On the Consequences of Mandatory CEO Pay Ratio Disclosure

Lucas Knust[a], David Oesch[b]

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Abstract:

We examine the consequences of the highly anticipated and controversial Section 953(b) of the Dodd-Frank Act, which mandates companies to disclose the CEO-to-median employee pay ratio starting from 2018. We address endogeneity concerns by using a regression discontinuity design around the public float of companies. Contrary to one of the main arguments of the supporters of the rule, the disclosure requirement does not reduce CEO compensation. We also find no evidence that investors are substantially influenced by the disclosure since firms that disclose the ratio experience no change in investor attention and no change in say-on-pay voting outcomes.

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[a] University of Zurich, Department of Business Administration, Plattenstrasse 14, 8032 Zurich, Switzerland. Email address: lucas.knust@business.uzh.ch.

[b] Corresponding Author. University of Zurich, Department of Business Administration, Plattenstrasse 14, 8032 Zurich, Switzerland. E-mail address: david.oesch@business.uzh.ch.

"[…] income and wealth inequality are growing concerns for many Americans. Over the last twenty years, incomes for the top 1 percent of earners have grown by more than 86 percent, while incomes for the other 99 percent have grown by less than 7 percent. Even as our economy has started to recover from the financial crisis, the top 1 percent have captured 95 percent of the income gains over the last three years, while real median income remains 9 percent below 1999 levels. Especially in this environment, investors need to know to what extent skyrocketing disparities between CEO and worker pay are justified based on performance or simply reflect value capture by insiders."

R. Menendez et al., November 26, 2013

Introduction

The rule requiring companies to disclose the ratio of the pay of the median employee to the compensation of the chief executive officer (pay ratio) is one of the most controversial and commented upon in the history of the Securities Exchange Commission (SEC) (Loh, 2017). The pay ratio disclosure is controversial due to its high public salience and its social purpose of contributing to a public conversation about pay inequality (Bank and Georgiev, 2019). After the preliminary rule, the SEC received more than 287,400 comment letters in support or opposition to the mandate, which represents a level of public engagement not experienced by the SEC before (SEC, 2015).

There has been an ongoing debate about the consequences of the pay ratio disclosure. Supporters of the pay ratio rule, including labor unions and institutional investors, claim that the pay ratio provides materially useful information to investors and could curb excessive chief executive officer (CEO) pay. Opponents of the pay ratio, such as industry trade groups, argue that investors are not interested in this information and that the rule only imposes costs on firms. Our paper assesses both the main arguments of the supporters and the main arguments of the opponents by examining whether the rule affects executive compensation and investor behavior.

Drawing causal conclusions about the consequences of a new rule on firms is inherently difficult as concurrent events, such as institutional changes, general time trends and market-wide shocks, can simultaneously impact the firm (Leuz and Wysocki, 2016). In our case the

years surrounding the pay ratio disclosure rule coincided with a variety of factors impacting executive pay and investor behavior. Examples of these potentially confounding events include deregulations introduced by president Trump (elected in 2016), tax code changes for executives and businesses under the Tax Cuts and Jobs Act, as well as accelerating economic growth and high stock returns (Egan and Wiener-Bronner, 2017).

To address these concerns, our identification strategy is based on a regression discontinuity research design in the spirit of Iliev and Vitanova (2019). We exploit exemptions granted to firms based on their filing status, which is determined by firms' public floats. Specifically, companies are generally considered "smaller reporting companies" if their public float is less than 75 million USD, and "smaller reporting companies" are exempted from complying with the pay ratio disclosure rule. Hence, we assess the impact of the pay ratio rule on a sample of firms around the public float threshold of 75 million USD. Comparing exempted firms (the "control" group) with firms that were obliged to disclose the pay ratio (the "treatment" group) allows us to isolate the impact of the disclosure regulation.

Ex-ante, the consequences of the mandated disclosure of the pay ratio are unclear as contradicting predictions have been put forth by proponents and opponents of the mandate. Proponents of the rule argue that investors are interested in pay inequality within firms and that the disclosure of the pay ratio helps investors in their advisory vote on executive compensation (e.g. AFL–CIO, 2017). Moreover, the proposed rule could curb CEO pay because investors are concerned about a high pay ratio as it negatively impacts the company's public perception (Kelly and Seow, 2016). This is in line with the Managerial Power view of Bebchuk, Fried and Walker (2002), who postulate that outrage constraints limit the extent to which executives can extract rents. Investors could also be concerned by a high pay ratio as Equity Theory predicts pay disparity to cause resentment among employees, which could negatively impact firm performance (Akerlof and Yellen, 1988, 1990).

In contrast, opponents of the pay ratio argue that the regulation imposes heavy costs on firms and results in no notable benefits for firms (e.g. Chamber of Commerce, 2014). According to the opponents, investors are not interested in the pay ratio because it is a meaningless figure that is not comparable across firms and over time. In addition, opponents contend that shareholders will not use the pay ratio when voting on executive compensation as the CEO and employee labor markets differ substantially, and a comparison of pay in both markets is not meaningful. Opponents also state that shareholders might not be opposed to a high pay ratio to begin with. According to Tournament Theory, greater pay disparity increases the value of a promotion, leading to increased effort and better results (Green and Stokey, 1983; Lazear and Rosen, 1981).

For a sample of 660 firm-observations from December 2015 to August 2018, we find that the pay ratio disclosure rule does not affect total CEO compensation. Moreover, firms do not respond to the pay ratio disclosure rule by altering the composition of their CEO's pay by strengthening or weakening its link to performance. Hence, our results suggest that the mandatory pay ratio disclosure does not change executive pay.

Analyzing investor responses in a sample of 354 firms, we do not observe a statistically significant change in support on executive pay in shareholder advisory votes on executive compensation (say-on-pay) for firms that are required to disclose the pay ratio compared to exempted firms. Examining the change in abnormal Google Search volume around the filing of the proxy statement of 89 firms, we also do not observe a change in investor attention for firms required to disclose the pay ratio compared to firms that are not required to do so. Thus, we find no evidence that the disclosure of the pay ratio influences investor behavior.

Our results stay insignificant in a battery of robustness tests, including a two-stage least square instrumental variables approach that accounts for potential manipulation of the public float in order to avoid compliance with the disclosure law. Overall, these results are inconsistent

with the main goals of the supporters of the pay ratio, but more in line with the position of the opponents of the pay ratio, who state that the pay ratio disclosure provides no material information to investors and does not decrease executive pay.

Next, we exploit a sudden rule change to analyze how firms that have disclosed the pay ratio in the first year of compliance react to a regulatory relief of disclosure. On June 28, 2018, the SEC raised the public float threshold to qualify for smaller reporting company status from 75 million USD to 250 million USD. The rule has expanded the number of firms which qualify as smaller reporting company. As a consequence, a significant number of firms that were obliged to disclose the pay ratio in the first year of compliance became exempted in the second year. Our paper finds that in a sample of 161 firms, 54% continue to report their pay ratio voluntarily. This indicates that it does not seem costly to continue disclosing the pay ratio for the majority of firms.

Our paper is one of the first to study the causal consequences of the mandatory pay ratio disclosure rule. Contemporaneous papers examine how cross-sectional variation in the disclosed pay ratios impacts job search and change (Balsam and Liang, 2019), media coverage, shareholder SOP voting, and labor productivity (Boone, Starkweather and White, 2020) as well as firm value (Pan et al., 2020). We extend this research by assessing the causal effects of introducing the pay ratio disclosure (as opposed to the effect of different values of the pay ratios themselves). We contribute to a better understanding of the consequences of the pay ratio disclosure by showing that the disclosure rule does not change investor behavior or CEO pay. Moreover, we provide novel evidence that a majority of firms continue to disclose the pay ratio voluntarily after a relief from mandatory disclosure.

Johnson (2018), Chang et al. (2019), and Irlbeck (2019) examine the impact of the pay ratio on executive compensation. While Johnson (2018) examines changes of CEO pay following the SEC's "Proposing Release" and "Final Release" of the pay ratio rule, our paper

focuses on the years around the actual disclosure of the pay ratio. Chang et al. (2019) show declines in total pay and pay-for-performance sensitivity for CEOs relative to a control group of chief financial officers (CFOs). Irlbeck (2019) documents increases in total pay and performance-based compensation for CEOs relative to a control group of named executive officers (NEOs). Our study differs from these papers as our identification strategy is based on a regression discontinuity design that compares firms that are obliged to disclose the pay ratio to firms that are exempted from disclosure. Additionally, we examine the consequences of the pay ratio disclosure on investor behavior, and our identification strategy allows us to assess whether firms respond to a sudden relief from being mandated to disclose the pay ratio.

Our paper also adds to the compensation disclosure literature. Mas (2017) shows that mandatory pay disclosure reduces the compensation of city-managers in the public sector. Jensen and Murphy (1990) as well as Kaplan (2013) suggest that mandatory executive compensation disclosure could trigger a populist response resulting in a decline in CEO pay in the private sector. Inconsistent with these predictions, empirical literature finds that mandatory compensation disclosure does not reduce executive pay in the private sector (e.g. Gipper, 2017; Hermalin and Weisbach, 2012; Lu and Shi, 2018; Mas, 2016; Murphy, 2012; Park, Nelson and Huson, 2014). We complement this literature by documenting that mandatory disclosure does not reduce CEO compensation in the private sector even in a setting in which the compensation disclosure has a high public salience. In doing so, our paper contributes to the policy debate over the effects of disclosure regulation (e.g. Gomes, Gorton and Madureira, 2007; Greenstone, Oyer and Vissing-Jorgensen, 2006; Leuz and Wysocki, 2016; Murphy and Jensen, 2018).

1. Institutional background and hypothesis development

1.1 Pay ratio rule

The CEO pay ratio disclosure rule requires companies to disclose (1) the median of the annual total compensation of the company's employees, except the CEO, (2) the annual total compensation of the CEO, and (3) the ratio of those two amounts (SEC, 2015). Since the annual total compensation of the CEO is already disclosed by companies, the public gains new information about the compensation of the median employee at a firm.

The SEC announced the final rule on August 5, 2015 (Table 1 provides a timeline of the rule). Passing the final rule took an unusually long time. The preliminary pay ratio rule was released two years before the final rule, and Section 953(b) of the Dodd-Frank legislation, which mandates a CEO-employee pay ratio, became law three years prior to the preliminary rule.

