benchmarking of total compensation or any material element of compensation, identifying the benchmark and, if applicable, its components (including component companies)" (*SEC final rules 33-8732a: Executive Compensation and Related Person Disclosure*). According to Equilar's (2016) report, more than 95% of S&P 500 firms disclose the use of compensation peers in determining executive pay. Appendix A provides an example of Apple, Inc.'s disclosure regarding its compensation benchmarking peer group. We utilize these actual self-selected peer firms, instead of hypothetical peers constructed based on certain firm characteristics (e.g., firm size and industry membership), to document directly the feedback effect of SOP voting. Compensation committees typically consider two types of peers in a compensation benchmark: compensation peers, which are used for setting overall CEO pay, and bonus peers, which determine performance-based awards

(Francis et al. 2016; Edmans, Gabaix, and Jenter, 2017; Bizjak et al 2022). We focus on compensation peers in our paper because relatively few firms report bonus peers (Francis et al. 2016).

We identify firms that fail in SOP voting (i.e., negative votes exceed 50%) from Semler Brossy's annual say-on-pay reports. An SOP vote failure indicates that investors are highly dissatisfied with existing CEO pay. We obtain financial accounting data from Compustat, executive compensation data from ExecuComp, stock-price data from CRSP, corporate-governance information from Institutional Investors Services (ISS), and institutional shareholdings from Thomson Reuters Institutional (13F) Holdings.

Panel A of table 1 reports the sample-selection process. We start with 7,432 firm-year observations representing 1,157 unique focal firms covered by Incentive Lab during the proxy seasons of 2011-2018. We first exclude 364 observations that are missing data needed for the empirical analyses. We then exclude 135 observations in which the focal firms fail in the SOP voting, because the market reaction to the focal firms' voting failure may confound the spillover effect from the peer failures.[3] We further exclude 5,582 observations (1,088 focal firms) without peers that fail in the voting. Hence, the final sample consists of 1,351 firm-event observations (686 focal firms) with at least one peer that experiences voting failure (85.6% of the focal firms have only one SOP-failed peer, 12.1% have two peers that fail in the SOP voting, and the remaining 2.3% have more than two peers with the voting failure). For the analyses on pay changes from the pre- to post-event years, each firm-event observation corresponds to two firm-year observations (i.e., one observation for the pre-event year and one for the post-event year). Accordingly, there

[3] In an untabulated analysis of the 135 SOP-failed focal firms, the negative market reaction for these target firms has a significantly positive association with the severity of their own voting failure (measured by the percentage of negative votes). The exclusion of these firms from the sample alleviates the potential confounding effect in our main test for a market feedback effect related to the peer event.

are 2,702 firm-year observations in the main sample.

3.2 Summary Statistics

Panel B of table 1 reports the descriptive statistics for the variables used in the main empirical analyses. The mean of the focal firms' total CEO pay (*CEOPAY*) is $8.6 million, and the natural logarithm of the focal firms' CEO compensation (*LN CEOPAY*) has a mean of 8.717. The mean of focal firms' market reactions to their peers' voting failure (*CAR*) in the full sample is close to zero, suggesting that news of compensation peers' SOP voting failures does not affect all focal firms.

Regarding event severity, the mean percentage of negative votes the SOP-failed peers receive is 61.7%, and the mean number (percentage) of SOP-failed peers is one (7.9%) in the focal firms' compensation peer group (with a mean group size of 22). Regarding SOP-failed peers' own market reactions to the event, the mean three-day *CAR* is -0.3%. On average, the common institutional ownership between the focal firm and its SOP-failed peer (*COMINST PCT*) is 52.6%. For 1.0% of the focal firms, at least one director has a board seat at the SOP-failed peers (*INTERLOCK*), and 1.9% of the focal firms have an industry peer that is not the compensation peer that fails in the SOP voting (*INDPR FAILSOP*).

[Table 1]

Relative to compensation peers, 44.9% of focal firms have CEO compensation above the median pay for the peer group (*CEOPAY ABOVE*). The mean firm size (*SIZE*) is 8.837, equivalent to approximately $6.9 billion of market capitalization. The means of the market-adjusted annual stock returns and the industry-adjusted return on assets are 0.026 (0.060) and 1.097 (0.654) for the current (prior) year, respectively.[4] CEO tenure is 8.1 years, on average. The mean CEO ownership is 1.3%, and the mean institutional ownership is 60.0%. The mean percentage of favorable votes

[4] For the raw ROA (not adjusted by industry average), the mean and median are 0.060 and 0.052, respectively.

is 76.2%, and 9.2% of the focal firms experience a negative voting recommendation by ISS.

With respect to peer firms' characteristics and labor-market conditions, the mean *PR CEOPAY* is 9.298, which is equivalent to approximately $10.8 million of CEO compensation paid by the SOP-failed peers; these peers' average stock performance (*PR RET*) is 7.1%, and accounting performance (*PR ROA*) is 4.0%, on average. For 37.8% of the focal firms, at least one of the top five executives moves from or to the industry of their SOP-failed peers. In terms of similarity of CEO capability, the mean difference in the focal firms' CEOs' stock performance relative to their SOP-failed peers is 20.5%.[5] Also, 7.2% of the focal firms experience CEO turnover, and 9.4% of the focal firms have a CEO who is 65 years old or older.

To explore the similarities and differences among the focal firms in our sample and the other firms covered by Incentive Lab, we compare the main variables for the firms with SOP-failed peers (1,351 firm-year observations in the final sample) and the firms without any peers that fail in the voting (5,582 firm-year observations, which we exclude from the final sample).

We report the results in appendix C. We find that for CEO-related variables, the two groups of focal firms have similar levels of CEO pay (*LN CEOPAY*) and their compensation peers' (CPs') pay (*PR MEDPAY*) is also similar. However, the CP-SOP-failure group (in the first two columns) has higher co-opted boards and longer CEO tenure than the remaining firms, implying that the directors in the former group are less effective in identifying proper CPs than are directors in the latter group. We find that for firm fundamentals, the CP-SOP-failure group has similar firm size, stock returns, and institutional ownership, but lower ROA, book-to-market ratio, and leverage compared to the remaining firms. For voting-related variables, we find no significant differences

[5] We measure the similarity of CEO capability (*DIFF CEORET*) as the absolute value of the difference in the mean annual stock returns (relative to the S&P 500 index returns) for the most recent three years for the CEO of the focal firm and for that of its SOP-failed peer. Differently, we measure stock performance for the focal firm (*RET*) and SOP-failed peer (*PR RET*) based on their annual stock returns. Hence, *DIFF CEORET* is not equal to the difference between *RET* and *PR RET*.

between the two groups.

Since differences in CEO-level or firm-level variables can explain the occurrence of SOP-failed CPs and the compensation of focal firms, we control for these variables in our main empirical tests of focal firms' compensation.

Figure 1 illustrates the framework for this analysis. Note that there are two elements of voting failure for compensation peers: the voting failure itself, and the market response to the failure announcement. Both factors may lead to market responses for the focal firm and compensation adjustments. We test whether a focal firm's stock returns in response to such an event have a feedback effect on its board's compensation-reduction decision, beyond the effects from compensation peers' voting results.

3.3 Market Responses to Compensation Peers' Voting Failure

In this section, we show that the response to the news is heterogeneous. First, we find that focal firms are more likely to experience a lower *CAR* when their compensation peers' SOP failure is severe, suggesting that the focal firms' investors observe the severe SOP failure of compensation peers and then convey their dissatisfaction with it. We view a compensation peer's SOP failure as severe if the peer receives a high percentage of negative votes or if a large fraction of peer firms experiences SOP voting failures. We also find that focal firms' *CAR* is lower when the compensation peer in question experiences a lower *CAR* around its SOP voting failure. These results suggest that the spillover effect is stronger when the market views the event as more salient and more negative.

In hypothesis 1, we posit that the immediate market reaction reflects shareholder dissatisfaction with the focal firm's inappropriate benchmarking and the consequently biased executive pay. We test hypothesis 1 with the following OLS regression.

$$CAR_{i,t} = \alpha_0 + \alpha_1 PR\ SEVERITY_{i,t} + \alpha_2 PR\ CAR_{i,t} + Controls. \qquad (1)$$

The dependent variable, *CAR*, captures the immediate market reaction to the peer event. To construct this variable, we first calculate the focal firm's three-day cumulative abnormal market-adjusted returns (*CAR*) from one day before to one day after the SOP-failed peer's annual shareholder meeting. If a focal firm has more than one SOP-failed peer, we first calculate its *CAR* around the announcement date of each peer's SOP failure. Then we use the firm's mean *CAR*, measured over these multiple-event periods as the *CAR* in year *t*.[6] A more negative market reaction would reflect stronger shareholder dissatisfaction with executive pay that is based on a potentially biased compensation benchmark.

The first independent variable of interest, *PR SEVERITY*, measures the extent of the event severity for a focal firm and is determined by two components. The first is the percentage of negative (i.e., dissenting) votes the SOP-failed peer receives (*PR NEGVOTE*). If a firm has more than one SOP-failed peer in one year, we use the mean *PR NEGVOTE*. The second component is the proportion of SOP-failed peers in the focal firm's compensation benchmarking group (*PR FAILPCT*), which is equal to the number of SOP-failed peers (*PR FAILNUM*) divided by the peer group size (*PR TOTNUM*). We then define *PR SEVERITY* as a dummy variable equal to 1 if either *PR NEGVOTE* or *PR FAILPCT* is in the top quintile of the sample distribution, and zero otherwise. The second independent variable, *PR CAR*, indicates the extent of an SOP-failed peer's market reaction to its own voting failure, which is measured by the three-day *CAR* surrounding its annual shareholders' meeting date. Hypothesis 1 predicts a negative coefficient on *PR SEVERITY* and a positive coefficient on *PR CAR* in eq. (1).

[6] For example, firm A has two compensation peers, firm B and firm C, which announce their SOP failures on December 1st and November 1st in year *t*, respectively. We first compute firm A's three-day *CARs* around the two dates separately, and then we use the mean of the two *CARs* as the measure of the market reaction for firm A in year *t*.

We include the following control variables, which could affect focal firms' market reactions to their peers' events. It is plausible that the spillover effect could go through the channels of common institutional investors (*COMINST*), shared directors (*INTERLOCK*), or other industry peers that experience voting failure (*INDPR FAILSOP*). We also control for a set of focal-firm and peer-firm characteristics (e.g., firm size, growth, profitability, and stock performance, etc.). Last, we control for firm and year fixed effects.

Table 2 reports the results for the regression based on eq. (1). In column (1), the regression result shows a negative coefficient on *PR SEVERITY* (the coefficient = -0.004 and t = -2.02) and a positive coefficient on *PR CAR* (coefficient = 0.188 and t = 6.46), consistent with hypothesis 1. The magnitude of market reaction is not negligible, as more severe voting failure decreases a focal firm's three-day *CAR* by 0.40%. Moreover, as SOP-failed peer *CAR* decreases by 1%, focal firm *CAR* decreases by 0.19% accordingly. In column (2), we include two components of *PR SEVERITY* (i.e., *PR NEGVOTE*, which is the percentage of negative votes the SOP-failed peer receives, and *PR FAILPCT*, which is the percentage of SOP-failed peers in the compensation benchmarking group). We find a significantly negative coefficient on *PR NEGVOTE* (coefficient = -0.025 and t = -2.39), suggesting that the focal firms have a more negative *CAR* when peers' SOP voting receives more dissenting (i.e., negative) votes. Instead, the proportion of the focal firm's compensation peers that fail in the voting (*PR FAILPCT*) does not drive the market reaction. The result is consistent with the notion that a focal firm's negative market reaction mostly reflects shareholder dissatisfaction with its pay practices when its compensation peer experiences stronger investor opposition in the SOP voting. Our finding supports the view that investors regard SOP as a value-creating mechanism (Cai and Walkling, 2011; Ferri and Maber, 2013; Cuñat, Giné, and Guadalupe, 2016; Iliev and Vitanova, 2019). The control variables have weak effects on *CAR*,

except for the proxy advisor's recommendation to vote against CEO pay (*ISSAGAINST*).

Overall, the results suggest that the severity of the compensation peers' SOP voting failure and the extent of the peers' own market reactions mainly determine the market responses to the focal firms (around peers' SOP events). The results ensure the validity of using market reactions around the peer event to capture investor dissatisfaction with focal firms' compensation benchmarking. It is notable that the adjusted R^2 is low (13.37%), suggesting that the market reactions may also reflect news other than the severity of the peer event on adjacent dates.

