Employee Responses to Increased Pay Transparency: An Examination of Glassdoor Ratings and the CEO Pay Ratio Disclosure

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Abstract

We study whether and how the United States' CEO pay ratio disclosure rule affected employee pay satisfaction. Using a staggered difference-in-differences design, we find that pay satisfaction increases after pay ratios were first disclosed. We find evidence that the mechanism behind the observed increase in pay satisfaction is a change in workers' reference wages. Specifically, there is a stronger effect on pay satisfaction for employees who had less pay information available to them through other sources prior to the disclosures. Our results are among the first to document a positive effect of increased pay transparency on employee morale.

Keywords: CEO Pay Ratio, Pay Fairness, Pay Transparency, Human Capital, Disclosure Policy

We thank Glassdoor, Inc (www.glassdoor.com) for their kind assistance in providing non-publicly available salary data for this paper. Additionally, we thank the participants at Monash University, Tulane University, the Management Accounting Workshop Series, the Hawaii Accounting Research Conference, and the Corporate Governance and Executive Compensation Research Series for providing comments and suggestions on this work. We thank Alyssa Greenfield for her hard work as our research assistant. We also thank the Tulane University A.B. Freeman School of Business and Carol Lavin Bernick Faculty Grant Program for providing financial support for this project.

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1 Introduction

Increasing pay transparency through mandatory disclosures is controversial (Hendrickson, 2022; Van Olman, 2022; Cullen, 2023). While workers' rights activists advocate for more transparency under the belief that it will make pay practices fairer, employers typically oppose it (Heisler, 2021). Managers are reluctant to make pay practices transparent because they believe employees will have lower pay satisfaction when they know their coworkers' wages (Akerlof and Yellen, 1990; Card et al., 2012), resulting in decreased productivity and reduced firm value (Huang et al., 2015; Melián-González et al., 2015; Oswald et al., 2015). Yet the actual effects of increased pay transparency on employees are largely unknown because the decision to disclose within-firm pay information is rare outside of experimental settings (Russsell, 2020; Fox, 2022). In this paper, we exploit a market-wide exogenous shock to the pay information environment of publicly traded firms in the United States, the mandatory CEO pay ratio disclosure, to examine whether the disclosure of median employee wages affects employee pay satisfaction.

Research on the CEO pay ratio (hereafter pay ratio) rule has largely focused on how equity investors respond to the disclosure (SEC, 2015). In general, investors respond more negatively to larger pay ratios, as these are seen as evidence of unfair pay practices, which can reduce employee morale and consequently firm performance (Kelly and Seow, 2016; Rouen, 2020; Chang et al., 2022; Pan et al., 2022; LaViers et al., 2022). However, less is known about the responses of employees to this information. We fill this gap in the literature by studying whether and how employees respond to pay ratio disclosures, especially median employee pay information. Our findings highlight important consequences of the pay ratio disclosure rule and provide novel insights regarding the impact of pay transparency on employee behavior.

In economic models of pay transparency, employees determine their pay satisfaction by comparing their wage to a reference wage (Clark and Oswald, 1996; Breza et al., 2018), with pay satisfaction increasing (decreasing) as they are paid more (less) relative to the reference wage. Employees calculate reference wages using available pay information and factoring its perceived accuracy and relevance to their current job (Akerlof and Yellen, 1990). Before the pay ratio rule took effect, employees had a variety of different information sources to learn about wages, but only the pay of

the five named executives was mandatorily disclosed by firms themselves (Ising and Marshall, 2016; Trotter et al., 2017). After the disclosure of the pay ratio, employees had an additional source of pay information: the compensation of their company's median employee. If this new information changes employees' reference wages, we should observe changes in pay satisfaction. As both pre- and post-disclosure reference wages are unobservable, the direction of the effect on pay satisfaction is difficult to predict. However, based on economic theory, we can interpret observed increases (decreases) in average levels of pay satisfaction as decreases (increases) in average reference wages after the pay ratio disclosure.