Mandating firms to disclose the pay ratio was controversial. Both the preliminary and final rules were passed by a 3-2 vote at the SEC and were surrounded by long political debates (SEC, 2013). After the preliminary rule, the SEC received more than 287,400 comment letters in support or opposition to the mandate, which represents a level of public engagement not experienced by the SEC before (SEC, 2015). Moreover, between the becoming law of Section 953(b) of the Dodd-Frank Act and the effectiveness of the ruling, five separate bills were introduced in the US House of Representatives to repeal the pay ratio rule (Bank and Georgiev, 2019). The rule has resulted in high media coverage as all large media outlets have reported about firms' pay ratios. Most notably, Bloomberg has launched an online tracker that permits the public to search pay ratios of specific companies (Melin, Perry and Zhao, 2019).

Firms are obliged to disclose the pay ratio following the first full fiscal year beginning on or after January 1, 2017 (SEC, 2015). This means that firms with December 2017 or later fiscal year-ends are required to disclose the pay ratio in 2018.

The SEC has not provided specific guidance on where the ratio should be disclosed. While it should be a part of a company's annual proxy statement (Def 14a), firms can decide whether it is in the Compensation Discussion and Analysis or any other section in that document. The disclosure rule also gives firms significant discretion in calculating the pay ratio. This includes leeway in determining the median employee, e.g. firms may use statistical sampling and can decide on which compensation figure to use. Companies also have flexibility in providing additional information, such as supplementary ratios or disclaimers about the ratio not being comparable to other firms (Jung et al., 2018, provide a summary of disclosure leeway for firms). Commentators have argued that this flexibility could reduce the usefulness of the disclosure, but the SEC stresses that all firms need to disclose in detail their methodology for calculating their pay ratio and contends that the ratio provides material company-specific information to investors (SEC, 2015).

1.2 Hypothesis development

The consequences of the mandatory disclosure of the pay ratio are uncertain as contradicting predictions have been made by proponents and opponents of the rule. Supporters and opponents of the pay ratio rule debate how the disclosure of the pay ratio affects the level and the composition of executive pay.

Supporters of the pay ratio rule argue that executive pay is excessive and that the disclosure of the pay ratio could reduce the level of CEO pay (e.g. AFL-CIO, 2017). These arguments are consistent with the academic literature arguing that managers influence their pay by having power over the board of directors (Bebchuk and Fried, 2003; Goergen and Renneboog, 2011).

In this literature, a limit to the level of executive pay is the amount of outrage the compensation creates (Bebchuk, Fried and Walker, 2002).

The pay ratio disclosure could increase the amount of outrage of the CEO compensation. Prior literature finds inequality aversion and fairness concerns of the public (Fehr, Goette and Zehnder, 2009; Fehr and Schmidt, 1999). Consistent with this, experiments by Kelly and Seow (2016) show that high pay ratios raise negative perceptions about the fairness of CEO pay. Investors could also react negatively to the pay ratio disclosure as high pay inequality could signal managerial rent-extraction and corporate governance problems (Bebchuk and Fried, 2003; Core, Holthausen, and Larcker, 1999) or since pay inequality could have a negative impact on firm performance (Card et al., 2012; Breza, Kaur and Shamdasani, 2018; Green and Zhuo, 2019; Rouen, 2020; Shaw, 2015; Wade, O'Reilly III and Pollock, 2006). To avoid possible outrage following the pay ratio disclosure, compensation committees could, thus, lower executive pay.

Opponents of the pay ratio rule argue that executive pay is not excessive and that the disclosure of the pay ratio could have no impact on the level of CEO pay (e.g. Chamber of Commerce, 2014). These arguments are in line with the academic literature arguing that executive compensation is based on efficient contracts determined in an efficient CEO labor market (Kaplan and Rauh, 2010). In this literature, the CEO and workers compete in very different markets and their pay is not linked (Edmans, 2017), and hence the pay ratio rule provides no material new information for evaluating CEO pay. As a consequence, compensation committees might not change executive pay.

Opponents of the pay ratio rule also contend that governmental intervention in the competitive equilibrium of executive compensation can have unintended consequences (Murphy and Jensen, 2018). Such unintended consequences occurred in prior regulations. For example, the introduction of say-on-pay (Iliev and Vitanova, 2019) increased the turnover risk

for the CEO. As the CEO needed to be compensated for this higher risk, the new regulations resulted in an increase in CEO pay.

For the disclosure of the pay ratio, an unintended consequence could be an increase in executive pay since pay disparity could improve firm performance (Banker, Bu and Mehta, 2016; Cheng, Ransinghe and Zhao, 2017; Faleye, Reis and Venkateswaran, 2013; Mueller, Ouimet and Simintzi, 2017). The positive relationship between firm performance and pay inequality is supported by Tournament Theory, which posits that greater pay disparity increases the value of a promotion, leading to increased effort and better results (Green and Stokey, 1983; Lazear and Rosen, 1981).

Irrespective of whether firms alter the level of CEO compensation, performance-linked pay could change. Shareholders are generally more interested in aligning executive pay with company performance than in the level of executive compensation (Myers and Sevier, 2019). If shareholders are concerned about the disclosure of the pay ratio, compensation committees could, thus, gain shareholder approval by increasing pay-for-performance sensitivity without changing the level of CEO pay. In contrast, if shareholders are not interested in the disclosure of the pay ratio, we expect no change in neither the level of CEO pay nor performance-linked pay.

Based on the arguments listed above, our Hypothesis 1 is as follows (in alternative form):

H1: The disclosure of the pay ratio alters executive compensation.

Opponents and supporters of the pay ratio rule also debate whether the disclosure of the pay ratio affects investors. Proponents of the rule argue that investors are interested in pay inequality within firms and that the pay ratio provides material information to investors. Thus, opponents claim that investors are going to use the pay ratio when voting on executive compensation. Consistent with this, Crawford, Nelson and Rountree (2018) find that voting

dissent on say-on-pay proposals is significantly higher for U.S. commercial banks in the top pay ratio decile.

In contrast, opponents of the pay ratio argue that investors are not interested in the pay ratio and shareholders will not be influenced by the pay ratio when voting on executive compensation. According to the opponents, the pay ratio is a meaningless number since the CEO and employee labor markets differ substantially, and a comparison of the two does not yield any insights about executive pay. In addition, opponents argue that investors are not able to compare the ratio across firms as a multitude of factors, including workforce location and workforce composition in terms of part-time, full-time and seasonal workers, influence the pay ratio.

Based on the above discussion, our Hypothesis 2 is the following (in alternative form):

H2: The disclosure of the pay ratio affects investor behavior.

2. Research design

To cleanly identify the effect of the pay ratio disclosure regulation on firms, we would ideally compare a randomly assigned treatment group of firms that are required to disclose the pay ratio to a randomly assigned control group of firms that are not subject to the disclosure rule. We exploit the fact that the public float of the firm determines whether the firm is obliged to comply with the pay ratio disclosure regulation ("treatment") or whether the firm is exempted from the pay ratio disclosure regulation ("control").

Other studies have analyzed regulatory events by exploiting a regression discontinuity design based on the public float of firms. Iliev (2010) has introduced this identification strategy and finds that the Sarbanes-Oxley Act increases auditor costs, decreases the use of accruals and reduces the market value of firms. Albuquerque and Zhu (2019) do not find a reduction in corporate investment and other risk-taking activities as a result of Section 404 of the Sarbanes-

Oxley Act. Iliev and Vitanova (2019) document that the introduction of say-on-pay increases CEO compensation.

The public float is the number of common shares held by non-affiliates multiplied by the market price. It is recorded as of the end of the second quarter of the fiscal year (SEC, 2018). For firms with a fiscal year-end on December 31, 2017, the public float is, thus, recorded as of June 30, 2017. The public float determines the filer status of the company, which in turn determines whether a firm needs to comply with the CEO pay ratio rule. Until September 2018, companies qualified for "smaller reporting company" status if their public float is less than 75 million USD (SEC, 2018). Once a company crosses the 75 million USD threshold, a company is an "accelerated filer". A company can re-gain smaller reporting company status if the public float of the company drops below 50 million USD. Smaller reporting companies are exempted from complying with the pay ratio disclosure rule, while accelerated filers are not.

As a consequence, firms with a public float below 50 million USD are not required to disclose the pay ratio, firms with a public float above 75 million USD are obliged to disclose the pay ratio, and firms with a public float between 50 and 75 million USD only need to disclose the pay ratio if they have had a public float of above 75 million USD in the past.

There are two important exceptions to the rules listed above. First, "emerging growth companies" are also exempted from disclosing the pay ratio. Firms qualify for emerging growth company status if they have had their initial public offering (IPO) in the previous five years, have annual revenue of less than 1.07 billion USD, have issued less than 1 billion in convertible debt, and have a public float of less than 700 million (SEC, 2017). Second, companies that lose their smaller reporting company status or their emerging growth company status have a one-year transition period before being obliged to comply with disclosing their pay ratio. This means that smaller reporting companies or emerging growth companies in the fiscal year 2016 are

exempted from disclosing the ratio in the year 2018 – even if they are not smaller reporting companies or emerging growth companies anymore in the year 2017 (SEC, 2015).

As a consequence of these exceptions, there might be firms which in their latest fiscal year report a public float of above 75 million USD, but are not obliged to disclose the pay ratio. Although these firms are not required to disclose the pay ratio, the exemption is only temporary: if there is no change in public float, these firms are required to disclose their pay ratio in subsequent years. Hence, it is uncertain whether these firms belong to the control group or also receive a treatment effect. Thus, in our main model we exclude these firms from our sample, allowing us to have clean rule cut-offs at 50 million and 75 million USD in public float. In robustness tests, we include these firms as part of the control group.

We employ a regression discontinuity design to mitigate potential omitted variable bias by assessing the impact of the pay ratio rule on a sample of firms around the public float threshold of 75 million USD. Comparing exempted firms (the control group) with firms that were obliged to disclose the pay ratio (the treatment group) allows us to isolate the impact of the disclosure regulation on executive compensation, say-on-pay vote outcomes and investor attention. We run separate regressions for the years 2015, 2016 and 2017. This enables us to identify whether any changes to our dependent variables occur in anticipation of the disclosure of the pay ratio (2015 and 2016) or as a result of the disclosure of the pay ratio (2017).