[Table 2]

3.4 Subsequent CEO Pay Reductions Due to the Feedback Effect

We use a difference-in-differences design to test the feedback effect (i.e., focal firms learn from the market and subsequently cut CEO pay), as developed in hypothesis 2:

$$LN\ CEOPAY_{i,t} = \beta_0 + \beta_1 POST_{i,t} * NEGCAR_{\mathrm{i,t-1}} + \beta_2 NEGCAR_{i,t-1} + \beta_3 POST_{i,t} + \beta_4 POST_{i,t} * PR\ SEVERITY_{\mathrm{i,t-1}} + \beta_5 PR\ SEVERITY_{i,t-1} + \beta_6 POST_{i,t} * PR\ NEGCAR_{i,t-1} + \beta_7 PR\ NEGCAR_{i,t-1} + Controls. \quad (2)$$

The dependent variable, *LN CEOPAY,* is the natural logarithm of total focal firm CEO compensation, measured in the most recent year after a compensation peer's SOP failure. We use the dummy variable, *NEGCAR*, as the independent variable to capture a strong versus weak negative market reaction for the focal firms. That is, *NEGCAR* equals 1 if the *CAR* is below the annual sample median, and zero otherwise. We define the post-event year (*POST* = 1) as the first year when a focal firm can adjust CEO pay subsequent to its peer's voting failure. To be the post-event year, we require that the focal firm's fiscal year end is at least six months after its peer's SOP event, because it takes time for the focal firm's directors to adjust their compensation design (Denis, Jochem, and Rajamani, 2020). The pre-event year (*POST* = 0) refers to the year prior to the post-

event year. Such identification of the focal firm's pre- and post-event periods provides a more precise way to capture the board's pay-cut decision in response to the peer's voting failure. In a robustness test reported in section 3.7.3, we use the one-year window as a relatively rough way of identifying the timing of the board's action.[7] If the feedback effect exists, we expect the coefficient on *POST*NEGCAR* to be negative.

We include *PR SEVERITY* (the severity of the peer event), which captures the spillover effect through the learning-from-peers and compensation-targeting channels (Denis, Jochem, and Rajamani, 2020), as well as its interaction term with *POST*. If these channels coexist with the learning-from-prices channel in our study, the coefficient on *POST*PR SEVERITY* would be negative. In addition, we also include *PR NEGCAR* (a dummy variable that equals 1 if SOP-failed peers' *CAR* is negative, and zero otherwise) and its interaction with *POST* to test the possibility that the focal firm's board also learns from its SOP-failed peer's market reaction.

We control for a comprehensive set of variables that could affect focal firms' CEO pay. First, it is plausible that common institutional investors, shared directors, or other industry peers that experience voting failure may affect CEO pay. We therefore control for common institutional ownership (*COMINST*, which equals 1 if the percentage of the focal firm and the SOP-failed peer's shares owned by a common institutional investor is above the annual sample median, and zero otherwise), board interlock (*INTERLOCK*, which equals 1 if a focal firm director also serves on the board of the SOP-failed peer, and zero otherwise), and an industry-peer effect (*INDPR*

[7] The choice of six months is motivated by Denis, Jochem, and Rajamani (2020), who write that "Compensation committees usually obtain information about peers' pay levels and say on pay votes approximately two quarters prior to the end of their fiscal year." There are two scenarios based on the time distance between the date of a peer's SOP voting failure and a focal firm's fiscal year end. As an example, assume the focal firm's fiscal year ends in December. In the first scenario, the peer's SOP voting failure occurs in April 2015 and its distance to the next fiscal year end of the focal firm (December 2015) is eight months. Under such a circumstance, the focal firm's board would have sufficient time to take the peer event into consideration when making CEO pay decisions in the 2015 fiscal year. As such, we refer to 2015 as the post-event year and 2014 as the pre-event period. In the second scenario, the peer's voting failure is in October 2015, and its distance to the next fiscal year end of the focal firm (December 2015) is only two months. It is unlikely that the focal firm' directors could respond to the peer event when determining CEO pay for the fiscal year ending in December 2015. Hence, we view 2016 as the post-event year and 2015 as the pre-event year.

FAILSOP, which equals 1 if the focal firm has an SOP-failed industry peer that is not the compensation peer, and zero otherwise).

Second, following prior studies (e.g., Core, Guay and Larcker, 2008), we control for focal firm characteristics and CEO characteristics, including the focal firm's lagged CEO pay (lagged *LN CEOPAY*), its CEO pay relative to its compensation peers (*CEOPAY ABOVE*, which equals 1 if the focal firm's CEO pay is above the median for the compensation peer group, and zero otherwise), median peer pay (*PR MEDPAY*, the median of the natural logarithm of compensation peers' CEO pay), firm size (*SIZE*, the natural logarithm of the market value of equity), growth opportunity (*BTM*, the book-to-market ratio), leverage (*LEV*, the leverage ratio), stock performance (*RET*, the annual market-adjusted stock returns), accounting profitability (*ROA*, the industry-adjusted return on assets), sales (*SALE*, the natural logarithm of sales revenue), CEO tenure (*LN TENURE*, the natural logarithm of the number of years the CEO has served in that role), CEO ownership (*CEOOWN*, the percentage of shares the CEO owns), and institutional ownership (*INST*, the percentage of shares institutional investors own).

Third, we further control for the focal firm's own SOP voting outcome (*SOPVOTE*, the percentage of favorable votes the focal firm receives), the proxy advisor's voting recommendation (*ISSAGAINST*, which equals 1 if ISS recommends voting against the focal firm, and zero otherwise), and SOP voting frequency (*SOPFREQ*, which equals 1 if the focal firm votes on SOP frequently, and zero otherwise).

Fourth, we control for the key independent variables in Denis, Jochem, and Rajamani (2020), who suggest that peer firm characteristics and labor-market conditions inform the pay decisions a focal firm's board makes in response to its peer's SOP voting failure. Hence, we further control for the peer's CEO pay (*PR CEOPAY*, the natural logarithm of the SOP-failed peer's total CEO

compensation), the peer's CEO pay relative to other compensation peers (*PR PAYABOVE*, which equals 1 if the SOP-failed peer's CEO pay is above the median for the compensation peer group, and zero otherwise), and the peer's performance (*PR RET*, the annual stock returns of the SOP-failed peer; and *PR ROA*, the return on assets of the SOP-failed peer). We also control for labor market commonality and mobility, including: *TALENTFLOW*, which equals 1 if at least one of the top five executives moves between the focal firm and the industry of the SOP-failed peer in the most recent five years, and zero otherwise; *DIFF CEORET*, the difference in CEO capability, measured by the mean market-adjusted annual stock returns for the most recent three years of the CEO's tenure, between the focal firm and its SOP-failed peers; *TURNOVER*, which equals 1 if the focal firm experiences CEO turnover, and zero otherwise; and *RETIRE*, which equals 1 if the focal firm's CEO is at least 65 years old, and zero otherwise (Jenter and Kanaan, 2015; Cai, Xu, and Yang, 2021). Last, we control for firm and year-fixed effects.

Table 3 reports the regression results for testing the feedback effect on the board's pay-cut decision, as developed in hypothesis 2. We first conduct the regressions of *LN CEOPAY* (the natural logarithm of the focal firm's total CEO pay) on *NEGCAR* (the dummy variable for a strong versus weak negative market reaction to the peer event), as specified in eq. (2), without including other learning channels (i.e., the event severity and the SOP-failed peer's market reaction). In column (1), the coefficients on the interaction term *POST*NEGCAR* are significantly negative (coefficient = -0.134 and t = -3.70). The results show that focal firms experiencing a negative market reaction to their peers' voting failure reduce CEO pay by more than 12.54% ($1 - e^{-0.134}$) in the subsequent year relative to those with no adverse market-spillover effect. Given the sample mean of CEO pay is \$8.62 million, the economic magnitude of our effect is around \$1.08 million (12.54% * \$8.62 million). This result is consistent with hypothesis 2, which suggests that focal firms learn from the

market to adjust potentially biased CEO compensation downward.[8]

[Table 3]

We then analyze whether the board learns from other channels, including the severity of the peer event (*PR SEVERITY*) and the peer's negative market reaction (*PR NEGCAR*). In column (2), we control for *PR SEVERITY* and its interaction with the post-event dummy, which captures the effect through the channel in Denis et al. (2020), without considering focal firms' market reactions surrounding the event (*NEGCAR*). The coefficient on the interaction term *POST*PR SEVERITY* is negative and significant (coefficient = -0.089 and t = -2.25). The results corroborate Denis, Jochem, and Rajamani (2020), who find that focal firms with SOP voting failures subsequently cut CEO pay. For the peer's negative *CAR*, the insignificant coefficient on *POST*PR NEGCAR* suggests that the focal firm's board does not learn from the market reaction of an SOP-failed peer when deciding to adjust CEO pay.

In column (3), we include the market-spillover variable *NEGCAR*, together with proxies for other learning channels in the regression. The coefficient on *POST*NEGCAR* continues to be significantly negative (-0.130, t-value is -3.55), and the coefficient on *POST*PR SEVERITY* also remains significant (-0.083, t-value is -2.09).[9] The results suggest that focal firms' *CAR* (e.g., *NEGCAR*) contains information beyond peers' SOP voting outcome and that it plays a more important role in the board's compensation policymaking than does peer voting outcome (e.g., *PR SEVERITY*). In other words, the board learns more from its own firm's prices than it does from peer voting failures. These results indicate that, more than any other learning channels, the

[8] In untabulated analyses, we find that the board tends to cut equity-based pay (stocks and options) more than cash pay (salary, bonus, and other nonequity incentives). Specifically, the regressions of total CEO pay (in dollars) and of pay components (in dollars) show that focal firms experiencing a strong negative *CAR* cut total pay by $0.88 million, $0.54 million of which is a reduction in equity-based pay and $0.34 million of which is a reduction in cash pay.

[9] In an untabulated robustness test, we also use the raw values of negative *CAR*. The coefficient on the interaction between *POST* and negative *CAR* remains negative and marginally significant. This result suggests that the board is sophisticated in differentiating the extent of negative market responses to a peer's voting failure when deciding on the magnitude of subsequent CEO pay reduction.

feedback effect has an incremental impact on CEO pay reduction at focal firms. The overall findings thus support hypothesis 2.

Our study shows that peers' SOP failures immediately affect focal firm stock prices. The spillover effect takes place before firms reduce CEO compensation, and it offers a potential explanation for why focal firms adjust CEO compensation. We add to this literature by offering a new explanation for the spillover effect that complements Denis et al. (2020). We differentiate between the sources of information (i.e., between learning from prices and learning from peers' voting outcomes). We also offer direct evidence that directors benefit from the learning (i.e., higher investor support in future elections). Our study sheds light on the black box of directors' decision-making processes.

With respect to control variables, CEO compensation is higher for larger firms (*SIZE*), those with better stock performance (*RET*), and CEOs with longer tenure (*LN TENURE*). Moreover, CEOs at retirement age (*RETIRE*) or who switch jobs between focal firms and SOP-failed peers (*TALENTFLOW*) receive lower compensation. The results also show a positive coefficient of lagged *SOPVOTE* and a negative coefficient of lagged *ISSAGAINST*, although not statistically significant. The signs of the coefficients on lagged *SOPVOTE* and lagged *ISSAGAINST* imply that focal firms with weaker shareholder support in prior SOP voting or that receive a negative recommendation for SOP proposals from the proxy advisor in the previous year have lower CEO pay in the next year.[10]

[10] The adjusted R^2 in our CEO pay regression is approximately 88%, which is higher than the typical adjusted R^2 in the literature (e.g., around 70% using firm and year fixed effects). One possible reason for this higher value is that our sample consists of firms covered by Incentive Lab (S&P 500 firms and the majority of S&P 400 firms) with SOP-failed peers, rather than the broader universe of ExecuComp typically used in prior studies. Another reason could be that our CEO pay regression includes not only the typical pay determinants, but also a set of variables related to SOP-failed events and compensation peers (e.g., CARs of focal firms and SOP-failed peers, severity of voting failure, and SOP-failed peers' CEO pay and performance). To compare with the typical adjusted R^2, we perform the CEO pay regressions for our sample firms, including only the typical pay determinants (i.e., firm size, market-to-book ratio, leverage, performance, CEO tenure and share ownership, and institutional ownership). In the unreported analysis with fewer control variables, the adjusted R^2 is 78.56% for the regression with firm and year fixed effects, which are similar to prior studies.

3.5 Cross-Sectional Analyses

In this section, we investigate how the feedback effect varies with CEOs' relative power on the board (proxied by CEO-chair duality, CEOs who are not newly appointed, directors hired after a CEO takes office, and busy boards), ISS's voting recommendation, and compensation consultants. Table 4 presents the regression results for these cross-sectional tests as in hypothesis 3.

3.5.1 Effect of Director Power Relative to CEOs

Bebchuk and Fried (2003) argue that CEOs could intervene in board compensation decision-making to benefit themselves via excess compensation. When the CEO has sufficient power to exert such influence, it is difficult for the board to adjust CEO compensation downward in response to a peer's voting failure. The feedback effect is thus weaker when the CEO is stronger (which is equivalent to a weaker board of directors).

Prior studies (e.g., Fich and Shivdasani, 2006; Morse, Nanda, and Seru, 2011; Abernethy, Kuang, and Qin, 2015) suggest that CEOs are more powerful when they also chair the board, when the board has more directors appointed after the CEO's tenure begins, and when directors are busier. Hence, to capture the CEO's relative power on the board, we consider four proxies: CEO-chair duality (*DUALITY*), CEOs beyond the first year of appointment (*INCUMCEO*), the percentage of directors hired after the CEO takes office (*COOPTDIR*) (Coles, Daniel, and Naveen, 2014), and the percentage of independent directors serving on three or more outside boards (*BUSYDIR*) (Fich and Shivdasani, 2006). The descriptive statistics for the proxies are in panel B of table 1. In 52.3% of focal firms, the CEO also chairs the board, 93.8% of firms do not have newly appointed CEOs, and an average of 41.0% of directors are hired after the CEO takes office. The mean percentage of busy directors is 9.6%. The composite index of CEO power (*CEOPOWER*) is the factor scores from the factor analysis on all proxies.