It is possible, however, that employees might not respond to the pay ratio disclosures at all or that employees would have different reactions to it depending on the amount of information they already had about the compensation of the other employees in their firm. Given that the pay ratios are disclosed in proxy statements, which are intended for use by shareholders, employees might not access the pertinent pay ratio information. Despite this, compensation consultants—such as Compensation Advisory Partners and Pearl Meyer—have advised managers to provide employees with information about and justifications for the firm's pay ratio (Lifshey and Podstupka, 2017; Engel, 2017). Furthermore, dozens of articles have been written in the popular press about the pay ratios of specific companies, so many employees likely received the information (Bushee et al., 2010).[1] However, even if employees were informed about their firms' pay ratios, it is unclear whether this information would have impacted their perceptions about their own pay because they may have already had a large amount of pay information if, for example, their firm is in a more transparent labor market or if their higher status in the firm gives them increased access to information about others' wages. Employees who already had an adequate amount of pay information may not have changed their reference wages in response to the information in the pay ratio disclosures, while those who had a lower level of knowledge may have had a more extreme reaction to it. As a result, it is an empirical question whether the increased pay transparency brought about by the pay ratio disclosures led to changes in employee pay satisfaction.

We use the Compensation-and-Benefits ratings from Glassdoor.com to proxy for employee pay satisfaction, which allows us to test whether employee pay satisfaction changes after firms first

[1] See, for example, Wilmont (2017); Francis and Fuhrmans (2018); Gelles (2018a,b).

disclose their pay ratios and median employee pay. Glassdoor covers many of the largest publicly traded companies in the U.S., allowing us to draw more generalizable inferences than can single-firm studies of pay transparency (Card et al., 2012; Breza et al., 2018; Grabner and Martin, 2021; SimanTov-Nachlieli and Bamberger, 2021). On top of this, the frequent and extensive employee activity on the website allows us to examine a relatively narrow window around the pay ratio disclosures, which reduces the concern that something other than the firms' pay ratio disclosures drives changes in employee pay satisfaction. Finally, the ratings include other dimensions of employee satisfaction, providing us with metrics to conduct falsification tests.

Using the Compensation-and-Benefits ratings for 1,362 firms in the months surrounding their initial pay ratio disclosures, we find that employee pay satisfaction increases after firms first disclose their pay ratios. In terms of economic significance, the disclosures lead to an increase in ratings of 0.05 (on a five-point scale), which is equivalent to 6% of the sample standard deviation. The magnitude of this documented effect is consistent with other papers that use Glassdoor data.[2] The increase in aggregate employee pay satisfaction suggests that, on average, employees' reference wages decreased when they first learned of the pay of their firms' median employee. We conjecture that reference wages decreased on average because, before the disclosure, employees likely placed some weight on executive pay when making their reference wage calculations, as executive pay was the only pay information firms had to disclose.[3] After the pay ratio disclosure, employees added the newly disclosed median employee pay to their reference wage calculations. As median employee pay is generally magnitudes lower than executive pay, shifting weight from the executives' pay to the median employees' pay would cause workers' post-disclosure reference wages to be lower than their pre-disclosure reference wages. This would have caused their own pay to appear more favorable, leading to the increase in pay satisfaction that we observe. These conjectures and findings align with the experiences described by compensation consultants from Pearl Meyer, who said: "The lower the median employee's pay, the better the employee population may feel about their own

[2]For example, Liu et al. (2023) consider changes in ratings of maternity leave benefits, and they document an effect size of 7% of the sample standard deviation.

[3]Surveys show that the average worker in the U.S. does have an awareness of CEO pay and uses this knowledge to inform their reference wages. For example, one survey shows that employees estimate mean CEO pay to be over $9 million (Larcker et al., 2016). This survey evidence also shows that employees believe a CEO should only make 17.6 times more than the firm's average employee indicating that they may also believe that their wages should be a particular fraction of the CEO's wages.

compensation. While everyone knows the CEO will make a lot more than most of the company, most employees are more concerned about how their pay compares with other employees" (Lifshey and Podstupka, 2017).

We perform several robustness tests to validate our finding that employee pay satisfaction increases following the pay ratio disclosure. To address a concern of the staggered difference-in-differences design that all firms in our sample are eventually treated, we use a stacked regression specification and find that our results are robust to this alternative specification (Cengiz et al., 2019; Barrios, 2021; Baker et al., 2022). Next, because 77% of our firms report their pay ratio disclosures in either March or April (due to the common fiscal year-end choice of December 31) our treated firm-months outnumber the control firm-months. To correct for this, we use a fixed-window difference-in-differences regression with an equal proportion of treatment and control months. Once again, our results hold. Our results are also robust if we back-fill missing firm-month ratings values using values from previous months.