Following Iliev and Vitanova (2019), we estimate the following regression model:

$$Y_{i,t} = \beta_0 + \beta_1\, Affected_{i,t} + \beta_2\, PFL_{i,t} + \beta_3\, PFL_{i,t} * \left(PFL > 75\ mio\ USD_{i,t}\right) + \beta_4\, PFL_{i,t} * \left(PFL < 50 mio\ USD_{i,t}\right) + \gamma X_{i,t} + \varepsilon_{i,t} \quad (1)$$

In Equation (1), *Affected* is an indicator variable equal to 1 for firm *i* in the year 2017 if the firm meets the public float criteria that oblige the firm to disclose the pay ratio for the year 2017. The *Affected* indicator variable is also equal to 1 for firm *i* in the year 2015 and in the year 2016 if the firm meets the public float criteria that results, under the assumption of no change in

public float, in the firm having to disclose the pay ratio for the year 2017. We control for the forcing variable, *Public Float* (*PFL*). We also take into account differential slopes of the disclosure rule cut-offs by interacting the public float with a dummy variable equal to 1 if the public float is above 75 million USD (*PFL*(PFL>75mio USD*)) and by interacting the public float with a dummy variable equal to 1 if the public float is below 50 million (*PFL*(PFL<50mio USD*)). In addition, we include a vector of firm-level controls (*X*) that includes variables related to CEO- and governance characteristics (*CEO Chairperson*, *CEO Age*, *CEO Tenure*, *CEO Ownership*, *Management Ownership* and *Staggered Board*), firm performance (*ROA*, *Firm Return*, *Sales Growth*) and firm characteristics (*Total Assets*).

An essential assumption of the regression discontinuity design is that treatment and control differ only by the treatment itself (Lee and Lemieux, 2010). Hence, an important empirical choice is the bandwidth of firms which are included in the sample (Imbens and Kalyanaraman, 2012). On the one hand, a small bandwidth implies that treatment and control firms are more likely to be very similar. On the other hand, a small bandwidth also means that the sample size is smaller. Following Iliev and Vitanova (2019), we choose to examine firms with a public float between 40 and 110 million USD in the year 2016 (the year before the rule became effective). In robustness tests, we also use a wider (public float between 35 and 115 million USD) and narrower (public float between 45 and 105 million USD) bandwidth.

Selection problems could be a concern for our research design if firms avoid the pay ratio regulation (Heckman, 1997). Figure 1 displays the public float of all Compustat firms with a market capitalization of less than 500 million USD. Panel A and Panel B show that there is no change in the concentration of firms below the cut-offs of 50 million or 75 million USD, indicating that it is unlikely that there was manipulation of the public float to avoid compliance with the pay ratio disclosure rule. A density test based on Cattaneo and Escanciano (2017) confirms this interpretation.

As an additional step to mitigate a potential manipulation of the public float, we run an alternative model based on a two-stage least square instrumental variables approach similar to Iliev (2010) as well as Albuquerque and Zhu (2019). Specifically, we run the following two-stage model with a two-stage least squares estimate:

$$Affected = \beta_0 + \beta_1\, Affected\ before\ Final\ Rule_{i,2015} + \beta_2\, PFL_{i,2015} + \beta_3\, PFL * \left(PFL > 75\ mio\ USD_{i,2015}\right) + \beta_4\, PFL_{i,2015} * \left(PFL < 50 mio\ USD_{i,2015}\right) + \gamma X_{i,2015} + u_{i,2015} \quad (2)$$

$$Y_{i,t} = \beta_0 + \beta_1\, \widehat{Affected}_{i,t} + \gamma X_{i,t} + \varepsilon_{i,t} \quad (3)$$

In the first stage (Equation (2)) we use the filing status before the release of the final pay ratio rule (*Affected before Final Rule*) to predict the *Affected* status in the year 2016 and 2017. The first-stage regression includes all control variables of the second-stage regression as well as the public float terms (Imbens and Lemieux, 2008). In the second stage (Equation (3)), we include the predicted $\widehat{Affected}$ status from the first-stage regression along with the same control variables as in Equation (1) to estimate the impact on our dependent variables.

3. Data

3.1 Sample construction

Table 2 Panel A details the first steps of our sample construction. We first download all firms with a fiscal year-end between December 2015 and August 2018 from Compustat via University of Pennsylvania's Wharton Research Data Services (WRDS). In our download, we exclude firms which are not incorporated in the US as they are not subject to the pay ratio rule.

We assign the observations to years based on fiscal year-ends. Observations with a fiscal year-end between December 31, 2017 and August 31, 2018 are assigned to the year 2011[1].

[1] Observations with fiscal year-end between September 30, 2018 and November 30, 2018 are not included as the threshold for smaller reporting company status has increased to 250 million USD for these observations (SEC, 2018). This rule change is further examined in Section 6.

These observations are the first for which the rule is effective. To have consistent year definitions, the year 2016 includes observations with a fiscal year-end between December 31, 2016 and November 30, 2017, and the year 2015 includes observations with a fiscal year-end between December 31, 2015 and November 30, 2016.

We then select our sample based on the bandwidth of the regression discontinuity design. Specifically, we include firms with public float between 40 and 110 million USD in 2016, the year before the rule became effective, in our sample. This results in an initial sample of 1,370 observations over the years 2015, 2016 and 2017 (Table 2 Panel A).

Table 2 Panel B summarizes the sample construction for the regressions involving executive compensation and say-on-pay. From the initial sample (1,370 observations), we drop observations for which we do not have compensation data. We require CEO compensation data for two full fiscal years. This leads us to exclude firms that do not report a CEO's pay for two consecutive fiscal years as a consequence of a CEO turnover. We drop observations if CEO pay is symbolic (e.g. 1 USD) or if CEO compensation includes a signing bonus, severance pay, or a bonus related to an IPO or acquisition. We also exclude observations if the difference of the sum of all the components of compensation and the total compensation figure is larger than 5%. We also exclude observations for which we do not have data for the independent variables used in the regressions.

Finally, as discussed in Section 2, there are firms which in their latest fiscal year report a public float of above 75 million USD but are not obliged to disclose the pay ratio. We exclude these observations by dropping observations that are emerging growth companies and have a public float above 75 million USD and by dropping, for the year 2017, observations when the firm transitions from smaller reporting company to accelerated filer, or when the firm is no longer an emerging growth company. For the regressions analyzing the effect on executive

compensation, we have a final sample of 660 observations over the years 2015, 2016 and 2017 (Table 2 Panel B).

For the regressions analyzing the impact on say-on-pay vote outcomes, we additionally drop observations for which we do not have data on voting outcomes. This is possible as emerging growth companies are exempted from requiring shareholders to have a non-binding vote on executive compensation. In addition, some firms do not hold the advisory vote each year but only every second or third year instead. Our final sample for these regressions includes 354 observations for the years 2015, 2016 and 2017 (Table 2 Panel B).

Table 2 Panel C summarizes the sample construction for the analysis of investor attention. As we assess the investor attention after the first disclosure of the pay ratio, we only include observations with a fiscal year-end between December 2017 and August 2018 from the initial sample.

After dropping observations without CEO compensation data, we search firms' ticker symbols in Google Search. We exclude firms that do not appear on the first page of the Google Search result as we consider this as an indication that the ticker symbol is ambiguous. Subsequently, we search the firms' ticker symbols in Google Trends. We drop observations with missing Google Search volume data. We also exclude observations if a portlet of Google Trends states that there is "not enough data", and we drop observations with missing proxy statements or missing independent variables.

As in the sample for the analysis of compensation, we also drop observations of firms that in their latest fiscal year report a public float of above 75 million USD, but are not obliged to disclose the pay ratio. Our final sample for the regressions analyzing the impact on investor attention consists of 89 observations for the year 2017 (Table 2 Panel C).

3.2 Variables

We hand-collect the public float, filer status (accelerated filer and smaller reporting company), and emerging growth company category status from a company's annual report (10-K) via the SEC's Electronic Data Gathering, Analysis, and Retrieval system (EDGAR).

For all regressions, we assign firms into treatment and control groups based on the rules discussed in Section 2 above. In the regressions, the *Affected* indicator variable takes the value of 1 if the company is obliged to disclose the pay ratio (treatment group). This includes firms that have a public float above 50 million and are accelerated filers. In contrast, the *Affected* indicator variable has the value 0 if the company is exempted from disclosing the pay ratio (control group). This includes firms that have a public float below 75 million and are smaller reporting companies or emerging growth companies.

For the analysis of compensation, we hand-collect the CEO compensation data from the Summary Compensation Table from proxy statements. The Summary Compensation Table reports the *Total Compensation* of the CEO and other executive officers, and breaks the *Total Compensation* down into its components: *Salary*, *Bonus*, *Stock Awards*, *Option Awards*, *Non-Equity Incentive*, *Change in Pension*, and *All Other Compensation*. We compute *Performance-Pay* as the sum of *Bonus*, *Stock Awards*, *Option Awards* and *Non-Equity Incentive* divided by the *Total Compensation*.

For the analysis of say-on-pay voting we hand-collect the voting results of the annual shareholder meetings from companies' 8-K filings via EDGAR. Following Iliev and Vitanova (2019), we calculate the *Support for Executive Pay* by dividing the "For" votes by the sum of "For", "Against", and "Abstain" votes cast in the advisory vote to approve the compensation paid to the company's named executive officers.

For the analysis of investor attention, we measure investor attention based on how frequent a firm's ticker symbol is searched in Google Search, the most popular search engine in the United States (Gao, Ren and Zhang, 2020). The search volume index shows the relative interest of a particular search term over a period in Google Search. The search volume index is always specific to a search term – which includes a name, a period and a region. For example, the search volume index of the search term "Jamba" for the period January 1, 2017 to December 31, 2018 in the region US, represents the weekly popularity of the search term "Jamba" over that period in the region US. The search volume index assigns a value of 100 to the most popular week, and values between 0 and 100 to the other weeks, with 50 indicating that the value was half as popular as during the peak. Since Google Trends uses a random sampling approach to calculate the search volume index, the search volume index of the same search query might be different from one day to another (Da, Engelberg and Gao, 2011). However, since Da, Engelberg and Gao (2011) find that there is no large difference between the search volume index when downloaded at different times, we believe that this random sampling approach should not impact the results of our analyses.