Column (1) of table 4 reports the regression with the CEO power variable included. The interaction term *POST*NEGCAR*CEOPOWER* has a significantly positive coefficient (coefficient = 0.072 and $t = 2.77$).[11] In economic terms, the magnitude of the CEO pay reduction (driven by the negative market-spillover effect) in focal firms with strong CEO power is 7.5% ($e^{0.072} - 1$) less than those with weak CEO power. This finding is consistent with the notion that the feedback effect is weaker when the CEO is more powerful (relative to the directors) and is consequently more capable of influencing board decisions. Alternatively, the feedback effect is stronger when directors are more powerful (relative to the CEO).

This result also allows us to infer CEOs' potential learning from prices. Previous research shows that CEOs learn from prices and use this knowledge when making corporate decisions. It also shows that CEOs influence board compensation decisions, suggesting that CEOs may agree with boards regarding compensation reduction after learning of shareholder dissatisfaction through price responses. In this case, we may find no differential effect if the board is more powerful. However, we find that the feedback effect is stronger when boards have greater bargaining power over the CEO, suggesting that the board's learning from stock prices is the driver of changes in compensation policy.

3.5.2 Effect of an ISS Voting Recommendation

We examine the role of the proxy advisor, whose voting recommendation greatly affects firm compensation decisions (Ertimur, Ferri, and Oesch, 2013). Firms under more pressure from the proxy advisor's negative voting recommendation have stronger incentives to learn from negative spillover effects in the market and to adjust potentially biased CEO pay to an appropriate level. As reported in panel B of table 1, the ISS recommends negative SOP votes against 9.2% of focal firms

[11] Untabulated analyses show no strong significant results when we include the individual proxies of *CEOPOWER* in the regressions separately, suggesting that only the aggregate effect of CEO power matters.

in the sample.

In column (2) of table 4, the regression includes the dummy variable *ISSAGAINST* and its interaction terms with *NEGCAR* and *POST*. The coefficient on the triple interaction term, *POST*NEGCAR*ISSAGAINST,* is negative and marginally significant (coefficient = -0.236 and *t* = -1.68). The result suggests that, when facing a negative market reaction to the peer event, focal firms with adverse ISS voting recommendations reduce their CEO pay by 21.0% ($1 - e^{-0.236}$) more than those not under such pressure. Therefore, the external threat of the proxy advisor's unfavorable voting recommendation strengthens the feedback effect.

3.5.3 Role of Compensation Consultants

In the process of setting executive pay, firms typically hire compensation consultants to help the compensation committee design compensation plans. We expect that the quality of the compensation consultant could play a role in the learning effect. Specifically, firms that hire low-quality compensation consultants are more likely to set inappropriate CEO pay and thus learn more from market responses to peers' voting failure.

Based on the number of clients served in our sample, we consider as high quality the following large compensation consultants: FW Cook, Willis Towers Watson, Pearl Meyer, Aon Hewitt, Pay Governance, Radford Consulting, and Semler Brossy Consulting; we consider any other consultants as low quality. In panel B of table 1, the descriptive statistics for the dummy variable *CONSULT SMALL* show that 52.9% of sample firms hire a small consultant (*CONSULT SMALL* = 1) and the remaining 47.1% hire a large consultant (*CONSULT SMALL* = 0).

As reported in column (3) of table 4, we include *CONSULT SMALL* in the regression.[12] The

[12] Selecting a small compensation consulting firm is not random, as it may be correlated with other firm and governance characteristics. Hence, in this test of the impact of compensation consultants, we include additional controls for focal firm size and information opacity measured by industry R&D share (the percentage of the firm's R&D expenses in the industry), industry sales growth (the percentage change in industry-level sales), and asset intangibility (the ratio of intangible assets to total assets) (Naaraayanana and Nielsen, 2021), as well as accrual quality based on modified Dechow and Dichev (2002) model (Billett and Yu,

coefficient on *POST*NEGCAR*CONSULT SMALL* is negative and significant (coefficient = -0.144 and t = -1.98). The relative magnitude of the pay reduction due to negative market reactions in the focal firms with low- versus high-quality consultants is 13.4% ($1 - e^{-0.144}$), which is economically significant. Consistent with prior expectations, the result suggests a stronger learning effect when the compensation consultants are of lower quality. Overall, our results support hypothesis 3.

[Table 4]

3.6 Placebo Tests

We conduct two placebo tests to address the concern that the market feedback effect on focal firms' pay reduction could be simply statistical significance obtained by chance. First, the main findings could be related to news unrelated to the peer event in adjacent periods. In view of this, we artificially choose a set of nonevent dates (i.e., 10 to 50 days before and after the actual event dates) and compute the three-day *CAR* surrounding these pseudo dates. Similarly, we define the dummy variable *NEGCAR PSEUDO* as 1 if the pseudo-*CAR* is below the annual sample median (i.e., a more negative *CAR*), and zero otherwise. We then run the regression by replacing *NEGCAR* (based on the actual event dates) with *NEGCAR PSEUDO* (based on the pseudo-event dates). Panel A of table 5 shows that none of the coefficients on the interaction term *POST*NEGCAR PSEUDO* is significant in any of the tests based on the pseudo-events.

Second, we identify 182 focal firms-year without SOP-failed compensation peers but with SOP-failed industry peers that are not compensation peers (excluding focal firm-year observations

2016; Xu and Yang, 2016). We also consider that financially constrained firms may tend to hire small compensation consultants because of limited financial resources. As such, we further control for the composite index of financial constraints (*FC*) based on the factor scores from the factor analysis on the modified KZ index (Kaplan and Zingales, 1997; Baker, Stein, and Wurgler, 2003) and the WW index (Whited and Wu, 2006). As a further check, we add these additional control variables in the analyses on the role of CEO power in column (1) and the role of the proxy advisor's voting recommendation in column (2). The results remain unchanged.

if any of its compensation peers has an SOP failure in the year). We use one-digit SIC to define industries (similar results if we rely on a reduced sample using two-digit or three-digit SIC). There are 364 focal firm-year observations in the regression after retaining pre- and post-event observations around each identified focal firm-year. As reported in panel B of table 5, the insignificant coefficient on *POST* NEGCAR PSEUDO* suggests no evidence of pay reduction for focal firms following the industry peers' voting failure. The result indicates that focal firms' directors do not respond by cutting CEO pay for these pseudo-events related to industry peers.[13]

Overall, the results from the placebo tests indicate that boards of focal firms do not respond by cutting CEO pay in the above pseudo-events, lending credibility to the evidence of the feedback effect documented in the main analyses.

[Table 5]

3.7 Robustness Tests

We conduct a set of tests to ensure the robustness of the main results, including: 1) an alternative sample consisting of event firms and propensity-score-matched (PSM) nonevent firms; 2) another alternative sample excluding event firms whose shareholders' meeting dates overlap with SOP-failed peers' meeting dates; 3) how focal firms' market reactions around peers' SOP events affect excessive CEO compensation; 4) an alternative window of market reactions from two days before to two days after the peer event; 5) the aggregate instead of the average *CAR* over multiple peer events during a year; 6) the residual *CAR* estimated from eq. (1); 7) an alternative measure of *CAR* based on returns on failed SOP dates minus returns on successful SOP dates; 8) another alternative measure of *CAR* based on returns on failed SOP dates minus returns on other dates in a year; and

[13] We thank the referee for suggesting the placebo tests. In the internet appendix, we perform a third placebo test using non-SOP-failed pseudo-events of compensation peers. In particular, pseudo-events in this test refer to extremely negative daily returns for compensation peers (i.e., a price decrease of more than 10% in one day), and we find similar insignificant results.

9) an alternative timeline for determining pre- and post-event years. The results of these robustness tests are reported in table 6.

[Table 6]

3.7.1 Alternative Sample Construction and Dependent Variable

In this robustness test, we match each event firm experiencing a negative market reaction (i.e., *NEGCAR* = 1) with a nonevent firm based on the propensity score with respect to industry membership, firm size, performance, and peer-group size. We then expand the primary sample of event firms to include these PSM nonevent firms whose *NEGCAR* is set to be zero. In table 6, column (1) of panel A shows the regression result based on the alternative sample. The coefficient on *POST*NEGCAR* remains negative and significant (coefficient = -0.108 and t = -2.85), consistent with the main finding.

The confounding effect may arise from concurrent events when shareholders' meeting dates for focal firms coincide with those of the peers with failed SOP voting. We exclude 99 focal firms (198 observations in the pre- and post-event periods) that hold their annual shareholders' meetings during the three-day (-1,+1) window of SOP-failed peers' voting event. Based on this alternative sample, column (2) of panel A in table 6 shows that the coefficient on *POST*NEGCAR* is significantly negative (coefficient = -0.134 and t = -3.44).

We use focal firms' total CEO pay as the dependent variable in the main tests. It is likely that the focal firm reduces excessive CEO pay after learning from the negative market reaction to the peer event. To test this possibility, we follow Core, Guay, and Larcker (2008) to estimate excessive pay based on the residuals from the annual regressions of the natural logarithm of total CEO pay on the natural logarithm of one-year lagged total sales, the one-year lagged market-to-book ratio, the current- and one-year lagged return on assets, the current- and one-year lagged annual stock

returns, CEO tenure, and the S&P500 index constituents. In the regression of excessive CEO pay, shown in column (3) of panel A of table 6, the interaction term *POST*NEGACAR* has a significantly negative coefficient (coefficient = -0.120 and t = -3.28). Hence, the main result still holds.

3.7.2 Alternative Measurement of Independent Variable

The independent variable *NEGCAR* used in the main tests is based on the three-day market reaction from one day before to one day after the peer event (i.e., *CAR[-1, +1]*). As a sensitivity test, we extend the window to include the five days surrounding the event (i.e., from two days before to two days after the peer's voting failure) (i.e., *CAR[-2, +2]*). Column (1) of panel B of table 6 reports the regression using the alternative *NEGCAR* as the independent variable. As with the main results, the coefficient on *POST*NEGCAR* continues to be significantly negative (coefficient = -0.082 and t = -2.25).[14] Again, the main finding is unchanged.

In the main tests, we use the mean *CAR* when a focal firm has more than one SOP-failed peer during a year. It is possible, however, that the board evaluates the market reactions to multiple peer events in aggregate instead of the average market spillover effect when making the decision to cut CEO pay. As an alternative measure, we sum the three-day *CAR* surrounding each peer event a focal firm experiences during a year and then redefine the dummy variable *NEGCAR* based on the aggregate *CAR* (i.e., *NEGCAR* equals 1 if the aggregate *CAR* is below the annual sample median, and zero otherwise). As reported in column (2) of panel B, the coefficient on the interaction term between *POST* and the redefined *NEGCAR* is negative and significant (coefficient = -0.143 and t = -0.393). Hence, the feedback effect found in the main result is robust to this alternative method

[14] Untabulated analyses show that the result weakens as the market-reaction window lengthens. While the coefficient on *POST*NEGCAR* remains significantly negative for *CAR[-3, +3]* and *CAR[-4, +4]*, the coefficient becomes insignificant, although still negative (with a smaller magnitude), for *CAR[-5, +5]*. This is possibly because the *CAR* with a longer window captures more noise in the market apart from the spillover effect related to the peer event.

of constructing *CAR*.

To explore whether the board learns from this additional news, we estimate the residual *CAR* by regressing *NEGCAR* on the independent variables as specified in eq. (1). Column (3) of panel B of table 6 shows that the coefficient on the interaction between *POST* and *NEGCAR* based on the residual *CAR* is significantly negative (coefficient = -0.089 and t = -2.45). This result further supports the feedback effect in which the board learns from market prices that incorporate all relevant information for compensation decision-making.

In this subsection, we alternatively define a focal firm's *CAR* as the differences in price reactions to its failing peers' failed SOP events and its nonfailing peers' successful SOP events. That is, the alternative *CAR* is equal to the three-day stock returns surrounding dates of the peers' failed vote (averaged across SOP-failed peers) minus the three-day stock returns surrounding the peers' successful vote (averaged across non-SOP-failed peers). As shown in column (4) of panel B of table 6, the coefficient on the interaction term, *POST* and *NEGCAR*, is still negative and significant (coefficient = -0.104 and t = -2.87), corroborating with the main result.

In a further robustness test using another alternative *CAR* measure, we compare the immediate market reaction to the SOP failure with the stock returns on other dates in a year. As such, the alternative *CAR* is the three-day stock returns surrounding the peers' SOP failure (summed across SOP-failed peers) minus the stock returns on all other dates (excluding the event dates). Column (5) of panel B shows a significantly negative coefficient on the interaction between *POST* and *NEGCAR* (coefficient = -0.081 and t = -1.96). Hence, the main result is robustness using this alternative measure of abnormal market reaction.