We perform several falsification tests to corroborate the claim that the increases in employee satisfaction result from the pay ratio disclosure. First, we show that the prior years' Compensation-and-Benefits ratings are not impacted by the prior years' proxy statement, which reduces the concern that other proxy statement information may be driving the observed effects on employee pay satisfaction. This finding confirms that the pay ratio and median employee pay information, which were novel items in the proxy statement, changed the employee pay information environment. In addition, we find no evidence that the pay ratio disclosures impacted employees' Work-Life-Balance ratings, an alternative measure of employee sentiment unrelated to compensation. This suggests that our pay satisfaction result is not simply capturing a more general trend in employee sentiment. Next, we show that the frequency of Glassdoor ratings is stable before and after the pay ratio disclosures. Together, these last two results help mitigate the concern that our results stem from managers encouraging employees to post positive reviews online immediately following the pay ratio disclosure to offset any potential negative sentiment. Next, we examine the effects on pay satisfaction ratings from employees at private firms who are not subject to the same SEC disclosure laws and did not have to disclose their pay ratios. We do not find any significant changes to private firm compensation ratings before and after public firms made their disclosures. Collectively, our

findings provide robust evidence that employee pay satisfaction improved due to the increased pay transparency brought about by the pay ratio disclosure.

We propose that the changes in pay satisfaction following the pay ratio disclosure are the result of employees using the newly disclosed median employee pay information to update their reference wage calculations. In other words, a change in employees' reference wages is the mechanism that links the information in pay ratio disclosures to changes in pay satisfaction. While we cannot directly observe employees' reference wages, we perform several tests to support this proposition and dismiss alternative explanations. First, we decompose the pay ratio into its two components—median employee pay and CEO pay—to see which of these impact pay satisfaction. We find that firms with lower levels of median employee pay drive the observed increase in pay satisfaction and that the increase is unrelated to CEO pay. This finding comports with the idea that updating reference wages with the median employee pay levels decreases the reference wages, causing employees' own pay to appear more favorable. It also suggests that the increase in pay satisfaction is indeed due to the new information provided to employees, the median employee's pay, and not due to the CEO pay information previously known to them. Second, we show that the effect on pay satisfaction does not differ based on the magnitude of the pay ratio itself, indicating that employees are not likely responding to the pay level of the median employee *relative* to that of the CEO, but instead to the *absolute* pay level of the median employee. This evidence is consistent with reference wage changes explaining the pay satisfaction effect.

In order to examine how the new information in the CEO pay ratio may affect some employees more than others, we examine firm- and employee-level differences in employees' pay information environments before the disclosure. We argue that the new median employee pay information will matter less (more) to employees who had more (less) pay information sources at their disposal already. We proxy for employees' pay information in four ways: (i) the number of Glassdoor ratings about their firms in the three months before the pay ratio disclosure, (ii) the level of labor-related media coverage of their firms in the prior year, (iii) the number of firms in their same industry within 20 miles of their firms' headquarters, and (iv) the number of peer firms that have disclosed their pay ratios. For the first three proxies, we find evidence that employees with more pay information realize a significantly lower increase in pay satisfaction, due to the pay ratio disclosure. The direction of

the effect is the same when using the fourth proxy, although not statistically significant.

Next, we examine how the effect of pay ratio disclosures on employee pay satisfaction varies based on two employee-level characteristics related to the pay information they had access to prior to the disclosure: managerial status and tenure. We predict and find a greater effect on that pay satisfaction of non-managers, supporting the idea that managers, who likely have access to a greater amount of pay information than entry-level employees, found the median pay level to be less informative. We also find that the pay satisfaction effect is being driven by short-tenured employees, not long-tenured employees, as the former are likely to have less pay information and find the newly disclosed information more meaningful. Together these firm- and employee-level results provide evidence that the disclosure of the median employee's pay had a greater affect on employees who had fewer pieces of pay information at their disposal with which they could make their reference wage calculations. When employees had less pay information ex ante, the new median employee pay information had a stronger affect on their pay satisfaction levels.