Following prior literature (Da, Engelberg and Gao, 2011; Drake, Roulstone and Thornrock, 2012), we use the company stock ticker symbol (e.g. "JMBA" for Jamba Inc.) as the search term that represents the interest of investors in the company. Using the company name as the search term has two main problems (Da, Engelberg and Gao, 2011). First, there are often multiple variations for searching the company via its name (e.g. "Jamba", "Jamba Inc", "Jamba Juice" or "Jamba Juice Company"). Second, the company name might be searched in Google for reasons unrelated to investing (e.g. consumers looking for the closest Jamba Juice store).

We download the Google Search volume index from the Google Trends API in R. Google only provides the weekly search volume index. Hence, we first download the weekly search volume index from January 1, 2017 to December 31, 2018. Then, we divide the period into

quarters and download the daily search volume index for each quarter. Next, we combine the daily search volume index of each quarter and the weekly search volume index to obtain the daily search volume index from January 1, 2017 to December 31, 2018 (Johansson, 2014).

We apply the logarithm to the daily search volume index as the variable has a logarithmic distribution, and we standardize the variable to have values between 0 and 100. Subsequently, following Drake, Roulstone and Thornrock (2012), we calculate *Abnormal Search* as the daily search volume index less the average daily search volume index for the same weekday over the previous 10 weeks divided by the average daily search volume index for the same weekday over the previous 10 weeks. We calculate the *Change in Abnormal Search* around the filing date of the proxy statement by subtracting the *Abnormal Search* of the filing date of the proxy statement published in 2018 from the *Abnormal Search* of the filing day of the proxy statement published in 2017.

From Compustat, we obtain the input to calculate the following dependent variables used in our regressions: the logarithm of *Total Assets* (Compustat item AT); the logarithm of *Sales Growth* (SALE divided by lagged SALE), and the return on assets, *ROA* (IBC divided by AT).

We download from the Center for Research in Security Prices (CRSP) from WRDS the monthly returns to calculate the 12-month stock market return of firms (*Firm Return)*. We merge Compustat and CRSP via the CRSP/Compustat Merged linking table from WRDS.

In addition, we manually collect from the proxy statements CEO characteristics (whether the CEO is a chairperson (*CEO Chairperson)*, *CEO Age*, and *CEO Tenure*) as well as governance characteristics (*CEO Ownership*[2], *Management Ownership*, and whether all board

[2] Some companies report ownership as of both voting power and economic power. In the spirit of Dlugosz et al. (2006), we use voting power in those cases.

members do not stand for re-election every year (*Staggered Board*)). We winsorize all continuous, non-logarithmic variables at 1% and 99%[3].

3.3 Descriptive statistics

Table 3 Panel A presents summary statistics for the firms in the compensation regressions. Firms have assets of around 424 million USD[4]. Sample firms experience a positive *Firm Return* with a mean of 6%. While sales increase by around 19% for firms, *ROA* is negative over the period. CEO characteristics (for around 35% of the firms the CEO has a dual CEO-chairperson role, the average CEO is around 59 years old and has tenure of 11 years), firm ownership of the CEO (around 8%) and management (around 21%), as well as the presence of staggered boards (slightly less than half of the firms) are in line with prior research (Iliev and Vitanova, 2019). The firm characteristics of the sample of the investor attention regressions (Table 3 Panel B) are similar to the ones of the compensation regressions.

Tables 3 Panel C shows that treatment and control firms are similar in the year before the rule became effective. Thus, an important assumption of the regression discontinuity design is not violated (Atanasov and Black, 2016). Treatment and control firms differ at the 10% significance level by their *Total Assets*. This is plausible given that *Total Assets* is closely related to the forcing variable, *Public Float*, as both variables proxy for firm size. Although *CEO Age* is also different between treatment and control firms, the economic magnitude of this difference is small (less than two years) and the difference is only borderline statistically significant. The other variables show no statistically significant difference between the treatment group and the control group.

[3] In robustness tests we do not winsorize any variables.
[4] The market capitalization (untabulated) is around 115 million USD. As a comparison, in 2016 the smallest market capitalization in the Russell 3000 index was 132 million USD. In our sample around 25% of all firms fall above this number.

Table 3 Panel D shows that the mean compensation of the CEOs in our sample is 1.1 million USD and *Salary* (0.4 million USD) is the largest component of compensation. Performance-pay related compensation in the form of *Bonus, Stock Awards, Option Awards,* and *Non-Equity Incentive* amount to a mean of 39%. The shareholder advisory vote supporting the company's executive compensation has a mean slightly below 88%. Looking at the distribution, 97% of firms receive more than 75% support and only 8% of firms receive less than 50% support. These numbers are comparable to the approval percentage at Russell 3000 firms (Semler Brossy, 2018).

4. Does the disclosure of the pay ratio alter executive compensation?

4.1 Change in total compensation

We run regression discontinuity specifications with the dependent variable *Change in Total Compensation* for three separate years (2015, 2016 and 2017) to examine Hypothesis 1. Our focus is on *Total Compensation* of the CEO as this figure is reported in the summary compensation table in the proxy statement and is also used to calculate the CEO pay ratio. We run two sets of regressions. In the base specification, we only include the treatment dummy (*Affected*), as well as controls for the public float and differential slopes of the disclosure rule cut-offs at a public float of 50 million and 75 million USD. In the full specification, we add the control variables specified in Section 2 (Equation (1)).

We present the results in Table 4 Panel A. The estimations show no significant effect for the variable of interest (*Affected* dummy) in any of our specifications. As expected, we do not see a change in CEO compensation in the year 2015, prior to the announcement of the final rule (Columns (1) and (2)). Affected firms do not alter their CEO pay in the year 2016 in anticipation to the mandatory disclosure of the pay ratio (Columns (3) and (4)). In the year 2017, firms that have to disclose the pay ratio also do not change their CEO pay compared to firms which are exempted from disclosing the pay ratio (Columns (5) and (6)). The results do not depend on

using the base specification with public float controls (Columns (1), (3) and (5)) or the full specification with additional control variables related to CEO- and governance characteristics (Columns (2), (4) and (6)). The findings that firms do not change their executive pay is consistent with arguments of opponents of the rule. In contrast, the goal of supporters to reduce executive pay does not seem to have been achieved with the pay ratio disclosure.

In Table 4 Panel B, we run various robustness tests for the full specification model to make sure that the results do not depend on research design implementation choices. In the first test, we change our outlier treatment and run the regressions without winsorizing any variable. The coefficient on the *Affected* dummy stays insignificant for all three years (Columns (1), (2), and (3)). In the second test, we increase our control group by including firms with a public float above 75 million USD that are exempted from disclosing the pay ratio (as discussed in Section 2). The coefficient on the *Affected* dummy stays insignificant for all three years (Columns (4), (5), and (6)). In the third test, we alter our bandwidth of firms which are included in the regressions (as discussed in Section 2). When using a wider public float bandwidth (35 to 115 million USD), the coefficient on the *Affected* dummy stays insignificant for all three years (Columns (7), (8), and (9)). The coefficient on the *Affected* dummy also stays insignificant for all three years (Columns (10), (11), and (12)) when we use a narrower public float bandwidth (45 to 105 million USD).

As an additional robustness test (Table 4 Panel C), we run an alternative model based on a two-stage least square instrumental variables (IV) approach that accounts for potential manipulation as described in Section 2 (Equations (2) and (3)). The results of the first-stage of the regressions of the years 2016 (Column (1)) and 2017 (Column (3)) show that *Affected before Final Rule* predicts the *Affected* status in the year 2016 and 2017 (significant at less than 1%). This indicates that the instrument is relevant. In the second-stage of the regressions of the year 2016 (Column (2)) and 2017 (Column (4)), the variable of interest remains insignificant for

both years, indicating that there is no significant statistical difference in the *Change in Total Compensation* for treated and control firms. Overall, the results from the robustness tests support our prior finding that, consistent with arguments of opponents of the rule, the mandatory disclosure of the CEO pay ratio does not seem to alter executive compensation (H1) in terms of the level of CEO pay.

4.2 Change in performance-pay

Even if there is no change in the level of CEO compensation, performance-pay could change as a result of the pay ratio disclosure regulation. In Table 5, we show the results of regressions with *Change in Performance-Pay* as the dependent variable. Following Iliev and Vitanova (2019), we compute *Performance-Pay* as the sum of *Bonus*, *Stock Awards*, *Option Awards*, and *Non-Equity Incentive* divided by the *Total Compensation.* As before, we run the base specification and the full specification for the years 2015, 2016 and 2017.

Affected firms do not alter the performance-linked pay of the CEO in the year 2015, the year prior to the announcement of the final rule (Columns (1) and (2)). In the year 2016, the year prior to the mandatory disclosure of the pay ratio, treated firms have decreased their *Performance-Pay* in the base specification (Column (3)). This effect, however, is only borderline significant at the 10% level, and disappears in the full specification with additional control variables related to CEO- and governance characteristics level (Column (4)). In the year 2017, the first year of the mandatory disclosure of the pay ratio, firms that have to disclose the pay ratio do not change their performance-linked pay compared to the control group of firms that are exempted from disclosing the pay ratio (Columns (5) and (6)).

To bolster our results, we run the same robustness and IV tests as in the prior compensation regressions. In all tests, the *Affected* variable is insignificant in every year (untabulated). We also run additional tests (untabulated) to make sure that our findings do not depend on our definition of performance-pay. In particular, we estimate whether any component of

compensation has changed as a percentage of compensation (e.g. *Bonus*/*Total Compensation*, *Stock Awards*/*Total Compensation*; *Option Awards*/*Total Compensation*). We do not find a statistically significant change in any of these components.

From these tests, we conclude that firms do not link their CEO pay closer to firm performance as a response of the pay ratio disclosure rule. As firms also do not change their level of executive pay, we reject Hypothesis 1 (the disclosure of the pay ratio alters executive compensation). Finding no change in executive pay supports the view of the opponents of the pay ratio who argue that executive pay is not excessive and that the disclosure of the pay ratio has no impact on CEO pay.

5. Does the disclosure of the pay ratio affect investor behavior?

5.1 Change in support for executive pay

We expand our analysis of the consequences of the pay ratio disclosure by looking at the response of investors. We test Hypothesis 2 by examining whether firms experience a change in their advisory vote on executive compensation (Section 5.1) and whether investors pay additional attention to firms that disclose the pay ratio (Section 5.2).

In Table 6 we run separate regressions with the *Change in Support for Executive Pay* as the dependent variable. As in the previous regressions, we run both our baseline and our full specifications for three separate years (2015, 2016 and 2017).