3.7.3 Alternative Timeline for Determining Pre- and Post-Event Years

As discussed in Denis, Jochem, and Rajamani (2020), boards typically obtain peers' pay and SOP

voting information six months prior to the fiscal year end. Accordingly, in the main tests, we use the refined six-month window for defining *POST* (i.e., the post-event year is the first fiscal year ending at least six months after the peer event) to better capture the timing of board pay decisions in response to peer voting failures. As a robustness check, we relax the six-month window to a one-year window when defining *POST*. Specifically, we first identify the event year (i.e., the year of the peer event) as the focal firm's fiscal year, which ends fewer than 360 days after the date of the peer's voting failure. We then define *POST* to equal to 1 (zero) for the fiscal year after (before) the event year.

Panel C of table 6 reports the results using this alternative timeline for replicating the main analyses on the feedback effect in table 5. The coefficients on *POST*NEGCAR* are significant and marginally significant (e.g., coefficient = -0.090 and t = -1.80 in column (3)). Hence, the results still support the feedback effect, although it is weaker, possibly because the one-year window is a less precise way to capture the timing of the board's action to cut CEO pay due to learning from the market. In this sense, the results from this alternative definition of *POST* highlight the importance of using the properly identified six-month window in the main tests to document the existence of a feedback effect arising from the peer event.[15]

4. Impact of Board CEO Pay Decisions on Future Director Elections

In this subsection, we analyze whether directors gain from building a reputation for being shareholder-friendly (by cutting CEO pay in response to negative shareholder views conveyed in price drops) as predicted in hypothesis 4. We explore shareholder support in the election of

[15] In unreported tests in the internet appendix, we also find the main results robust to (1) controlling for compensation targeting, which is a phenomenon mentioned in Denis, Jochem, and Rajamani (2020); (2) controlling for unobservable negative shock to both SOP-failed peers and focal firms in the CEO pay regression to alleviate the concern that such shock could confound the result; (3) controlling for SOP-failed peers' CEO pay relative to the median pay of the compensation peer group and its interaction with the POST indicator to mitigate the concern that SOP-failed peers with excess CEO pay drive the main result.

incumbent directors on the board. Aggarwal, Sandeep, and Prabhala (2019) find that dissent shareholder votes have power and result in negative consequences for directors, i.e., they either depart boards or move to less prominent board positions (also see Liu, Low, Masulis, and Zhang (2020) for investors' voting on director elections). Please refer to Yermack (2010) for a review of shareholder voting literature.

If the board of directors learns from negative market reactions and cuts CEO pay, such actions could ease shareholder dissatisfaction and thus reduce votes against directors in their subsequent reelection. To test the impact on director elections, we employ the following regression at the individual director level in a difference-in-differences analysis. We focus on the subsample of focal firms that experience a strong negative *CAR* around peer events (*NEGCAR* = 1).

$$DIR\ VOTE_{i,j,t} = \gamma_0 + \gamma_1 POST_t * CEOPAY\ CUT_i + \gamma_2 CEOPAY\ CUT_i + \gamma_3 POST_t + Controls. \quad (3)$$

The dependent variable, $DIR\ VOTE_{i,j,t}$, is the percentage of favorable votes director *j* receives in the board election during the annual shareholders meeting of focal firm *i* in fiscal year *t*. The independent variable, $CEOPAY\ CUT_i$, equals 1 if the change in CEO pay of firm *i* from the pre- to the post-event year (*CHG CEOPAY*) is below the sample median (i.e., a larger pay cut), and zero otherwise. $POST_t$ equals 1 if the observation is in the year following the CEO pay change and zero if the observation is before the CEO pay change. For each focal firm that experiences a negative *CAR* around its peers' SOP event, we keep all director elections within two years around the SOP event. For example, if one of firm *i*'s peer firms experiences an SOP voting failure in 2017, we measure $CEOPAY\ CUT_i$ based on the difference in CEO pay in 2018 and 2017 and let $POST_t$ equal 1 (zero) for director elections in fiscal year 2018 (2017).

The independent variable of interest is the interaction term between *POST* and *CEOPAY CUT*.

We expect its coefficient to be positive. We control for director and firm characteristics that could affect director elections (Ertimur, Ferri, and Stubben, 2010). The director-level control variables include director age, which is either between 65 and 69 years old (*DIR AGE65-69*) or older than 70 (*DIR AGE70*); director tenure (*DIR TENURE*); outside directorships (*DIR OUTBOARD*); an affiliated (grey) outside director who is employed with the company or has material interests in transactions with the focal firm or family relationships (*DIR GREY*); a female director (*DIR FEMALE*); a director serving as a board committee member (*DIR COMPCOM, DIR AUDITCOM, DIR NOMCOM,* and *DIR CGCOM* for the compensation, audit, nominating, and corporate-governance committees, respectively) or as chair of any of these committees (*DIR COMPCHAIR*); director ownership (*DIROWN*); and directors up for election (*DIR ELECTION*). The controls at the focal firm include firm size (*SIZE*), book-to-market ratio (*BTM*), leverage (*LEV*), stock performance (*RET*), and CEO turnover (*TURNOVER*). We also include firm and year-fixed effects in the regression.

Table 7 presents the results of the regression as specified in eq. (3). In column (1), the coefficient on *POST*CEOPAY CUT* is significantly positive, consistent with hypothesis 4. The economic magnitude is also significant: Directors at focal firms that cut CEO pay in the prior year receive 1.5% more votes from outstanding shares supporting their election than do directors at other focal firms (this magnitude is equal to 11.8% of one standard deviation of the dependent variable *DIR VOTE*). In an unreported analysis, the standard deviation of *DIR VOTE* is 0.127. This result suggests that the board's decision to cut CEO pay reduces pressure from shareholders to vote against directors in subsequent elections.

[Table 7]

The compensation committee typically proposes CEO pay, which is then subject to approval

by the entire board of directors. When the board does not cut CEO pay following a peer SOP-vote-failure event, shareholder opinion of compensation committee members may become more negative relative to other directors in the proxy voting. In this case, the consequence for reelection may be more salient for directors on the compensation committee. We estimate the regression separately for compensation committee members (column (2)) versus other committee members (column (3)). Although the coefficients on *POST*CEOPAY CUT* are positive and statistically significant in both columns, the magnitude of the coefficient tends to be considerably greater (although the difference is not statistically significant) in the regression for the subsample of compensation committee members (coefficient = 0.018 in column (2) versus coefficient = 0.012 in column (3)). The results provide further evidence consistent with the notion of stronger shareholder dissatisfaction with directors who are closely involved in CEO pay decision-making.

5. Conclusion

Our study highlights the importance of market feedback in the context of SOP voting. We find that firms learn from negative market reactions following their peers' SOP voting failures and subsequently adjust their own CEO pay. This learning effect is stronger when CEOs have less power over board decision-making processes, when external pressure from proxy advisors such as ISS is stronger, and when compensation consultants are of lower quality.

Furthermore, we show that directors who learn from prices and cut excess CEO pay are perceived as more capable by investors, resulting in higher support in future director elections. These findings have important implications for both firms and regulators in the evaluation of the effectiveness of the SOP voting rule.

References

Abernethy, M., Y. Kuang, and B. Qin. 2015. Influence of CEO Power on Compensation Contract Design. *The Accounting Review* 90(4): 1265–1306.

Aggarwal, R., D. Sandeep, and N. R. Prabhala. 2019. The Power of Shareholder Votes: Evidence from Uncontested Director Elections. *Journal of Financial Economics* 133(1): 134–153.

Albuquerque, A., G. DeFranco, and R. Verdi. 2013. Peer Choice in CEO Compensation. *Journal of Financial Economics* 108(1): 160–181.

Baker, M., X. Pan, and J. Wurgler. 2012. The Effect of Reference Point Prices on Mergers and Acquisitions. *Journal of Financial Economics* 106(1): 49–71.

Baker, M., J. C. Stein, and J. Wurgler. 2003. When Does the Market Matter Stock Prices and the Investment of Equity-Dependent Firms. *Quarterly Journal of Economics* 118(3): 969–1005.

Bebchuk, L. A., and J. M. Fried. 2003. Executive Compensation as an Agency Problem. *Journal of Economic Perspectives* 17(3): 71–92.

Becker, B., D. Bergstresser, and G. Subramanian. 2013. Does Shareholder Proxy Access Improve Firm Value? Evidence from the Business Roundtable Challenge. *Journal of Law and Economics* 56(1): 127–160.

Billett, M., and M. Yu. 2016. Asymmetric Information, Financial Reporting, and Open-market Share Repurchases. *Journal of Financial and Quantitative Analysis* 51(4): 1165–1192.

Bird, A., S. A. Karolyi, T. G. Ruchti, and P. Truong. 2021. More Is Less: Publicizing Information and Market Feedback. *Review of Finance* 25(3): 745–775.

Bizjak, J., S. Kalpathy, Z. Li, and B. Young. 2022. The Choice of Peers for Relative Performance Evaluation in Executive Compensation. *Review of Finance* 26(5): 1217–1239.

Bizjak, J., M. Lemmon, and T. Nguyen. 2011. Are All CEOs above Average? An Empirical Analysis of Compensation Peer Groups and Pay Design. *Journal of Financial Economics* 100(3): 538–555.

Bond, P., A. Edmans, and I. Goldstein. 2012. The Real Effects of Financial Markets. *Annual Review of Financial Economics* 4: 339–60.

Cai, J., J. Garner, and R. Walkling. 2009. Electing Directors. *Journal of Finance* 64(5): 2387–2419.

Cai, J., and R. Walkling. 2011. Shareholders' Say On Pay: Does It Create Value? *Journal of Financial and Quantitative Analysis* 46(2): 299–339.

Cai, Y., J. Xu, and J. Yang. 2021. Paying by Donating: Corporate Donations Affiliated with Independent Directors. *Review of Financial Studies* 34(2): 618–660.

Campello, M., and J. R. Graham. 2013. Do Stock Prices Influence Corporate Decisions? Evidence from the Technology Bubble. *Journal of Financial Economics* 107(1): 89–110.

Chen, Q., I. Goldstein, and W. Jiang. 2007. Price Informativeness and Investment Sensitivity to Stock Price. *Review of Financial Studies* 20(3): 1409–1445.

Coles, J., N. Daniel, and L. Naveen. 2014. Co-opted Boards. *Review of Financial Studies* 27(6): 1751–96.

Coles, J. L., and C. K. Hoi. 2003. New Evidence on the Market for Directors: Board Membership and Pennsylvania Senate Bill 1310. *Journal of Finance* 58(1): 197–230.

Core, J. E., W. Guay, and D. F. Larcker. 2008. The Power of the Pen and Executive Compensation. *Journal of Financial Economics* 88(1): 1–25.

Cuñat, V., M. Giné, and M. Guadalupe. 2016. Say Pays! Shareholder Voice and Firm Performance. *Review of Finance* 20(5): 1799–1834.

Daniel, N. D., Y. Li, and L. Naveen. 2020. Symmetry in Pay for Luck. *Review of Financial Studies*

33(7): 3174–3204.
Dechow, P., and I. Dichev. 2002. The Quality of Accruals and Earnings: The Role of Accrual Estimation Errors. *The Accounting Review* 77(Supplement): 35–59.
Denis, D. K., T. Jochem, and A. Rajamani. 2020. Shareholder Governance and CEO Compensation: The Peer Effects of Say on Pay. *Review of Financial Studies* 33(7): 3130–3173.
Dittmar, A., R. Duchin, and S. Zhang. 2020. The Timing and Consequences of Seasoned Equity Offerings: A Regression Discontinuity Approach. *Journal of Financial Economics* 138(1): 254–276.
Duchin, R., J. G. Matsusaka. and O. Ozbas. 2010. When Are Outside Directors Effective? *Journal of Financial Economics* 96(2): 195–214.
Edmans, A., I. Goldstein, and W. Jiang. 2012. Real Effects on Financial Markets: The Impact of Prices on Takeovers. *Journal of Finance* 67(3): 933–971.
Edmans, A., T. Gosling, and D. Jenter. 2023. CEO Compensation: Evidence from the Field. *Journal of Financial Economics* 150(3): 103718.
Edmans, A., X. Grabaix, and D. Jenter. 2017. Executive Compensation: A Survey of Theory and Evidence. *The Handbook of the Economics of Corporate Governance* (Edited by Benjamin E. Hermalin and Michael S. Weisbach): 383–539.
Edmans, A., S. Jayaraman, and J. Schneemeier. 2017. The Source of Information in Prices and Investment-Price Sensitivity. *Journal of Financial Economics* 126(1): 74–96.
Equilar. 2016. Report on Peer Group Composition and Benchmarking. Equilar, Inc.: Redwood City, CA.
Ertimur, Y., F. Ferri, and D. Oesch. 2013. Shareholder Votes and Proxy Advisors: Evidence from Say on Pay. *Journal of Accounting Research* 51(5): 951–996.
Ertimur Y., F. Ferri, and S. Stubben. 2010. Board of Directors' Responsiveness to Shareholders: Evidence from Shareholder Proposals. *Journal of Corporate Finance* 16(1): 53–72.
Faulkender, M., and J. Yang, 2010. Inside the Black Box: The Role and Composition of Compensation Peer Groups. *Journal of Financial Economics* 96(2): 257–270.
Ferreir, D., M. Ferreira, and C. Raposo. 2011. Board Structure and Price Informativeness. *Journal of Financial Economics* 99(3): 523–545.
Ferri, F. 2012. "Low-Cost" Shareholder Activism: A Review of the Evidence. *Research Handbook on the Economics of Corporate Law*. Edited by C. A. Hill and B. H. McDonnell. Edward Elgar Publishing Limited.
Ferri, F., and D. A. Maber. 2013. Say on Pay Votes and CEO Compensation: Evidence from the UK. *Review of Finance* 17(2): 527–563.
Ferri, F., and T. Sandino. 2009. The Impact of Shareholder Activism on Financial Reporting and Compensation: The Case of Employee Stock Options Expensing. *The Accounting Review* 84(2): 433–466.
Fich, E., and A. Shivdasani. 2006. Are Busy Boards Effective Monitors? *Journal of Finance* 61(2): 689–724.
Fos, V., K. Li, and M. Tsoutsoura, 2018. Do Director Elections Matter? *Review of Financial Studies* 31(4): 1499–1531.
Fos, V., and M. Tsoutsoura, 2014. Shareholder Democracy in Play: Career Consequences of Proxy Contests. *Journal of Financial Economics* 114(2): 316–340.
Francis, B., I. Hasan, S. Mani, and P. Ye. 2016. Relative Peer Quality and Firm Performance. *Journal of Financial Economics* 122(1): 196–219.
Francis, J., R. LaFond, P. Olsson, and K. Schipper. 2005. The Market Pricing of Accruals Quality.