As a final test to substantiate our interpretations, we examine the change in ratings dispersion after the disclosures. If the disclosure of median employee pay information provided novel, precise information for employees to incorporate into their reference wage calculations, there should be less dispersion in employees' reference wages after the disclosure. Consequently, we would expect the dispersion in pay satisfaction to decrease as well. Consistent with these predictions, we find that dispersion in Compensation-and-Benefits ratings (measured as the standard deviation of ratings in a given firm-month) decreases after the disclosure. This finding supports the notion that the pay ratio disclosure increased pay transparency for employees, leading them to develop more similar and more accurate beliefs about wages.

Our paper makes several contributions to the literature. It is one of the first to estimate the effects of the CEO pay ratio disclosure rule on employees. This disclosure is the first rules-based human capital management disclosure, and it is novel because it required firms to report pay information that employees likely did not know beforehand (Pan et al., 2022; LaViers et al., 2022). While firm managers (and their consultants) feared the worst when they first reported their pay ratios (Kohler and Seelig, 2017; McGregor, 2018; Zhao, 2018), our paper shows that there was at least one positive aggregate effect on employees—their pay satisfaction levels increased. Our

findings complement those of Clark and Oswald (1996), who show that relative pay levels are associated with employees' compensation satisfaction. By examining the effects of a new disclosure law, we shed light on the mechanism behind the relation between pay transparency and pay satisfaction. Our findings also relate to those of Green and Zhou (2019)—who estimate a positive relation between the pay levels reported on Glassdoor and employee pay satisfaction—but differ in that Green and Zhou (2019) do not study employee responses to changes in their pay information environment. Additionally, our findings contrast with those of a concurrent working paper by Boone et al. (2021), who document a negative relation between pay ratio magnitudes and pay satisfaction. This discrepancy in results is most likely due to differences in econometric specifications, as we use firm-month level data to document within-firm changes in employee satisfaction, whereas Boone et al. (2021) perform cross-firm comparisons using yearly data.

This paper also contributes more broadly to the understanding of employees' responses to increased pay transparency. While analytical models suggest that pay transparency could both boost and damp employee pay satisfaction, most empirical studies document only negative effects (Colella et al., 2007; Downes and Choi, 2014; Brown et al., 2022). Breza et al. (2018), for instance, document a negative or insignificant effect, depending on employees' perceptions of the fairness of their pay structure.[4] Similarly, Card et al. (2012) find only negative effects of increased pay transparency, but their study is limited by a measurement issue wherein negative responses (e.g., employee turnover) are much easier to observe than positive ones (e.g., working additional hours). We contribute new evidence that increased pay transparency, through its impact on employees' reference wages, can lead to increased pay satisfaction in a setting where both negative and positive reactions are equally observable. Other studies, particularly those conducted in laboratory settings, have typically assumed that employees go from pay complete secrecy to some form of pay transparency (Gächter and Thöni, 2010; Grasser et al., 2021). However, pay secrecy is uncommon in the labor market, particularly in recent times with online job search engines, limiting the generalizability of these previous findings. Our results highlight how important the total pay information environment of the firm is in moderating employees' responses to newly disclosed pay information.

Our findings also contribute to the literature on human capital management disclosures. Un-

[4] Grabner and Martin (2021) make similar conclusions using an archival data set.

derstanding how the pay ratio disclosure affects firm stakeholders is particularly important, as the SEC is currently debating the implementation of more human capital management disclosures (Herren Lee, 2020; Herren Lee and Crenshaw, 2021). The SEC is doing this because human capital is now seen as the driver of a modern firm's value (Clayton, 2020; Crenshaw, 2021; Brandenburg and Khanna, 2022). Our findings provide novel insights as to how these types of disclosures can impact employee satisfaction, which could, in turn, affect effort and productivity. Finally, our results complement the growing literature on how corporate disclosures can affect labor market outcomes (Chakravarthy et al., 2014; Golshan et al., 2022). While some studies document the effects of earnings announcements and diversity disclosures on labor market participants' behavior (Choi et al., 2020; deHaan et al., Forthcoming; LaViers and Sandvik, 2022), our work documents the effects of compensation-focused disclosures on employees. Our findings stand to inform academics, managers, and regulators about the potential effects of human capital management disclosures on employees.