We do not observe a statistically significant *Change in Support for Executive Pay* for treated firms compared to control firms prior to the announcement of the final rule (Columns (1) and (2)). Affected firms also do not experience a *Change in Support for Executive Pay* prior to the mandatory disclosure of the pay ratio (Columns (3) and (4)) or after the mandatory disclosure of the pay ratio (Columns (5) and (6)). The results do not depend on using the base specification with public float controls (Columns (1), (3), and (5)) or the full specification with

additional control variables related to CEO and governance characteristics (Columns (2), (4), and (6)). In addition, we run the same robustness and IV tests as in the prior compensation regressions with no change in the insignificance of *Affected* (untabulated).

We can conclude that shareholders do not alter their advisory vote on executive pay as a response to the pay ratio disclosure rule. This indicates that pay inequality within firms does not seem to be material to investors when evaluating CEO pay. Consequently, the disclosure of the pay ratio does not affect the behavior of investors (H2) in terms of say-on-pay votes.

5.2 Change in investor attention

In Table 7, we present the results of regressions with the *Change in Abnormal Search* as the dependent variable. Compared to the previous regressions, we run the investor attention regressions only for the year 2017 since we do not expect a change in investor attention prior to the release of the pay ratio. Following prior literature (Da, Engelberg and Gao, 2011; Drake, Roulstone and Thornrock, 2012), we also do not include variables related to CEO- and governance characteristics.

Both the base specification (Column (1)) and the full specification (Column (2)) show no statistically significant effect on the *Affected* variable. Firms that disclose the pay ratio do not experience an increase in investor attention compared to firms that do not disclose the pay ratio. This is consistent with the arguments of the opponents of the pay ratio who state that most investors are not interested in pay inequality within firms. We run various robustness tests to substantiate our results. First (Column (3)), we do not winsorize any variable. Subsequently (Column (4)), we increase our control group by including firms with a public float above 75 million USD that are exempted from disclosing the pay ratio. Finally (Columns (5) and (6)), we run regressions with a wider (35 to 115 million USD) and narrower (45 to 105 million USD) public float bandwidth. In all robustness tests, the *Affected* variable remains statistically insignificant.

Concluding, although the pay ratio rule has received a lot of attention from the media and politicians, investors do not seem to pay more attention to firms disclosing the pay ratio compared to firms that do not disclose the pay ratio. Taken together with firms not experiencing a change in their advisory vote on executive pay, we reject Hypothesis 2 (the disclosure of the pay ratio affects investor behavior). This supports the view of the opponents of the pay ratio who stress that investors are not interested in the pay ratio.

Rejecting Hypothesis 2 is consistent with rejecting Hypothesis 1 since a lack of change in the say-on-pay voting outcomes and investor attention could explain why firms do not alter the level and the performance related composition of CEO pay. Since the disclosure of the pay ratio does not change investor behavior, firms might not have been pressurized to alter their CEO pay.

6. Smaller reporting company threshold rule change

As explained in Section 2, firms are exempted from disclosing the pay ratio if they file as smaller reporting companies. Until September 2018, companies generally qualified as a smaller reporting company if their public float was less than 75 million USD. However, on June 28, 2018, the SEC significantly altered the rule determining whether a company files as a smaller reporting company. Specifically, effective as of September 10, 2018, firms qualify as a smaller reporting company if their public float is below 250 million USD.

This smaller reporting company threshold rule change considerably expanded the number of smaller reporting companies. As a consequence, a number of firms that were obliged to disclose the pay ratio in the first year of compliance suddenly became exempted from disclosing the pay ratio in the second year. We exploit this rule change to examine how firms respond to a sudden relief from being mandated to disclose the pay ratio.

Our analysis starts with the firms from our initial sample (i.e. firms with a public float between 40 and 110 million USD in 2016) that have disclosed the pay ratio for the year 2017, the first year of the effectiveness of the pay ratio disclosure rule (Figure 2). We then focus on the 130 firms which subsequently file for the year 2018 with a public float below 250 million USD. These firms were obliged to disclose the pay ratio in the first year of compliance but were suddenly exempted from disclosing the pay ratio in the second year. Our analysis of these firms' filings reveals that the majority of these 130 firms (70 firms) continues to disclose the pay ratio voluntarily. In contrast, only 60 firms make use of the regulatory relief and stop reporting the pay ratio.

A potential explanation for why firms continue to disclose the pay ratio is that they are not aware of the SEC changing the threshold that qualify firms to file as smaller reporting company. However, we can reject this suggestion since most firms (77%) have followed the new threshold rule by correctly changing their filing status. An alternative explanation is that firms behave opportunistically by reporting improvements in their pay ratio. We do not find evidence for this conjecture as the pay ratio in the second year of disclosure (38.9) is not different, in terms of both economic and statistical significance, compared to the pay ratio in the first year of disclosure (35.1). We also find no support for the notion that firms continuing to disclose the pay ratio have a low ratio to begin with. In fact, there is no economically or statistically significant difference in the pay ratio in the first year of compliance between the firms which continue (35.0) and the firms which stop reporting the figure (35.1). Thus, a likely explanation for firms continuing to disclose the pay ratio is that for a large number of firms the costs of continuing to disclose the pay ratio do not significantly outweigh the benefits.

7. Conclusion

The rule to require companies to disclose the ratio of the pay of the median employee to the compensation of the chief executive officer is one of the most controversial in the history

of the SEC. This paper examines the main consequences of the highly anticipated pay ratio disclosure. Our identification strategy exploits exemptions granted to firms based on their free float and, thus, isolates the impact of the disclosure regulation.

We find that the pay ratio disclosure rule does not affect the total compensation of the CEO and that firms do not change the performance-linked pay of the CEO. Our paper also finds that the disclosure of the pay ratio does not influence investor behavior. Investors do not seem to pay more attention to firms that have disclosed the pay ratio compared to firms that have not disclosed the pay ratio. Furthermore, firms experience no change in their advisory vote on executive compensation.

Overall, our findings support the view of the opponents of the pay ratio who argue that investors are not interested in the pay ratio and that the disclosure of the pay ratio has no impact on CEO pay.

Finally, we exploit a sudden rule change to analyze how firms that have disclosed the pay ratio in the first year of compliance react to a regulatory relief of disclosure. We find evidence that for the majority of firms the costs of continuing to disclose the pay ratio do not seem to be particularly high.

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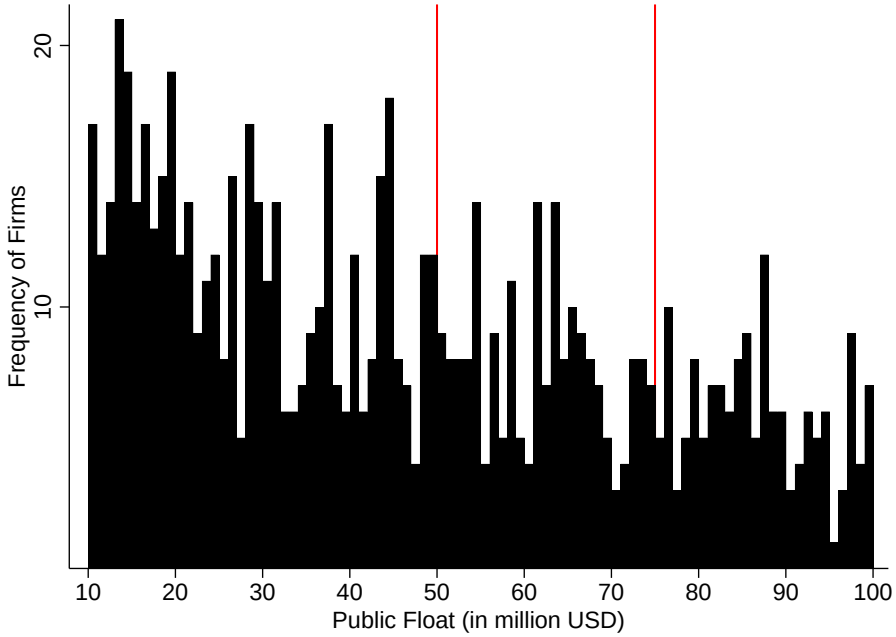
Figure 1. Public float

These figures plot the public float distribution of firms near the public float cut-offs of 50 and 75 million USD (Panel A for firms in 2016, Panel B for firms in 2017). The public float is the number of common shares held by non-affiliates multiplied by the market price at the second quarter end of the fiscal year. Firms are obtained from Compustat and have a market capitalization of less than 500 million USD.

Panel A: 2016 public float distribution of firms



Panel B: 2017 public float distribution of firms

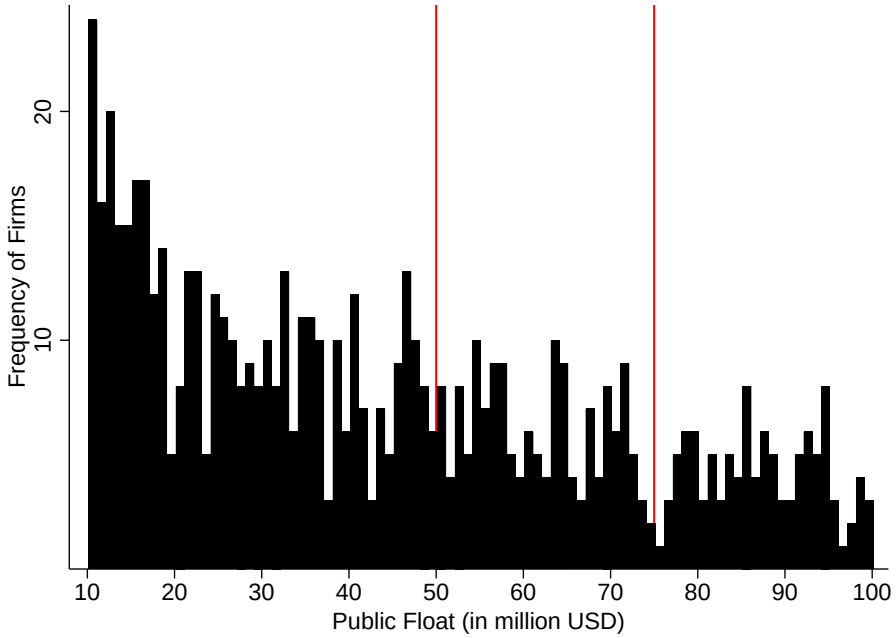


Figure 2. Smaller reporting company rule change

This figure illustrates whether firms continue to disclose the pay ratio after the smaller reporting company threshold rule change, effective September 10, 2018. The initial sample of disclosure firms consists of firms with a public float (PFL) between 40 and 110 million USD in 2016 (fiscal year-end between December 31, 2016 and November 30, 2017) that have disclosed the pay ratio for 2017 (fiscal year-end between December 31, 2017 and August 31, 2018), the first year of the effectiveness of the pay ratio disclosure rule. Subsequent refers to the year 2018 (fiscal year-end December 31, 2018 and August 31, 2019), the second year of the effectiveness of the pay ratio disclosure rule.