Journal of Accounting and Economics 39(2): 295–327.
Gleason, C. A., N. T. Jenkins, and W. B. Johnson. 2008. The Contagion Effects of Accounting Restatements. *The Accounting Review* 83(1): 83–110.
Hovakimian, A., and H. Hu. 2020. Anchoring on Historical High Prices and Seasoned Equity Offerings. *Journal of Financial and Quantitative Analysis* 55(8): 2588–2612.
Iliev, P., K. Lins, D. Miller, and L. Roth. 2015. Shareholder Voting and Corporate Governance around the World. *Review of Financial Studies* 28(8): 2167–202.
Illiev, P., and S. Vitanova. 2019. The Effect of the Say-on-Pay Vote in the United States. *Management Science* 65(10): 4451–4949.
Jayaraman, S., and J. S. Wu. 2020. Should I Stay or Should I Grow? Using Voluntary Disclosure to Elicit Market Feedback. *Review of Financial Studies* 33(8): 3854–3888.
Jensen, M. C., and K. J., Murphy. 1990. Performance Pay and Top-Management Incentives. *Journal of Political Economy* 98(2): 225–264.
Jenter, D., and F. Kanaan. 2015. CEO Turnover and Relative Performance Evaluation. *Journal of Finance* 70(5): 2155–2184.
Kaplan, S. N., and L. Zingales. 1997. Do Investment-Cash Flow Sensitivities Provide Useful Measures of Financing Constraints? *Quarterly Journal of Economics* 112(1): 169–215.
Kuhnen, C. M., and A. Niessen. 2012. Public Opinion and Executive Compensation. *Management Science* 58(7): 1249–1272.
Levit, D., and N. Malenko. 2016. The Labor Market for Directors and Externalities in Corporate Governance. *Journal of Finance* 71(2): 775–808.
Liu, C., A. Low, R.W. Masulis, and L. Zhang. 2020. Monitoring the Monitor: Distracted Institutional Investors and Board Governance. *Review of Financial Studies* 33(10): 4489–4531.
Morse, A., V. Nanda, and A. Seru. 2011. Are Incentive Contracts Rigged by Powerful CEOs? *Journal of Finance* 66(5): 1779–1821.
Naaraayanana, S. L, and K. M. Nielsen. 2021. Does Personal Liability Deter Individuals from Serving as Independent Directors? *Journal of Financial Economics* 140(2): 621–643.
Rajgopal, S., T. Shevlin, and V., Zamora. 2006. CEOs' Outside Employment Opportunities and The Lack of Relative Performance Evaluation in Compensation Contracts. *The Journal of Finance* 61(4): 1813–1844.
Semler Brossy. 2019. Say On Pay and Proxy Results – Year-end Report. Semler Brossy Consulting Group: Los Angeles, CA.
Whited, T. M., and G. Wu. 2006. Financial Constraints Risk. *Review of Financial Studies* 19(2): 531–559.
Xu, J., and J. Yang. 2016. Golden Hellos: Signing Bonuses for New Top Executives. *Journal of Financial Economics* 122(1): 175–195.
Yermack, D. 2010. Shareholder Voting and Corporate Governance. *Annual Review of Financial Economics* 2: 103–125.
Zuo, L. 2016. The Informational Feedback Effect of Stock Prices on Management Forecasts. *Journal of Accounting and Economics* 61(2–3): 391–413.

Appendix A: Example of the Disclosure of Compensation Benchmarking Peers

The following is extracted from Apple Inc.'s CD&A section of its proxy statement in 2015.

The Role of Peer Companies and Benchmarking. With the assistance of Pay Governance, the Compensation Committee identified a group of primary peer companies to use for compensation comparison purposes for 2014. In determining the primary peer group, the Compensation Committee selected U.S.-based, stand-alone, publicly traded companies that, in its view, compete with the Company for talent and are in the technology, media, and internet services industries. The threshold revenue and market capitalization requirements for a company to be considered for the peer group were $15 billion and $35 billion, respectively. Based on these criteria, the Company is significantly larger than the other companies in the primary peer group, with a 2014 revenue of $182.8 billion ana d market capitalization of $591 billion at the end of 2014. In addition, although it was slightly below the revenue threshold, the Compensation Committee decided to retain Viacom in the primary peer group for consistency with prior years.

The Compensation Committee selected the following primary peer group for 2014:

Amazon.com	Disney	IBM	Twenty-First Century Fox
AT&T	eBay	Microsoft	Verizon
CBS	EMC	Oracle	Viacom
Cisco Systems	Google	Qualcomm	
Comcast	Hewlett-Packard	Time Warner	
DIRECTV	Intel	Time Warner Cable	

The Compensation Committee also identified a group of secondary peer companies to use for compensation comparison purposes for 2014. With the assistance of Pay Governance, the Compensation Committee decided to focus on premier companies that have iconic brands or are industry bellwethers and category leaders, rely on significant R&D and innovation for growth, and require highly skilled human capital. This was a change from the methodology used in prior years, in which the secondary peer group consisted of "mega-cap" companies, regardless of industry. Applying these criteria, the Compensation Committee selected the following secondary peer group for 2014:

3M	Johnson & Johnson
American Express	Nike
Boeing	PepsiCo
Coca-Cola	Procter & Gamble
General Electric	

Unless otherwise specified, references in this Compensation Discussion and Analysis to peer companies include both the primary and the secondary peer group companies listed above.

Appendix B: Variable Definitions

CEOPAY	=	Total CEO compensation (in thousands) paid by the focal firm.
LN CEOPAY	=	Natural logarithm of total CEO compensation paid by the focal firm.
CAR	=	Focal firm's cumulative abnormal market-adjusted returns from one day before to one day after the annual shareholders' meeting date of an SOP-failed peer.
NEGCAR	=	1 if *CAR* is below the annual sample median, and 0 otherwise.
POST	=	1 for the post-event year (i.e., the first fiscal year ending at least six months after the peer's voting failure), and 0 for the pre-event year.
PR NEGVOTE	=	Percentage of negative votes received by the SOP-failed peer.
PR FAILNUM	=	The number of SOP-failed peers in the compensation benchmarking group.
PR TOTNUM	=	The total number of peers in the compensation benchmarking group.
PR FAILPCT	=	Percentage of SOP-failed peers in the compensation benchmarking group.
PR SEVERITY	=	1 if either *PR NEGVOTE* or *PR FAILPCT* has a value in the top quintile of its sample distribution, and 0 otherwise.
PR CAR	=	SOP-failed peer's cumulative abnormal market-adjusted returns from one day before to one day after the annual shareholders' meeting date.
PR NEGCAR	=	1 if *PR CAR* is negative, zero otherwise.
COMINST PCT	=	Percentage of shares owned by common institutional investors in both the focal firm and an SOP-failed peer.
COMINST	=	1 if *COMINST PCT* is above the sample median, and 0 otherwise.
INTERLOCK	=	1 if the focal firm's director also serves on the board of an SOP-failed peer, and 0 otherwise.
INDPR FAILSOP	=	1 if the focal firm has an SOP-failed industry (SIC-3) peer that is not a compensation peer, and 0 otherwise.
CEOPAY ABOVE	=	1 if the focal firm's CEO pay is above the median pay of the compensation peer group, and 0 otherwise.
PR MEDPAY	=	Median of natural logarithm of a focal firm's all compensation peers' CEO pay.
SIZE	=	Natural logarithm of the focal firm's market value of equity.
BTM	=	Focal firm's book-to-market ratio.
LEV	=	Focal firm's leverage ratio.
RET	=	Focal firm's annual market-adjusted returns.
ROA	=	Focal firm's industry-adjusted return on assets (income before extraordinary items divided by lagged total assets) in percentage points.
TENURE	=	The number of years since the focal firm's CEO has served in that position.
LN TENURE	=	Natural logarithm of the focal firm's CEO tenure.
CEOOWN	=	Percentage of shares owned by the focal firm's CEO.
INST	=	Percentage of shares owned by the focal firm's institutional investors.
SOPVOTE	=	Percentage of favorable votes received by the focal firm.
ISSAGAINST	=	1 if ISS recommends a negative SOP vote against the focal firm, and 0 otherwise.
SOPFREQ	=	1 if the focal firm holds SOP frequency voting, and 0 otherwise.
PR CEOPAY	=	Natural logarithm of an SOP-failed peer's total CEO compensation.
PR PAYABOVE	=	1 if SOP-failed peers' CEO pay is above the median pay of the compensation peer group, and 0 otherwise.
PR RET	=	SOP-failed peer's annual stock returns.
PR ROA	=	SOP-failed peer's return on assets.
TALENTFLOW	=	A measure of the talent flow between the focal firm and an SOP-failed peer. The measure is equal to 1 if at least one of the top five executives moves between the focal firm and the industry of the SOP-failed peer in the most recent five years, 0 otherwise.
DIFF CEORET	=	A measure of the similarity in CEO capability between the focal firm and an SOP-failed peer, equal to the absolute value of the difference in the mean annual stock returns (relative to the S&P 500 index returns) for the most recent three years for the CEO of the focal firm and for that of its SOP-

		failed peer.
TURNOVER	=	1 if the focal firm experiences CEO turnover, and 0 otherwise.
CEOAGE	=	Focal firm's CEO age.
RETIRE	=	1 if the focal firm's CEO is at least 65 years old, and 0 otherwise.
DUALITY	=	1 if the focal firm's CEO also serves as chairman of the board, and 0 otherwise.
INCUMCEO	=	1 if the focal firm's CEO is an incumbent CEO (i.e., not in his/her first year of appointment), and 0 otherwise.
COOPTDIR	=	Percentage of co-opted directors (i.e., directors hired after the focal firm's CEO takes office) following (Coles, Daniel, and Naveen, 2014).
BUSYDIR	=	Percentage of independent directors serving on three or more outside boards.
CEOPOWER	=	Composite index of the CEO's relative power on the board, based on factor scores obtained from the factor analysis on *DUALITY*, *INCUMCEO*, *COOPTDIR*, and *BUSYDIR*.
CONSULT SMALL	=	1 if the focal firm does not hire any of the following top compensation consultants: FW Cook, Willis Towers Watson, Pearl Meyer, Aon Hewitt, Pay Governance, Radford Consulting, and Semler Brossy Consulting, and 0 otherwise.
NEGCAR PSEUDO	=	1 if the focal firm's three-day *NEGCAR* surrounding the pseudo-event dates is above the sample median, and 0 otherwise.
EXCESSPAY	=	Focal firm's excessive CEO compensation, following Core, Guay and Larcker (2008).
EARLY SIGNAL	=	1 if an SOP-failed peer's next fiscal year end following the event is more than six months before the focal firm's next fiscal year end, and 0 otherwise.
NEGSHOCK ROA	=	1 if the change in annual ROA for both focal firms and SOP-failed peers is in the bottom quintile of the sample distribution, and 0 otherwise.
NEGSHOCK RET	=	1 if the change in annual stock returns for both focal firms and SOP-failed peers is in the bottom quintile of the sample distribution, and 0 otherwise.
DIR VOTE	=	Percentage of favorable vote for the director in the election of board membership.
CHG CEOPAY	=	Change in *LN CEOPAY* from the pre- to post-event year.
CEOPAY CUT	=	1 if *CHG CEOPAY* is below the sample median, and 0 otherwise.
DIR AGE65-69	=	1 if the director's age is between 65 and 69, and 0 otherwise.
DIR AGE70	=	1 if the director's age is 70 or older, and 0 otherwise.
DIR TENURE	=	Number of years the director has served on the board.
DIR OUTBOARD	=	Number of the director's outside directorships.
DIR GREY	=	1 for an affiliated outside director (e.g., employment with the company, material interests in the transactions with the focal firm, and family relationships), and 0 otherwise.
DIR FEMALE	=	1 for a female director, and 0 otherwise.
DIR COMPCOM	=	1 if the director is a member of the compensation committee, and 0 otherwise.
DIR AUDITCOM	=	1 if the director is a member of the audit committee, and 0 otherwise.
DIR NOMCOM	=	1 if the director is a member of the nominating committee, and 0 otherwise.
DIR CGCOM	=	1 if the director is a member of the corporate governance committee, and 0 otherwise.
DIR COMCHAIR	=	1 if the director chairs any of the four board committees (compensation, audit, nominating, and corporate governance), and 0 otherwise.
DIROWN	=	Percentage of shares the director owns.
DIR ELECTION	=	1 if the director is up for election at the annual shareholders' meeting, and 0 otherwise.