The rest of the paper is organized as follows. In Section 2, we discuss the literature on the pay ratio disclosures and pay transparency, which motivates our research question. We describe our data collection and sample in Section 3. In Section 4, we detail our approach to estimating the effect on pay satisfaction and describe our results. We examine the mechanisms that drive our main results in Section 5. We conclude in Section 6.

2 Background and Hypothesis Development

The United States has experienced one form of pay transparency for many years. Since 1992, firms have had to disclose the pay of the CEO and top executives (Ising and Marshall, 2016).[5] This information is disclosed annually in the firm's publicly available proxy statement, and the CEO's pay from this disclosure is the numerator of the pay ratio. The denominator of the ratio, the median employee's pay, is the new information that became a mandatory disclosure item for fiscal years beginning on or after January 1, 2017. The median employee's pay is calculated in the same way as that of the CEO and the other named executive officers.[6] Prior to the pay ratio disclosures,

[5]See https://www.law.cornell.edu/cfr/text/17/229.402.

[6]The SEC allows little discretion in the selection of the median employee. While firms can use a handful of statistical sampling methods to select the median employee, they can exclude only a few employees. They

employees knew the pay of the CEO and the other top earners in the firm, but they did not have any mandatorily reported information about the pay of other rank-and-file employees. As such, the pay ratio disclosure rule is the first disclosure law that increases pay transparency for non-executive employees in the United States.

Rank-and-file pay transparency is generally resisted by firms because of a longstanding belief that it will hurt overall firm value. This harm is described by the fair-wage effort hypothesis, which draws on equity theory and predicts that, when employees learn they are paid less than their coworkers, they become less productive (Akerlof and Yellen, 1990). Conversely, employees who learn that they are paid more than their coworkers do not become any more productive, as they feel that they deserve their higher levels of compensation. The resulting shirking by low-paid workers and the lack of response by high-paid ones suggest that this form of pay transparency will damp total firm productivity.

However, newer models of pay transparency, like the one of Breza et al. (2018), are more nuanced. This model allows for employees to have multiple types of social preferences. As a result, it allows for increased pay transparency to have both positive and negative effects on employee effort and pay satisfaction. The model describes employees' utilities as a function of their wages, costly effort, and their morale related to within firm pay equality.[7] Employee morale is determined by the relationship between an employee's own wage and the wage of another employee, i.e., the reference wage. The addition of another employee's wage into the utility function means that the focal employee's utility is explicitly a function of not only their own compensation but also the compensation of another employee made known through pay transparency.

In most U.S. firms, where pay transparency has not been regularly practiced, employees are unlikely to be in pay information environments that include complete knowledge of their firms' pay structures. As a result, they are unlikely to know peer employees' salaries with perfect certainty (Brown et al., 2022). Instead, they must calculate reference wages themselves using any information available to them. This information might include labor market data, anonymous pay information

must include all part-time and seasonal workers, and the compensation cannot be annualized. Firms may exclude foreign employees but only if those employees comprise less than 5% of the firm's global workforce or if they work in a country where data privacy laws do not allow their inclusion (SEC, 2015).

[7]This model is also consistent with predictions from both tournament theory and equity theory (Adams, 1963; Leventhal, 1980; Lazear and Rosen, 1981).

disclosed online, and any pay information disclosed by the firm. Employees' access to this type of information determines their ability to precisely estimate a reference wage. After they form this estimate, they compare their own wage to it (Breza et al., 2018). Employees' levels of pay satisfaction strictly increase as they are paid more relative to their reference wages and decrease as they are paid less relative to them. This symmetric effect produces the possibility of both positive and negative effects on pay satisfaction from increased pay transparency. Importantly, employees' reference wages may change when the information available to them changes. In the case of the pay ratio disclosure, if the median employee pay disclosed to them is higher (lower) than their previously calculated reference wage, then their reference wage should increase (decrease), leading to lower (higher) pay satisfaction. In other words, the directional effect of pay transparency on pay satisfaction depends on whether the new information raises or lowers employees' reference wages. In Appendix B, we provide an example that shows that both increases and decreases in reference wages are possible outcomes under a variety of scenarios.[8]