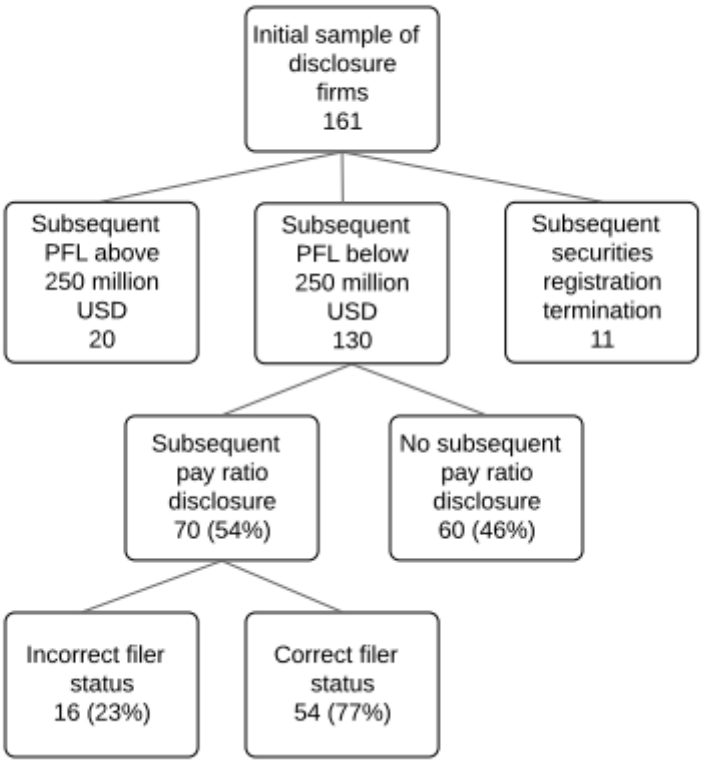


Table 1. Timeline of pay ratio rule

This table summarizes regulatory events related to the pay ratio rule as well as the implications of the events on the pay ratio disclosure for firms with a fiscal year-end in December.

Date	**Regulatory event**	**Disclosure of pay ratio (for firms with fiscal year-end December)**
July 21, 2010	Becoming law of Section 953(b) of the Dodd-Frank Act. The Dodd-Frank Act mandates companies to disclose the pay ratio. There was no imposed deadline on the pay ratio mandate.	
Sep 8, 2013	SEC "Proposing Release" of pay ratio. The compliance date for firms is Jan 1, 2017. Commentators were unsure whether smaller reporting companies are exempted from disclosing the pay ratio (SEC, 2013).	
Aug 5, 2015	SEC "Final Release" of pay ratio. The compliance date for firms is Jan 1, 2018 - a delay of one year versus the "Proposing Release". In the final release the SEC clarified that among others, smaller reporting companies, are exempted from disclosing the pay ratio (SEC, 2015).	
Jan 1, 2018	Pay ratio rule becomes effective.	Required of firms in their proxy statements released in 2018 unless firms qualify for an exemption.

Table 2. Sample construction

These tables illustrate the sample construction process. Panel A describes the initial observations, Panel B the compensation and say-on-pay observations and Panel C the investor attention observations.

Panel A: Initial observations

Sample	Change	Observations
All observations (obs.) in Compustat/CRSP merged database with fiscal year-ends between December 31, 2015 and August 31, 2018		13,061
Obs. with fiscal year-ends between December 31, 2015 and August 31, 2018 and a market capitalization of less than 500 million USD	*(7,040)*	6,021
Obs. with a public float between 40 and 110 million USD in 2016	*(4,651)*	1,370

Panel B: Compensation and say-on-pay observations

Sample	Change	Observations
Drop if no CEO compensation data	*(136)*	1,234
Drop if CEO turnover during the current and prior fiscal year	*(301)*	933
Drop if missing independent variables	*(121)*	812
Drop if symbolic pay, signing bonus, severance pay, bonus due to IPO or acquisition, or deviation >5% between total compensation and sum of components of compensation	*(19)*	793
Drop if emerging growth company with PFL > 75 million USD, transition from smaller reporting company to accelerated filer in 2017, or ceasing of emerging growth company status in 2017	*(133)*	660
Final sample compensation		**660**
Drop if no say-on-pay elections in 2017 and 2016	*(306)*	354
Final sample say-on-pay		**354**

Panel C: Investor attention observations

Sample	Change	Observations
Obs. with fiscal year-ends between December 31, 2017 and August 31, 2018	*(955)*	415
Drop if no CEO compensation data	*(57)*	358
Drop if no relevant results on the first page of Google Search	*(104)*	254
Drop if not enough data in Google Trends	*(78)*	176
Drop if no published proxy statements in 2018 and 2017	*(5)*	171
Drop if missing independent variables	*(60)*	111
Drop if emerging growth company with PFL > 75 million USD, transition from smaller reporting company to accelerated filer in 2017, or ceasing of emerging growth company status in 2017	*(22)*	89
Final sample investor attention		**89**

Table 3. Descriptive statistics

These tables report descriptive statistics. Panel A shows independent variables of the compensation sample for the years 2015, 2016 and 2017. Panel B displays independent variables of the investor attention sample for the year 2017. Panel C splits the compensation sample of the year 2016, the year before the rule became effective, into control and treatment group. Panel D provides descriptive statistics of the dependent variables related to the compensation and support for executive pay for the years 2015, 2016 and 2017. Control firms are exempted from the disclosure regulation, while treated firms are not. *Public Float* is the number of common shares held by non-affiliates multiplied by the market price at the second quarter-end of the fiscal year. *Total Assets* are the total assets (Compustat AT). *Sales Growth* is the logarithm of current sales (Compustat SALE) divided by lagged sales. *Firm Return* is the last 12-month stock return from CRSP. *ROA* is the return on assets calculated income before extraordinary items (Compustat IBC) divided by total assets (Compustat AT). *Staggered Board* is an indicator variable that equals 1 if all board members do not get re-elected together. *CEO Chairperson* is a dummy variable equal to 1 if the CEO is also the chairperson. *CEO Ownership* is the total stock ownership of the CEO. *Mgmt. Ownership* is the total stock ownership of the management and board members. *CEO Age* is the age of the CEO. *CEO Tenure* is the tenure of the CEO. *Total Compensation* is the total CEO compensation as reported in the Summary Compensation Table of the company's proxy statement. *Salary*, *Stock Awards*, *Option Awards*, *Bonus*, *Non-Equity Incentive*, Change in pension value and nonqualified deferred compensation earnings (*Change in Pension*) and *All Other Compensation* are also collected from the relevant fields of the Summary Compensation Table from the company's proxy statement. The performance-linked Pay (*Performance-Pay*) variable is the sum of the *Stock Awards*, *Option Awards*, *Bonus*, and *Non-Equity Incentive* divided by the *Total Compensation*. *Support for Executive Pay* is the "For" votes divided by the sum of "For", "Against," and "Abstain" votes cast in the advisory vote to approve the compensation paid to the company's named executive officers. The votes are collected from the company's 8-K. ***, **, * indicate the significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Compensation observations (all years)

	Units	Mean	SD	Obs.	Source
Public Float	Millions	82.40	52.85	660	10-K
Total Assets	Millions	424.01	495.29	660	Compustat
Sales Growth	Rate	0.04	0.47	660	Compustat
Firm Return	Rate	0.06	0.55	660	CRSP
ROA	Ratio	-0.10	0.41	660	Compustat
Staggered Board	Dummy	0.47	0.50	660	Def 14a
CEO Chairperson	Dummy	0.35	0.48	660	Def 14a
CEO Ownership	%	8.01	13.91	660	Def 14a
Mgmt. Ownership	%	20.55	19.15	660	Def 14a
CEO Age	Years	58.53	8.24	660	Def 14a
CEO Tenure	Years	11.37	9.33	660	Def 14a

Panel B: Investor attention observations (year 2017)

	Units	Mean	SD	Obs.	Source
Public Float	Millions	82.42	53.48	89	10-K
Total Assets	Millions	176.64	272.11	89	Compustat
Sales Growth	Rate	-0.01	0.70	89	Compustat
Firm Return	Rate	0.14	1.05	89	CRSP
ROA	Ratio	-0.32	0.47	89	Compustat

Panel C: Compensation observations (year 2016) split into control and treatment

	Control		Treatment		Diff.	p-value
	Mean	SD	Mean	SD		
Public Float	54.46	10.57	81.89	15.87	-27.43***	(0.000)
Total Assets	369.73	381.07	480.95	530.42	-111.23*	(0.058)
Sales Growth	0.06	0.48	-0.00	0.51	0.06	(0.375)
Firm Return	0.16	0.72	0.21	0.52	-0.04	(0.613)
ROA	-0.10	0.34	-0.09	0.40	-0.01	(0.839)
Staggered Board	0.48	0.50	0.45	0.50	0.03	(0.663)
CEO Chairperson	7.83	12.09	7.99	14.56	-0.16	(0.925)
CEO Ownership	0.37	0.48	0.33	0.47	0.04	(0.486)
Mgmt. Ownership	20.82	17.55	20.11	20.13	0.71	(0.768)
CEO Age	59.50	9.77	57.56	7.16	1.94*	(0.081)
CEO Tenure	12.40	10.28	10.85	8.57	1.56	(0.203)

Panel D: CEO compensation and support for executive pay (all years)

	Units	Mean	SD	Obs.
Total Compensation	USD	1,123,545	1,148,287	660
Salary	USD	446,131	198,765	660
Stock Awards	USD	254,289	831,370	660
Option Awards	USD	133,772	378,808	660
Non-Equity Incentive	USD	154,309	385,502	660
Change in Pension	USD	9,583	43,730	660
All Other Compensation	USD	52,789	164,640	660
Performance-Pay	%	39.1	25.8	660
Support for Executive Pay	%	87.8	12.8	449