Appendix C: Comparison of Firms with and without SOP-Failed Compensation Peers

This table compares 1,351 unique firm-years that experience a SOP-failure event (CP-SOP-failure group) and the remaining 5,582 firm-years that do not experience such SOP failures (non-CP-SOP-failure). We measure all these variables in the prior year except for CEO pay. All variables are defined in appendix B. The last two columns report the difference in mean and median. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

	Firm-Years with SOP-Failed Compensation Peers (N=1,351): A		Firm-Years without SOP-Failed Compensation Peers (N=5,582): B		Test of Difference (A – B)	
	Mean	Median	Mean	Median	Diff.Mean	Diff.Median
(1) CEO related variables						
LN CEOPAY$_t$	8.736	8.858	8.751	8.805	-0.015	0.053
CEOPAY ABOVE$_{t-1}$	0.460	0.000	0.445	0.000	0.015	0.000
PR MEDPAY$_{t-1}$	8.826	8.847	8.808	8.833	0.018	0.014
TURNOVER$_{t-1}$	0.074	0.000	0.064	0.000	0.010	0.000
CEOAGE$_{t-1}$	56.460	57.000	56.650	57.000	-0.019	-0.000
RETIRE$_{t-1}$	0.090	0.000	0.090	0.000	0.000	0.000
DUALITY$_{t-1}$	0.523	1.000	0.537	1.000	-0.014	-0.000
INCUMCEO$_{t-1}$	0.938	1.000	0.929	1.000	0.009	0.000
COOPTDIR$_{t-1}$	0.410	0.375	0.380	0.333	0.030***	0.042***
BUSYDIR$_{t-1}$	0.096	0.090	0.098	0.091	-0.002	-0.001
CEOPOWER$_{t-1}$	0.000	0.207	0.000	0.173	0.000	0.034
LN TENURE$_{t-1}$	1.699	1.820	1.595	1.719	0.104***	0.101***
CEOOWN$_{t-1}$	0.013	0.003	0.012	0.003	0.001	0.000
(2) Firm fundamentals						
SIZE$_{t-1}$	8.788	8.550	8.740	8.640	0.048	-0.090
BTM$_{t-1}$	0.449	0.397	0.491	0.412	-0.042***	-0.015**
LEV$_{t-1}$	0.593	0.586	0.618	0.612	-0.025***	-0.026***
RET$_{t-1}$	0.085	0.055	0.082	0.038	0.003	0.017
ROA$_{t-1}$	0.995	0.072	1.307	0.071	-0.312*	0.001
INST$_{t-1}$	0.592	0.775	0.608	0.775	-0.016	-0.000
(3) Voting related variables						
SOPVOTE$_{t-1}$	0.706	0.920	0.691	0.924	0.015	-0.004
ISSAGAINST$_{t-1}$	0.091	0.000	0.078	0.000	0.013	0.000
SOPFREQ$_{t-1}$	0.001	0.000	0.001	0.000	0.000	0.000

Figure 1. Framework of the Feedback Effect Triggered by Peers' SOP Voting Failure

This figure plots the channels of learning for focal firm directors after a firm's compensation peers announce an SOP voting failure. The peer firm's SOP voting failure may cause a negative market response toward the focal firm. The focal firm's *CAR* provides an additional source of information for the firm's directors regarding shareholder opinion about the event. The focal firm may cut CEO pay after its directors observe the compensation peer firm's SOP voting failure and the focal firm's own *CAR* around the peer's event.

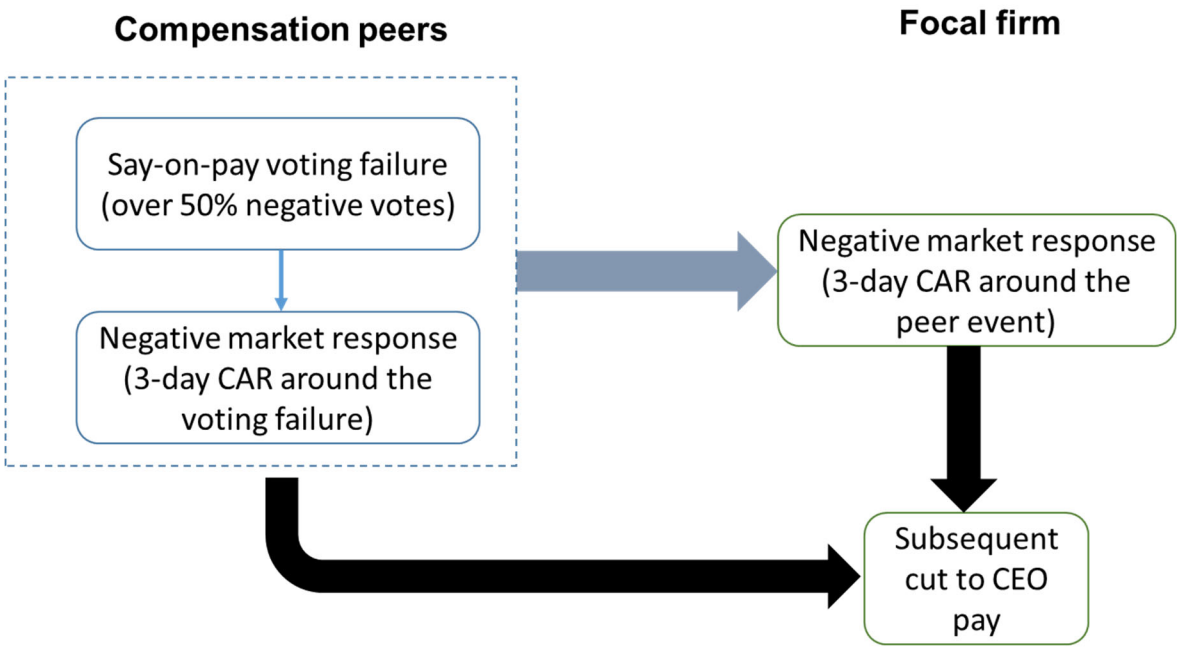

Table 1: Sample Selection and Summary Statistics

This table shows the sample selection process and presents the summary statistics. Panel A presents the sample selection. In the final sample, each firm-event corresponds to two firm-year observations, including one pre-event year and one post-event year. The post-event year refers to the first year when a focal firm adjusts its executive pay subsequent to the peers' SOP voting failure (i.e., the fiscal year end of the focal firm is at least six months after the peer event). The pre-event year refers to the year before the post-event year. Panel B reports the descriptive statistics for the variables used in the firm-level analyses. All variables are defined in appendix B.

Panel A: Sample selection

	Number of Firm-Year Observations	Number of Focal Firms
Focal firms covered by Incentive Lab during the proxy seasons of 2011-2018	7,432	1,157
Less: Focal firms with missing data	364	286
Focal firms that fail in the SOP voting	135	100
Focal firms without any SOP-failed peers	5,582	1,088
Focal firms with at least one SOP-failed peer	1,351 firm-events (2,702 firm-years)	686

Panel B: Descriptive statistics for firm-level variables (N = 2,702)

	Mean	Std. Dev.	25%	Median	75%
CEOPAY$_t$ (in USD 000's)	8,621	7,229	4,216	7,217	10,757
LN CEOPAY$_t$	8.717	1.350	8.347	8.884	9.283
CAR$_{t-1}$	0.000	0.023	-0.012	0.000	0.012
PR NEGVOTE$_{t-1}$	0.617	0.091	0.546	0.590	0.675
PR FAILNUM$_{t-1}$	1.176	0.491	1.000	1.000	1.000
PR TOTNUM$_{t-1}$	21.811	30.922	12.000	15.000	20.000
PR FAILSOP$_{t-1}$	0.079	0.046	0.053	0.071	0.091
PR SEVERITY$_{t-1}$	0.356	0.479	0.000	0.000	1.000
PR CAR$_{t-1}$	-0.003	0.031	-0.019	-0.004	0.011
COMINST PCT$_{t-1}$	0.526	0.228	0.438	0.557	0.667
INTERLOCK$_{t-1}$	0.010	0.101	0.000	0.000	0.000
INDPR FAILSOP$_{t-1}$	0.019	0.135	0.000	0.000	0.000
CEOPAY ABOVE$_{t-1}$	0.449	0.497	0.000	0.000	1.000
PR MEDPAY$_{t-1}$	8.689	1.324	8.492	8.870	9.224
SIZE$_{t-1}$	8.837	1.446	7.875	8.609	9.615
BTM$_{t-1}$	0.442	0.337	0.232	0.393	0.605
LEV$_{t-1}$	0.596	0.223	0.462	0.587	0.723
RET$_t$	0.026	0.293	-0.154	0.008	0.187
RET$_{t-1}$	0.060	0.313	-0.132	0.035	0.214
ROA$_t$	1.097	4.671	0.004	0.073	0.638
ROA$_{t-1}$	0.654	10.541	0.004	0.073	0.546
TENURE$_{t-1}$	8.126	7.147	2.786	6.165	11.431
LN TENURE$_{t-1}$	1.693	0.979	1.025	1.819	2.436
CEOOWN$_{t-1}$	0.013	0.031	0.001	0.003	0.010
INST$_{t-1}$	0.600	0.393	0.001	0.783	0.911
SOPVOTE$_{t-1}$	0.762	0.322	0.689	0.931	0.968
ISSAGAINST$_{t-1}$	0.092	0.289	0.000	0.000	0.000
SOPFREQ$_t$	0.100	0.299	0.000	0.000	0.000
PR CEOPAY$_{t-1}$	9.298	0.927	8.688	9.273	9.760
PR PAYABOVE$_{t-1}$	0.494	0.500	0.000	0.000	0.000
PR RET$_{t-1}$	0.071	0.328	-0.133	0.014	0.244
PR ROA$_{t-1}$	0.040	0.077	0.009	0.041	0.083

$TALENTFLOW_{t-1}$	0.378	0.485	0.000	0.000	1.000
$DIFF\ CEORET_{t-1}$	0.205	0.243	0.027	0.135	0.292
$TURNOVER_{t-1}$	0.072	0.258	0.000	0.000	0.000
$CEOAGE_{t-1}$	56.584	6.642	52.000	57.000	61.000
$RETIRE_{t-1}$	0.094	0.292	0.000	0.000	0.000
$DUALITY_{t-1}$	0.523	0.497	0.000	1.000	1.000
$INCUMCEO_{t-1}$	0.938	0.240	1.000	1.000	1.000
$COOPTDIR_{t-1}$	0.410	0.351	0.000	0.375	0.714
$BUSYDIR_{t-1}$	0.096	0.115	0.000	0.090	0.143
$CEOPOWER_{t-1}$	0.000	1.701	-0.831	0.207	1.238
$CONSULT\ SMALL_{t-1}$	0.529	0.499	0.000	1.000	1.000

Table 2: Sensitivity of Focal Firms' Prices to the Severity of Peers' SOP-Failure Events

This table reports the determinants of a focal firm's price response to its peer's SOP-failure event. The dependent variable is CAR_t, which equals the focal firm's cumulative abnormal market-adjusted returns from one day before to one day after the annual shareholders' meeting date of an SOP-failed peer. *PR SEVERITY*$_t$ is an indicator that equals 1 if the focal firm's peers experience severe SOP-failures, and zero otherwise (it is calculated based on both the percentage of negative votes received by SOP-failed peers and the percentage of SOP-failed peers in the compensation benchmarking group; please refer to main text for a detailed definition). *PR NEGVOTE*$_t$ is the percentage of negative votes the SOP-failed peers receive. *PR FAILPCT*$_t$ is the percentage of SOP-failed peers in the compensation benchmarking group. *PR CAR*$_t$ is the SOP-failed peer's cumulative abnormal market-adjusted returns from one day before to one day after the annual shareholders' meeting date. All variables are defined in appendix B. The *t*-statistics are reported below the corresponding coefficients. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)
Dependent Variable	CAR_t	
PR SEVERITY$_t$	-0.004**	
	(-2.02)	
PR NEGVOTE$_t$		-0.025***
		(-2.39)
PR FAILPCT$_t$		0.029
		(1.10)
PR CAR$_t$	0.188***	0.181***
	(6.46)	(6.20)
COMINST$_t$	-0.002	-0.003
	(-0.87)	(-1.17)
INTERLOCK$_t$	0.004	0.002
	(0.50)	(0.25)
INDPR FAILSOP$_t$	0.001	0.001
	(0.18)	(0.10)
LN CEOPAY$_{t-1}$	0.002	0.002
	(1.41)	(1.34)
CEOPAY ABOVE$_{t-1}$	-0.001	-0.001
	(-0.66)	(-0.47)
PR MEDPAY$_{t-1}$	0.001	0.001
	(0.11)	(0.25)
SIZE$_{t-1}$	0.000	-0.000
	(0.15)	(-0.04)
BTM$_{t-1}$	0.005	0.004
	(0.81)	(0.58)
LEV$_{t-1}$	0.013	0.008
	(1.01)	(0.63)
RET$_{t-1}$	-0.000	-0.000
	(-0.08)	(-0.03)
ROA$_{t-1}$	-0.000	-0.000
	(-0.27)	(-0.56)
LN TENURE$_{t-1}$	-0.001	-0.001
	(-1.08)	(-1.03)
CEOOWN$_{t-1}$	0.016	0.004