Table 4. Total Compensation regressions

These tables present regression discontinuity specifications using year-by-year models to test for the impact of the pay ratio disclosure rule on total CEO compensation. Panel A shows the main results, Panel B robustness tests and Panel C instrumental variables tests. In the first test of Panel B, we present results without winsorizing any variable. In the second test, we increase our control group by including firms with a public float above 75 million USD that are exempted from disclosing the pay ratio. In the third and fourth test, we conduct regressions with a wider (35 to 115 million USD) and narrower (45 to 105 million USD) public float bandwidth. Columns (1) and (3) of Panel C present the results for the first-stage regression, while Columns (2) and (4) of Panel C show the results of the second-stage regression, using the estimated *Affected* from the first stage. The dependent variable in all regressions, except the first-stage in the instrumental variables regressions, is the percentage *Change in Total Compensation*. The *Affected* indicator variable takes the value of 1 the company is obliged to disclose the pay ratio (i.e. firms that have a public float above 50 million and are accelerated filers). *PFL* is the public float of the firm. *PFL>75 mio* (*PFL<50 mio*) is an indicator variable equal to 1 if the organization's public float was above 75 million USD (below 50 million USD). All other independent variables are explained in Table 3. T-statistics are presented in the parenthesis. All continuous, non-logarithm variables are winsorized at the 1% and 99%. Standard errors are robust. ***, **, * indicate the significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Main results – Change in Total Compensation

	Dependent variable: *Change in Total Compensation*					
	(1)	(2)	(3)	(4)	(5)	(6)
Year:	2015	2015	2016	2016	2017	2017
Affected	-10.82	-5.95	-11.44	-5.76	43.47	38.21
	(-0.98)	(-0.55)	(-1.19)	(-0.60)	(1.34)	(1.50)
Public Float (PFL)	0.19	0.22	-0.40	-0.66	0.07	0.16
	(0.94)	(0.98)	(-0.89)	(-1.50)	(0.14)	(0.31)
PFL(PFL>75 mio)*	-0.08	-0.10	0.19	0.18	-0.13	-0.13
	(-0.49)	(-0.50)	(1.05)	(1.03)	(-0.25)	(-0.29)
PFL(PFL<50 mio)*	-0.26	-0.25	-0.15	-0.14	0.79	0.51
	(-1.18)	(-1.11)	(-0.56)	(-0.62)	(1.27)	(0.78)
Log (Total Assets)		-0.81		-3.35*		-2.17
		(-0.31)		(-1.74)		(-0.37)
Sales Growth		17.02		14.15*		-11.32
		(0.97)		(1.85)		(-0.73)
Firm Return		19.80*		29.27***		16.18
		(1.68)		(4.51)		(1.08)
ROA		-19.15		23.42**		19.43
		(-0.85)		(2.11)		(0.91)
Staggered Board		-3.73		12.86*		-30.27*
		(-0.40)		(1.92)		(-1.81)
CEO Chairperson		-22.72**		8.36		48.64
		(-2.15)		(1.07)		(1.27)
CEO Ownership		0.14		0.81**		-2.80*
		(0.31)		(2.33)		(-1.77)
Mgmt. Ownership		-0.03		-0.18		2.67**
		(-0.15)		(-0.86)		(2.18)
CEO Age		-0.27		-1.12***		-2.58
		(-0.50)		(-2.66)		(-1.61)
CEO Tenure		0.54		-0.76*		-0.77
		(1.18)		(-1.95)		(-0.66)
Observations	233	233	246	246	181	181
R-squared	0.020	0.084	0.010	0.245	0.015	0.160

Panel B: Robustness tests – Change in Total Compensation

	Dependent Variable: *Change in Total Compensation*											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Test:	No winsorizing			Expanded control group			Wider public float bandwidth			Narrower public float bandwidth		
Year:	2015	2016	2017	2015	2016	2017	2015	2016	2017	2015	2016	2017
Affected	-7.76	13.13	38.73	-2.44	-2.73	5.30	-4.62	-3.10	42.28	-15.19	-7.38	37.31
	(-0.63)	(0.46)	(1.51)	(-0.25)	(-0.37)	(0.40)	(-0.45)	(-0.28)	(1.58)	(-1.41)	(-0.86)	(1.45)
PFL	0.21	-1.58	0.16	0.29	-0.34	0.38	0.22	-0.95**	0.24	0.15	-0.77*	0.40
	(0.91)	(-1.27)	(0.30)	(1.27)	(-0.74)	(1.23)	(1.12)	(-2.12)	(0.51)	(0.66)	(-1.78)	(0.72)
PFL (PFL>75 mio)*	-0.09	-0.09	-0.13	-0.16	0.05	-0.09	-0.08	0.22	-0.24	0.02	0.20	-0.28
	(-0.46)	(-0.19)	(-0.29)	(-0.82)	(0.29)	(-0.36)	(-0.46)	(1.18)	(-0.58)	(0.11)	(1.24)	(-0.62)
PFL (PFL<50 mio)*	-0.29	1.01	0.50	-0.18	0.02	0.46	-0.20	-0.21	0.65	-0.35	-0.35	-0.12
	(-1.13)	(1.16)	(0.76)	(-0.88)	(0.07)	(0.87)	(-0.92)	(-0.84)	(1.32)	(-1.41)	(-1.54)	(-0.14)
Log (Total Assets)	-2.20	-13.85*	-2.03	-0.58	-2.34	-5.19	-1.45	-5.08**	0.91	-4.05	-1.67	-2.36
	(-0.73)	(-1.69)	(-0.35)	(-0.21)	(-1.25)	(-1.20)	(-0.59)	(-2.16)	(0.20)	(-1.49)	(-0.95)	(-0.35)
Sales Growth	21.65	8.26	-11.67	10.00	7.34	-4.20	12.85	18.93**	-2.91	18.70	13.12*	-17.40
	(1.05)	(0.34)	(-0.75)	(0.72)	(1.52)	(-0.37)	(0.72)	(2.10)	(-0.28)	(1.07)	(1.94)	(-1.04)
Firm Return	14.59	244.67**	13.97	16.70	28.97***	2.60	29.69**	28.37***	6.57	23.63*	24.98***	16.71
	(1.29)	(2.24)	(1.17)	(1.54)	(4.74)	(0.26)	(2.46)	(3.62)	(0.55)	(1.72)	(4.54)	(1.03)
ROA	-10.10	6.35	19.81	-17.57	9.66	13.18	-16.04	32.98***	-17.05	-14.33	27.99***	31.48
	(-1.17)	(0.22)	(0.98)	(-0.79)	(0.72)	(0.76)	(-0.78)	(2.87)	(-0.64)	(-0.65)	(2.72)	(1.29)
Staggered Board	-4.56	19.88	-30.54*	-1.59	11.06*	-11.78	-4.13	15.62**	-18.53*	0.79	6.91	-32.89*
	(-0.48)	(1.15)	(-1.80)	(-0.18)	(1.77)	(-1.31)	(-0.49)	(2.09)	(-1.70)	(0.07)	(1.25)	(-1.76)
CEO Chairperson	-25.12**	-4.24	49.08	-20.18**	5.75	9.40	-21.82**	12.43*	15.68	-27.52**	6.05	70.10
	(-2.26)	(-0.20)	(1.28)	(-1.99)	(0.78)	(0.50)	(-2.33)	(1.66)	(0.69)	(-2.53)	(0.88)	(1.48)
CEO Ownership	0.22	6.14**	-2.84*	0.02	0.85**	-1.40	0.80	0.97*	-1.78*	0.22	0.71**	-3.06*
	(0.45)	(2.02)	(-1.76)	(0.04)	(2.51)	(-1.52)	(1.30)	(1.92)	(-1.91)	(0.45)	(2.43)	(-1.78)
Mgmt. Ownership	-0.09	-0.72	2.69**	-0.00	-0.16	1.52**	-0.12	-0.09	1.82**	0.04	-0.33*	3.01**
	(-0.41)	(-1.05)	(2.16)	(-0.01)	(-0.78)	(2.09)	(-0.60)	(-0.39)	(2.31)	(0.17)	(-1.68)	(2.25)
CEO Age	-0.39	-4.50**	-2.50	-0.37	-1.09***	-1.89*	-0.20	-1.20**	-1.61	-0.01	-1.30***	-2.94
	(-0.64)	(-2.23)	(-1.55)	(-0.69)	(-2.66)	(-1.76)	(-0.40)	(-2.59)	(-1.39)	(-0.02)	(-2.97)	(-1.54)
CEO Tenure	0.51	-4.76**	-0.70	0.63	-0.74*	0.17	-0.15	-0.81*	0.08	0.54	-0.19	-1.45
	(1.16)	(-2.02)	(-0.66)	(1.44)	(-1.88)	(0.22)	(-0.24)	(-1.88)	(0.10)	(1.08)	(-0.52)	(-1.01)
Observations	233	246	181	260	279	242	260	274	206	203	211	152
R-squared	0.091	0.558	0.159	0.066	0.183	0.114	0.107	0.257	0.126	0.113	0.274	0.198

Panel C: Instrumental variables tests – Change in Total Compensation

Dependent variable:	*Affected*	*Change in Total Compensation*	*Affected*	*Change in Total Compensation*
	(1)	(2)	(3)	(4)
Year:	2016	2016	2017	2017
Affected before Final Rule	0.47***		0.33***	
	(9.93)		(5.10)	
Predicted Affected		-9.62		23.39
		(-1.53)		(0.92)
PFL	-0.00		-0.00**	
	(-1.48)		(-2.59)	
PFL(PFL>75 mio)*	0.01***		0.01***	
	(5.47)		(4.58)	
PFL(PFL<50 mio)*	-0.01***		-0.01***	
	(-6.69)		(-8.07)	
Log (Total Assets)	-0.02	-4.16**	-0.02	-5.01
	(-1.16)	(-2.12)	(-1.11)	(-0.86)
Sales Growth	-0.04**	13.67*	0.02	-12.95
	(-2.17)	(1.83)	(0.43)	(-0.84)
Firm Return	0.05*	26.97***	-0.02	14.40
	(1.76)	(4.42)	(-0.44)	(0.96)
ROA	0.03	25.07**	0.13*	36.23
	(0.45)	(2.32)	(1.69)	(1.49)
Staggered Board	-0.01	12.52**	-0.07	-26.77*
	(-0.44)	(1.98)	(-1.63)	(-1.74)
CEO Chairperson	-0.00	9.54	0.08	58.26
	(-0.11)	(1.21)	(1.50)	(1.49)
CEO Ownership	0.00	0.81**	0.00	-2.96
	(0.26)	(2.26)	(1.61)	(-1.61)
Mgmt. Ownership	-0.00	-0.14	-0.00	2.93**
	(-0.64)	(-0.59)	(-1.45)	(2.00)
CEO Age	0.00	-1.22***	0.00*	-2.84*
	(0.37)	(-2.87)	(1.69)	(-1.76)
CEO Tenure	-0.00	-0.72*	-0.01**	-1.05
	(-1.07)	(-1.83)	(-2.50)	(-0.86)
Observations	236	236	167	167
R-squared	0.789	0.243	0.761	0.164