	(0.21)	(0.05)
$INST_{t-1}$	0.006	0.007
	(0.77)	(0.87)
$SOPVOTE_{t-1}$	0.001	0.001
	(0.30)	(0.16)
$ISSAGAINST_{t-1}$	-0.007**	-0.007**
	(-2.18)	(-2.23)
$PR\ CEOPAY_{t-1}$	-0.001	-0.000
	(-0.57)	(-0.21)
$PR\ PAYABOVE_{t-1}$	0.001	0.000
	(0.25)	(0.10)
$PR\ RET_{t-1}$	-0.003	-0.003
	(-0.97)	(-1.07)
$PR\ ROA_{t-1}$	-0.016	-0.015
	(-1.13)	(-1.08)
$TALENTFLOW_{t-1}$	0.000	-0.000
	(0.09)	(-0.05)
$DIFF\ CEORET_{t-1}$	0.001	0.002
	(0.27)	(0.37)
$TURNOVER_{t-1}$	0.001	0.002
	(0.39)	(0.41)
$RETIRE_{t-1}$	0.002	0.002
	(0.51)	(0.49)
Constant	-0.025	-0.015
	(-0.55)	(-0.34)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Observations	1,351	1,351
Adjusted R-squared	13.37%	13.59%

Table 3: Feedback Effect and Focal Firms' CEO Pay – A Difference-in-Differences Analysis

This table reports the effect of focal firms' market responses on their total compensation. The dependent variable *LN CEOPAY*$_t$ is the natural logarithm of a focal firm's total CEO pay. *NEGCAR*$_{t-1}$ equals 1 if the focal firm's market reaction to the peer event is below the annual sample median (i.e., a more negative *CAR*), and zero otherwise. *POST*$_t$ is equal to 1 for the post-event year (i.e., the first fiscal year ending at least six months after the peer's voting failure), and zero otherwise. All variables are defined in appendix B. The *t*-statistics are reported below the corresponding coefficients. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)
Dependent Variable		*LN CEOPAY*$_t$	
POST$_t$ * *NEGCAR*$_{t-1}$	-0.134***		-0.130***
	(-3.70)		(-3.55)
NEGCAR$_{t-1}$	0.088***		0.085***
	(2.76)		(2.62)
POST$_t$ * *PR SEVERITY*$_{t-1}$		-0.089**	-0.083**
		(-2.25)	(-2.09)
PR SEVERITY$_{t-1}$		0.036	0.032
		(1.01)	(0.89)
POST$_t$ * *PR NEGCAR*$_{t-1}$		-0.016	0.003
		(-0.43)	(0.09)
PR NEGCAR$_{t-1}$		0.019	0.006
		(0.57)	(0.18)
POST$_t$	0.074**	0.045	0.099***
	(2.34)	(1.29)	(2.61)
COMINST$_{t-1}$	-0.028	-0.029	-0.029
	(-0.79)	(-0.81)	(-0.83)
INTERLOCK$_{t-1}$	0.104	0.106	0.104
	(0.82)	(0.83)	(0.82)
INDPR FAILSOP$_{t-1}$	0.082	0.082	0.083
	(0.89)	(0.89)	(0.90)
LN CEOPAY$_{t-1}$	0.007	0.005	0.005
	(0.43)	(0.34)	(0.35)
CEOPAY ABOVE$_{t-1}$	-0.043	-0.041	-0.040
	(-1.52)	(-1.46)	(-1.43)
PR MEDPAY$_{t-1}$	-0.004	-0.003	-0.002
	(-0.24)	(-0.16)	(-0.13)
SIZE$_{t-1}$	0.282***	0.287***	0.283***
	(6.62)	(6.70)	(6.61)
BTM$_{t-1}$	0.180**	0.173**	0.178**
	(2.12)	(2.03)	(2.10)
LEV$_{t-1}$	0.247	0.231	0.243
	(1.38)	(1.28)	(1.35)
RET$_t$	0.258***	0.258***	0.255***
	(6.11)	(6.08)	(6.03)
RET$_{t-1}$	0.075*	0.067	0.075*
	(1.80)	(1.61)	(1.81)
ROA$_t$	0.005	0.004	0.004

	(1.63)	(1.48)	(1.49)
ROA_{t-1}	0.001	0.001	0.001
	(1.08)	(1.14)	(1.10)
$LN\ TENURE_{t-1}$	0.042**	0.042**	0.042**
	(2.35)	(2.35)	(2.38)
$CEOOWN_{t-1}$	-0.498	-0.417	-0.488
	(-0.50)	(-0.42)	(-0.49)
$INST_{t-1}$	0.229**	0.241**	0.240**
	(2.16)	(2.26)	(2.26)
$SOPVOTE_{t-1}$	0.039	0.036	0.037
	(0.78)	(0.72)	(0.75)
$ISSAGAINST_{t-1}$	-0.042	-0.043	-0.042
	(-1.01)	(-1.02)	(-1.02)
$SOPFREQ_{t-1}$	0.013	0.022	0.021
	(0.28)	(0.50)	(0.47)
$PR\ CEOPAY_{t-1}$	-0.002	-0.005	-0.002
	(-0.12)	(-0.25)	(-0.13)
$PR\ PAYABOVE_{t-1}$	0.002	-0.002	0.001
	(0.08)	(-0.06)	(0.04)
$PR\ RET_{t-1}$	0.081**	0.081**	0.079**
	(2.29)	(2.27)	(2.23)
$PR\ ROA_{t-1}$	-0.151	-0.149	-0.170
	(-0.88)	(-0.86)	(-0.99)
$TALENTFLOW_{t-1}$	-0.066**	-0.071**	-0.069**
	(-2.23)	(-2.40)	(-2.33)
$DIFF\ CEORET_{t-1}$	-0.108*	-0.114**	-0.116**
	(-1.92)	(-2.01)	(-2.05)
$TURNOVER_{t-1}$	0.055	0.054	0.057
	(1.13)	(1.10)	(1.16)
$RETIRE_{t-1}$	-0.125**	-0.128**	-0.129**
	(-2.23)	(-2.27)	(-2.29)
Constant	5.674***	5.681***	5.645***
	(12.37)	(12.28)	(12.24)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	2,702	2,702	2,702
Adjusted R-squared	88.00%	87.94%	88.01%

Table 4: Cross-Sectional Analyses

This table reports the cross-sectional analyses of the feedback effect on CEO pay. The dependent variable is *LN CEOPAY*$_t$, the natural logarithm of the focal firm's total CEO pay. *NEGCAR*$_{t-1}$ equals 1 if the focal firm's market reaction to the peer event is below the annual sample median (i.e., a more negative *CAR*), and zero otherwise. *POST*$_t$ is equal to 1 for the post-event year (i.e., the first fiscal year ending at least six months after the peer's voting failure), and zero otherwise. In column (1), *VAR*$_{t-1}$ = *CEOPOWER*$_{t-1}$, which refers to the CEO's relative power on the board (based on CEO-chair duality, whether the CEO is not newly appointed, the presence of co-opted directors, and the busyness of the board). In column (2), *VAR*$_{t-1}$ = *ISSAGAINST*$_{t-1}$, equal to 1 if ISS recommends a negative SOP vote against the focal firm, and zero otherwise. In column (3), *VAR*$_{t-1}$ = *CONSULT SMALL*$_{t-1}$, equal to 1 if the focal firm does not hire any of the top compensation consultants and zero otherwise. We use control variables from table 3. In addition, in column (3), we control for industry R&D share (*IND RD* = percentage of the firm's R&D expenses in the industry), industry sales growth (*IND SALEG* = percentage change in industry-level sales), asset intangibility (*INTANG* = ratio of intangible assets to total assets) (Naaraayanana and Nielsen, 2021), as well as accrual quality (*AQ*) based on the Dechow and Dichev (2002) model modified by Francis, LaFond, Olsson, and Schipper (2005) (Billett and Yu, 2016; Xu and Yang, 2016). We also control for the composite index of financial constraints (*FC*) based on the factor scores from the factor analysis on the modified KZ index (Kaplan and Zingales, 1997; Baker, Stein, and Wurgler, 2003) and the WW index (Whited and Wu, 2006). All variables are defined in appendix B. The *t*-statistics are reported below the corresponding coefficients. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)
Dependent Variable		*LN CEOPAY*$_t$	
Interaction Variable	*CEOPOWER*$_{t-1}$	*ISSAGAINST*$_{t-1}$	*CONSULT SMALL*$_{t-1}$
POST$_t$ * *NEGCAR*$_{t-1}$ * *VAR*$_{t-1}$	0.072***	-0.236*	-0.144**
	(2.77)	(-1.68)	(-1.98)
NEGCAR$_{t-1}$	0.085***	0.070**	0.054
	(2.63)	(2.08)	(1.16)
POST$_t$	0.101***	0.100**	0.062
	(2.64)	(2.52)	(1.35)
VAR$_{t-1}$	0.018	-0.058	-0.048
	(0.99)	(-0.75)	(-0.85)
POST$_t$ * *NEGCAR*$_{t-1}$	-0.129***	-0.108***	-0.053
	(-3.53)	(-2.80)	(-1.01)
NEGCAR$_{t-1}$ * *VAR*$_{t-1}$	-0.047**	0.154	0.058
	(-2.12)	(1.47)	(0.91)
POST$_t$ * *VAR*$_{t-1}$	-0.013	-0.002	0.076
	(-0.71)	(-0.02)	(1.47)
POST$_t$ * *PR SEVERITY*$_{t-1}$	-0.083**	-0.080**	-0.086**
	(-2.10)	(-2.02)	(-2.18)
PR SEVERITY$_{t-1}$	0.026	0.030	0.033
	(0.74)	(0.85)	(0.93)
POST$_t$ * *PR NEGCAR*$_{t-1}$	0.001	0.001	0.006
	(0.02)	(0.04)	(0.17)
PR NEGCAR$_{t-1}$	0.006	0.007	0.002
	(0.18)	(0.20)	(0.06)
Control variables	Yes	Yes	Yes
Observations	2,702	2,702	2,702
Adjusted R-squared	88.50%	88.03%	88.01%

This table reports the results based on three types of pseudo-event dates. The dependent variable is the natural logarithm of the focal firm's CEO pay (*LN CEOPAY*$_t$). Panel A presents the results of the placebo test based on a set of pseudo-event dates from 50 days before to 50 days after the actual event dates. Panel B presents the result of the placebo test based on a sample of focal firms without SOP-failed compensation peers but with SOP-failed industry peers that are not compensation peers. The pseudo-event dates in panel B are the industry peers' SOP-failure event dates. *NEGCAR PSEUDO*$_{t-1}$ equals 1 if the focal firm's market reaction surrounding the pseudo-event date is below the annual sample median, and zero otherwise. *POST*$_t$ equals 1 for the post-event year, and zero otherwise. We use the same set of control variables as in column (3) of table 3. All variables are defined in appendix B. The *t*-statistics are reported below the corresponding coefficients. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Panel A: Pseudo-event dates from 50 days before to 50 days after the actual event dates

	-50 days (1)	-40 days (2)	-30 days (3)	-20 days (4)	-10 days (5)	+10 days (6)	+20 days (7)	+30 days (8)	+40 days (9)	+50 days (10)
Dependent Variable	*LN CEOPAY*$_t$									
POST$_t$ * *NEGCAR PSEUDO*$_{t-1}$	0.042	-0.009	-0.041	-0.003	0.020	0.017	0.009	-0.048	0.001	-0.003
	(1.18)	(-0.25)	(-1.16)	(-0.08)	(0.56)	(0.48)	(0.24)	(-1.34)	(0.02)	(-0.08)
NEGCAR PSEUDO$_{t-1}$	-0.042	0.017	0.033	-0.006	-0.010	-0.008	-0.044	0.020	0.008	0.015
	(-1.33)	(0.55)	(1.05)	(-0.20)	(-0.32)	(-0.26)	(-1.43)	(0.66)	(0.25)	(0.47)
POST$_t$	0.008	0.033	0.058	0.026	0.021	0.022	0.022	0.062	0.029	0.030
	(0.21)	(0.87)	(1.49)	(0.69)	(0.55)	(0.56)	(0.58)	(1.58)	(0.76)	(0.77)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,702	2,702	2,702	2,702	2,702	2,702	2,702	2,702	2,702	2,702
Adjusted R^2	88.38%	88.49%	88.43%	88.36%	88.37%	88.37%	88.38%	88.44%	88.49%	88.37%