Table 5. Performance-Pay regressions

This table presents regression discontinuity specifications using year-by-year models to test for the impact of the pay ratio disclosure rule on the performance linked-pay ratio. The dependent variable in all regressions is the percentage *Change in Performance-Pay*. Following Iliev and Vitanova (2019), we compute *Performance-Pay* as the sum of *Bonus*, *Stock Awards*, *Option Awards* and *Non-Equity Incentive* divided by the *Total Compensation.* The *Affected* indicator variable takes the value of 1 if the company is obliged to disclose the pay ratio (i.e. firms that have a public float above 50 million and are accelerated filers). *PFL* is the public float of the firm. *PFL>75 mio* (*PFL<50 mio*) is an indicator variable equal to 1 if the organization's public float was above 75 million USD (below 50 million USD). All other independent variables are explained in Table 3. T-statistics are presented in the parenthesis. All continuous, non-logarithm variables are winsorized at the 1% and 99%. Standard errors are robust. ***, **, * indicate the significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: *Change in Performance-Pay*					
	(1)	(2)	(3)	(4)	(5)	(6)
Year:	2015	2015	2016	2016	2017	2017
Affected	-8.51	2.84	-42.29*	-37.21	4.35	9.00
	(-0.79)	(0.24)	(-1.82)	(-1.57)	(0.25)	(0.48)
PFL	0.11	0.14	-0.26	-0.14	0.03	-0.07
	(0.54)	(0.69)	(-0.20)	(-0.10)	(0.07)	(-0.19)
PFL(PFL>75 mio)*	-0.08	-0.06	0.25	0.13	0.12	0.11
	(-0.45)	(-0.32)	(0.61)	(0.29)	(0.44)	(0.41)
PFL(PFL<50 mio)*	-0.04	0.11	-0.78	-0.61	0.14	0.03
	(-0.13)	(0.38)	(-0.91)	(-0.69)	(0.29)	(0.06)
Log (Total Assets)		4.13		1.29		4.90
		(1.39)		(0.28)		(0.99)
Sales Growth		16.17		18.61**		-3.71
		(1.13)		(2.00)		(-0.45)
Firm Return		31.30**		14.59		28.13***
		(2.21)		(1.13)		(3.08)
ROA		-1.98		34.28		-11.61
		(-0.12)		(1.40)		(-0.58)
Staggered Board		4.21		-2.32		-10.29
		(0.52)		(-0.17)		(-0.80)
CEO Chairperson		-15.59*		26.60		-8.76
		(-1.83)		(1.33)		(-0.48)
CEO Ownership		-0.52		-0.63		-1.20
		(-1.21)		(-0.90)		(-1.48)
Mgmt. Ownership		-0.10		-0.23		1.09
		(-0.43)		(-0.57)		(1.51)
CEO Age		-0.10		-1.34		-0.34
		(-0.18)		(-1.34)		(-0.36)
CEO Tenure		0.98		-0.12		0.21
		(1.45)		(-0.12)		(0.23)
Observations	206	206	222	222	166	166
R-squared	0.008	0.106	0.020	0.063	0.024	0.106

Table 6. Support for Executive Pay regressions

This table presents regression discontinuity specifications using year-by-year models to test for the impact of the pay ratio disclosure rule on say-on-pay vote outcomes. The dependent variable in all regressions is the percentage *Change in Support for Executive Pay*. Following Iliev and Vitanova (2019), *Support for Executive Pay* is the "For" votes divided by the sum of "For", "Against," and "Abstain" votes cast in the advisory vote to approve the compensation paid to the company's named executive officers. The *Affected* indicator variable takes the value of 1 if the company is obliged to disclose the pay ratio (i.e. firms that have a public float above 50 million and are accelerated filers). *PFL* is the public float of the firm. *PFL>75 mio* (*PFL<50 mio*) is an indicator variable equal to 1 if the organization's public float was above 75 million USD (below 50 million USD). All other independent variables are explained in Table 3. T-statistics are presented in the parenthesis. All continuous, non-logarithm variables are winsorized at the 1% and 99%. Standard errors are robust. ***, **, * indicate the significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: *Change in Support for Executive Pay*					
	(1)	(2)	(3)	(4)	(5)	(6)
Year:	2015	2015	2016	2016	2017	2017
Affected	-1.18	0.84	-4.95	-5.17	-3.47	2.57
	(-0.43)	(0.30)	(-1.24)	(-1.10)	(-0.61)	(0.42)
PFL	0.01	0.05	0.06	-0.03	-0.05	-0.01
	(0.22)	(0.87)	(0.24)	(-0.12)	(-0.56)	(-0.16)
PFL(PFL>75 mio)*	-0.01	-0.02	0.03	0.06	0.03	-0.02
	(-0.21)	(-0.43)	(0.35)	(0.66)	(0.36)	(-0.32)
PFL(PFL<50 mio)*	-0.08	-0.06	0.05	0.01	0.12	0.15
	(-0.81)	(-0.75)	(0.47)	(0.09)	(0.85)	(0.91)
Log (Total Assets)		-1.06		-1.08		-0.88
		(-1.54)		(-0.89)		(-0.89)
Sales Growth		-2.54		-1.03		-2.28
		(-0.69)		(-0.32)		(-0.91)
Firm Return		5.71		5.57		6.21**
		(1.43)		(1.27)		(2.46)
ROA		5.92		5.33		3.39
		(1.04)		(0.49)		(0.69)
Staggered Board		5.00**		0.55		5.22
		(2.28)		(0.14)		(1.45)
CEO Chairperson		4.50**		-2.63		-7.79**
		(2.28)		(-0.72)		(-2.17)
CEO Ownership		-0.03		0.05		-0.01
		(-0.35)		(0.44)		(-0.08)
Mgmt. Ownership		0.01		0.07		0.05
		(0.14)		(0.71)		(0.51)
CEO Age		0.03		-0.06		-0.22
		(0.26)		(-0.23)		(-1.07)
CEO Tenure		-0.07		0.11		0.37
		(-0.72)		(0.44)		(1.59)
Observations	128	128	126	126	100	100
R-squared	0.013	0.139	0.018	0.063	0.051	0.228

Table 7. Investor attention regressions

This table presents regression discontinuity specifications to test for the impact of the pay ratio disclosure rule on investor attention. In the base specification (Column (1)) we include the *Affected* dummy, as well as controls for the public float and for differential slopes of the disclosure rule cut-offs. In the full specification (Column (2)) we add firm-level controls. In Column (3) we present results without winsorizing the variables. In Column (4) we increase our control group by including firms with a public float above 75 million USD that are exempted from disclosing the pay ratio. In Column (5) we conduct regressions with a wider public float bandwidth (35 to 115 million USD) and in Column (6) with a narrower (45 to 105 million USD) public float bandwidth. The dependent variable in all regressions is the *Change in Abnormal Search* between the filing of proxy statement in 2018 and 2017. *Abnormal Search* is calculated as the daily Google Search volume index less the average daily Google Search volume index for the same weekday over the previous 10 weeks divided by the average daily Google Search volume index for the same weekday over the previous 10 weeks. The *Affected* indicator variable takes the value of 1 if the company is obliged to disclose the pay ratio (i.e. firms that have a public float above 50 million and are accelerated filers). *PFL* is the public float of the firm. *PFL>75 mio* (*PFL<50 mio*) is an indicator variable equal to 1 if the organization's public float was above 75 million USD (below 50 million USD). All other independent variables are explained in Table 3. T-statistics are presented in the parenthesis. All continuous, non-logarithm variables are winsorized at the 1% and 99%. Standard errors are robust. ***, **, * indicate the significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: *Change in Abnormal Search*					
	(1)	(2)	(3)	(4)	(5)	(6)
Test:	Base specification	Full specification	No winsorizing	Expanded control group	Wider public float	Narrower public float
Year:	2017	2017	2017	2017	2017	2017
Affected	-0.00	0.03	0.03	-0.07	0.05	0.03
	(-0.01)	(0.13)	(0.13)	(-0.56)	(0.25)	(0.11)
PFL	-0.01	-0.01*	-0.01*	0.00	-0.00	-0.01
	(-1.33)	(-1.68)	(-1.68)	(0.75)	(-1.17)	(-1.46)
PFL(PFL>75 mio)*	0.00	0.00	0.00	-0.00	0.00	0.00
	(0.31)	(0.20)	(0.20)	(-1.11)	(0.02)	(0.30)
PFL(PFL<50 mio)*	-0.01**	-0.01*	-0.01*	-0.00	-0.00	-0.01*
	(-2.00)	(-1.78)	(-1.78)	(-1.13)	(-0.71)	(-1.96)
Log (Total Assets)		0.18**	0.18**	0.05	0.13*	0.18**
		(2.14)	(2.14)	(0.69)	(1.79)	(2.03)
Sales Growth		-0.04	-0.04	-0.10**	-0.03	-0.04
		(-0.76)	(-0.76)	(-2.21)	(-0.51)	(-0.80)
Firm Return		0.07	0.07	0.03	0.03	0.07*
		(1.55)	(1.55)	(0.62)	(0.50)	(1.92)
ROA		-0.19*	-0.19*	-0.06	-0.19	-0.21*
		(-1.67)	(-1.67)	(-0.54)	(-1.57)	(-1.71)
Observations	89	89	89	111	101	75
R-squared	0.103	0.178	0.178	0.051	0.096	0.158