Panel B: Placebo tests based on focal firms without SOP-failed compensation peers but with SOP-failed industry peers

Dependent Variable	*LN CEOPAY*$_t$
POST$_t$ * *NEGCAR PSEUDO*$_{t-1}$	0.208
	(1.05)
NEGCAR PSEUDO$_{t-1}$	-0.483
	(-0.53)
POST$_t$	0.166
	(0.83)
Control variables	Yes
Observations	364
Adjusted R-squared	36.68%

Table 6: Robustness Tests

Panel A reports the results using the alternative sample consisting of event firms (i.e., focal firms with SOP-failed peers) and propensity-score-matched nonevent firms (i.e., focal firms without SOP-failed peers) in column (1), the alternative sample excluding event firms whose voting dates overlap with SOP-failed peers' voting dates in column (2), and excessive CEO compensation ($EXCESSPAY_t$) as the dependent variable in column (3). Panel B reports the results using the alternative window of market reaction to the peer event from two days before to two days after the peer event (*CAR[-2,+2]*) in column (1), the aggregate *CAR* over multiple peer events in column (2), the residual *CAR* (estimated from the regression of *CAR* on independent variables as specified in eq. (1)) in column (3), the alternative definition of *CAR* based on returns on failed SOP dates minus returns on successful SOP dates in column (4), and the alternative definition of *CAR* based on returns on failed SOP dates minus returns on other dates in the year in column (5). Panel C reports the results based on the alternative timeline for defining *POST* by excluding the event year (i.e., the focal firm's fiscal year when its peer fails in the SOP voting). Control variables are included as in column (3) of table 4. All variables are defined in appendix B. The *t*-statistics are reported below the corresponding coefficients. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Panel A: Alternative samples and alternative CEO pay measure

	Alternative Sample: Event Firms and PSM Nonevent Firm	Alternative Sample: Excluding Event Firms Whose Voting Dates Overlap with SOP-Failed Peers' Voting Dates	Excessive CEO Pay
	(Dependent Variable = $LN\ CEOPAY_t$)	(Dependent Variable = $LN\ CEOPAY_t$)	(Dependent Variable = $EXCESSPAY_t$)
	(1)	(2)	(3)
$POST_t * NEGCAR_{t-1}$	-0.108***	-0.134***	-0.120***
	(-2.85)	(-3.44)	(-3.28)
$NEGCAR_{t-1}$	0.088***	0.084**	0.076**
	(2.68)	(2.40)	(2.36)
$POST_t$	0.044*	0.092**	0.078**
	(1.80)	(2.26)	(2.06)
Control variables	Yes	Yes	Yes
Observations	4,054	2,504	2,702
Adjusted R^2	80.83%	88.16%	87.11%

Panel B: Alternative *CAR* measures

	CAR [-2,+2]	Aggregate *CAR[-1,+1]*	Residual *CAR [-1,+1]*	Alternative *CAR*: Returns on Failed SOP Dates Minus Returns on Successful SOP Dates	Alternative *CAR*: Returns on Failed SOP Dates Minus Returns on Other Dates in the Year
	(1)	(2)	(3)	(4)	(5)
Dependent Variable				$LN\ CEOPAY_t$	
$POST_t * NEGCAR_{t-1}$	-0.082**	-0.143***	-0.089**	-0.104***	-0.081**
	(-2.25)	(-3.93)	(-2.45)	(-2.87)	(-1.96)
$NEGCAR_{t-1}$	0.047	0.092***	0.047	0.044	0.032
	(1.49)	(2.82)	(1.46)	(1.40)	(0.89)
$POST_t$	0.080**	0.105***	0.090**	0.094**	0.059
	(2.09)	(2.76)	(2.28)	(2.41)	(1.47)
Control variables	Yes	Yes	Yes	Yes	Yes
Observations	2,702	2,702	2,702	2,702	2,702
Adjusted R^2	87.96%	88.02%	87.96%	87.98%	88.17%

Panel C: Alternative timeline for determining pre- and post-event years

	(1)	(2)	(3)
Dependent Variable		*LN CEOPAY$_t$*	
*POST$_t$ * NEGCAR$_{t-1}$*	-0.086*		-0.090*
	(-1.74)		(-1.80)
NEGCAR$_{t-1}$	-0.006		-0.010
	(-0.14)		(-0.22)
POST$_t$	0.089**	0.044	0.081*
	(2.27)	(1.06)	(1.73)
*POST$_t$ * PR SEVERITY$_{t-1}$*		0.006	0.011
		(0.12)	(0.20)
PR SEVERITY$_{t-1}$		-0.031	-0.031
		(-0.63)	(-0.64)
*POST$_t$ * PR NEGCAR$_{t-1}$*		0.002	0.015
		(0.04)	(0.29)
PR NEGCAR$_{t-1}$		0.030	0.036
		(0.65)	(0.76)
Control variables	Yes	Yes	Yes
Observations	2,644	2,644	2,644
Adjusted R^2	80.83%	80.77%	80.81%

Table 7: Feedback Effect and Consequence on Voting in Director Elections

This table reports the joint effect of board learning and CEO compensation on the voting outcomes of director elections. The dependent variable is measured at the director-firm level: $DIR\ VOTE_{i,j,t}$ is the percentage of favorable votes for the director (j) of firm i in the board election in the annual meeting for fiscal year (t). $CEOPAY\ CUT_i$ equals 1 if the change in the focal firm's CEO pay from the pre- to post-event year (i.e., from t-1 to t) is below the sample median, and zero otherwise. $POST_t$ equals 1 for director elections in the post-event year, and zero for those in the pre-event year. The regression in columns (1) is based on the sample of all directors. The regression in column (2) is based on the subsample of directors on the compensation committee, and the regression in column (3) is based on the subsample of those on other board committees (i.e., audit committee, nominating committee, and governance committee). The sample includes directors of all firms that experience a negative market response around their peer firms' SOP voting failures. All variables are defined in appendix B. The t-statistics are reported below the corresponding coefficients. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Sample of Directors Used	All Directors	Compensation Committee Members	Other Committee Members
	(1)	(2)	(3)
Dependent Variable		$DIR\ VOTE_{i,j,t}$	
$POST_t$ * $CEOPAY\ CUT_i$	**0.015*** **	**0.018*** **	**0.012** **
	(3.50)	**(2.63)**	**(2.16)**
$CEOPAY\ CUT_i$	-0.000	-0.003	0.002
	(-0.15)	(-0.60)	(0.41)
$POST_t$	-0.007**	-0.007	-0.005
	(-2.21)	(-1.47)	(-1.32)
$DIR\ AGE65\text{-}69_t$	0.003	0.004	-0.000
	(1.08)	(1.02)	(-0.09)
$DIR\ AGE70_t$	0.001	0.009*	-0.000
	(0.47)	(1.83)	(-0.09)
$DIR\ TENURE_t$	-0.002***	-0.003***	-0.002***
	(-12.08)	(-8.73)	(-7.89)
$DIR\ OUTBOARD_t$	-0.010***	-0.008***	-0.011***
	(-9.85)	(-5.08)	(-8.53)
$DIR\ GREY_t$	-0.021***		-0.017***
	(-4.25)		(-3.25)
$DIR\ FEMALE_t$	0.008***	0.012***	0.009***
	(3.20)	(2.84)	(2.73)
$DIR\ COMPCOM_t$	-0.011***		
	(-5.19)		
$DIR\ AUDITCOM_t$	0.004**	-0.004	0.011***
	(2.05)	(-1.01)	(3.96)
$DIR\ NOMCOM_t$	0.013	0.008	0.014
	(0.80)	(0.28)	(0.63)
$DIR\ CGCOM_t$	-0.024	-0.016	-0.026
	(-1.42)	(-0.55)	(-1.12)
$DIR\ COMPCHAIR_t$	-0.004*	-0.004	-0.006*
	(-1.78)	(-1.25)	(-1.84)
$DIROWN_t$	0.038	0.685***	0.010
	(0.86)	(2.61)	(0.18)
$DIR\ ELECTION_{t+1}$	0.004	0.020	-0.002
	(0.43)	(1.29)	(-0.18)
RET_t	-0.014**	-0.020**	-0.012
	(-2.18)	(-2.07)	(-1.40)
RET_{t+1}	-0.011*	-0.024***	-0.003
	(-1.95)	(-2.70)	(-0.45)

$SIZE_t$	0.018***	0.032***	0.009
	(3.13)	(3.53)	(1.23)
BTM_t	0.021*	0.078***	-0.016
	(1.90)	(4.49)	(-1.15)
LEV_t	-0.046*	-0.034	-0.046
	(-1.79)	(-0.82)	(-1.43)
$TURNOVER_t$	0.014**	0.003	0.019***
	(2.56)	(0.30)	(2.70)
Constant	0.836***	0.673***	0.927***
	(13.92)	(7.08)	(12.12)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	8,167	3,095	5,072
Adjusted R^2	60.32%	64.21%	60.13%

Internet Appendix

Table A1: Additional Robustness Tests and Placebo Test

Panel A reports the results with additional controls for compensation targeting. The two proxies related to compensation targeting include *CEOPAY ABOVE*$_{t-1}$ (equal to 1 if the focal firm's CEO pay is above the median pay for the compensation peer group, and zero otherwise) and *EARLY SIGNAL*$_t$ (equal to 1 if an SOP-failed peer's next fiscal year end following the event is more than six months before the focal firm's next fiscal year-end, and zero otherwise). Panel B reports the results with additional controls for extreme declines in ROA (*NEGSHOCK ROA*$_{t-1}$) and stock returns (*NEGSHOCK RET*$_{t-1}$) of focal firms and their SOP-failed peers. Panel C reports the result for the impact of SOP-failed peers' CEO pay relative to the median pay of compensation peer group (*PR PAYABOVE*$_{t-1}$). Panel D presents the result based on pseudo-event dates when non-SOP-failed compensation peers experience extremely negative returns (i.e., a price decrease of more than 10% in one day). *NEGCAR PSEUDO*$_{t-1}$ equals 1 if the focal firm's market reaction surrounding the pseudo-event date is below the annual sample median, and zero otherwise. *POST*$_t$ equals one for the post-event year, and zero otherwise.

Panel A: Robustness - Control for compensation targeting

Dependent Variable	*LN CEOPAY*$_t$
POST$_t$ * *NEGCAR*$_{t-1}$	-0.134***
	(-3.66)
NEGCAR$_{t-1}$	0.088***
	(2.71)
POST$_t$	0.161***
	(3.68)
POST$_t$ * *PR SEVERITY*$_{t-1}$	-0.086**
	(-2.18)
PR SEVERITY$_{t-1}$	0.036
	(1.01)
POST$_t$ * *PR NEGCAR*$_{t-1}$	0.005
	(0.14)
PR NEGCAR$_{t-1}$	0.004
	(0.13)
POST$_t$ * *CEOPAY ABOVE*$_{t-1}$	-0.127***
	(-3.23)
CEOPAY ABOVE$_{t-1}$	0.023
	(0.68)
POST$_t$ * *EARLY SIGNAL*$_t$	-0.006
	(-0.12)
EARLY SIGNAL$_t$	-0.017
	(-0.31)
Control variables	Yes
Observations	2,702
Adjusted R-squared	88.05%

Panel B: Robustness - Control for extreme declines in firm performance

	Extreme Decline in ROA	Extreme Decline in RET
Dependent Variable = *LN CEOPAY*$_t$	(1)	(2)
POST$_t$ * *NEGCAR*$_{t-1}$	-0.130***	-0.130***
	(-3.55)	(-3.55)
NEGCAR$_{t-1}$	0.082**	0.083**

	(2.50)	(2.55)
$POST_t$	0.098**	0.098***
	(2.55)	(2.57)
NEGSHOCK ROA$_{t-1}$	-0.035	
	(-0.57)	
NEGSHOCK RET$_{t-1}$		0.019
		(0.36)
Control variables	Yes	Yes
Observations	2,702	2,702
Adjusted R-squared	87.99%	87.99%

Panel C: Robustness - Control for SOP-failed peers' CEO pay relative to the median pay of compensation peer group

Dependent Variable	*LN CEOPAY$_t$*
*POST$_t$ * NEGCAR$_{t-1}$*	-0.131***
	(-3.56)
NEGCAR$_{t-1}$	0.084***
	(2.61)
POST$_t$	0.087*
	(1.78)
*POST$_t$ * PR PAYABOVE$_{t-1}$*	0.020
	(0.40)
PR PAYABOVE$_{t-1}$	-0.009
	(-0.23)
Control variables	Yes
Observations	2,702
Adjusted R-squared	88.00%

Panel D: Pseudo-events of extreme negative returns experienced by non-SOP-failed compensation peers

Dependent Variable	*LN CEOPAY$_t$*
*POST$_t$ * NEGCAR PSEUDO$_{t-1}$*	-0.017
	(-0.68)
NEGCAR$_{t-1}$	-0.022
	(-0.99)
POST$_t$	0.063***
	(2.94)
*POST$_t$ * PR NEGCAR$_{t-1}$*	-0.016
	(-0.64)
PR NEGCAR$_{t-1}$	0.011
	(0.52)
Control variables	Yes
Observations	6,974
Adjusted R-squared	66.30%