While theory suggests that increased pay transparency can improve employee pay satisfaction, the common belief continues to be that it is likely to reduce employees' pay satisfaction. Because of this, managers were concerned about how employees might react to the disclosure of the pay ratio in their proxy statements. Despite the fact that employees may be unlikely to read the entire proxy, they may still be exposed to the pay information in it through other channels. For example, before the first year's pay ratios were disclosed, compensation consultants advised firms to prepare internal communications to help employees understand the new compensation information and prevent negative reactions to it (Kohler and Luss, 2017; Kohler and Seelig, 2017; Lifshey and Podstupka, 2017). As an example of this, Compensation Advisory Partners advised: "There is a high potential for negative publicity associated with pay ratio disclosure. Get in front of it and anticipate employee reactions to the disclosure. Provide talking points to the leadership team so that they can respond to employee concerns in a consistent manner" (Engel, 2017). As a result, employees may have received the new pay information through firm-wide emails or meetings with managers. Additionally, the business media covered the pay ratios extensively (Picchi, 2018;

[8]These scenarios assume that CEO pay can be in the employee's information set. Using the actual sample distributions of both types of pay disclosed by the firms, CEO and median employee pay, we provide the distribution of reference wage outcomes, assuming different weights on the elements of the information set.

Weaver, 2018; Tuttle, 2018). For instance, a February 2018 article in The Washington Post discussed the pay ratios of Teva Pharmaceuticals, Umpqua Holdings, and Honeywell (McGregor, 2018), and a May 2018 article in Forbes reported the pay ratio information of Mondelēz International, Mattel, and Berkshire Hathaway (Hembree, 2018). Since the media serves as an important intermediary for financial information, it may have helped employees more quickly process this information (Bushee et al., 2010). These internal disclosures and media articles indicate that firm managers and the media believed that workers were interested in and would respond to the information in the pay ratio disclosures. As a result, these efforts could have made the disclosures even more salient to employees and caused median employee pay information to weigh more heavily into employees' reference wage calculations, leading to changes in their pay satisfaction.

We test this possibility by examining whether the pay ratio disclosures affected employee pay satisfaction. If their reference wages decreased, due to the disclosure of median employee pay information, then their own wages would have appeared more favorable, and their pay satisfaction would have increased. Alternatively, if their reference wages increased, then their own wages would have appeared less favorable, and their pay satisfaction would have decreased. The aggregate effect on pay satisfaction will depend on which effect dominates. If, however, relatively equal proportions of employees had increases and decreases in reference wages, this would have led to relatively equal proportions of decreases and increases in pay satisfaction. In that case, we might not observe an overall effect. That said, if employees are not exposed to this information, if the information does not matter to them, or if the information does not revise any of their beliefs about relative pay within the firm, then the disclosures will not affect employee pay satisfaction.

We also test whether or not the information in the pay ratio disclosure affected employees differently based on the depth of their pay information environments prior to the disclosure. This new information should matter less to employees who had more sources of pay information available to them. We predict this based on the notion that employees weigh all the relevant information at their disposal when computing their reference wages. As all the weights need to sum to one, using many information sources will likely decrease the weight on each one. Said differently, the more pay information employees have before the ratio's disclosure, the less important the new information will be, which will cause these employees to revise their reference wages to a lesser extent. As

a result, we predict that the new information about median employee pay provided in the pay ratio disclosure will have less of an effect on the reference wage calculations and pay satisfaction of employees who had more pay information available to them prior to the disclosure.

3 Data Compilation

To compile our sample, we hand-collect the pay ratio disclosures for the 3,000 largest publicly traded U.S. companies. We searched all proxy statements on EDGAR filed between January 1, 2017, and December 31, 2019, as these disclosures were mandated beginning with fiscal years starting on or after January 1, 2017. We obtained pay ratio disclosures for 2,237 companies.[9] As we are examining how pay satisfaction is affected when employees *first* learn about the median employee's pay information, we focus on firms' first mandated pay ratio disclosures.[10] The first disclosures in our sample are reported in February 2018, and we include all disclosures through January 2019.[11] Table A.1 in the appendix tabulates the year-months of the disclosure events in our sample. The most common disclosure months are March and April of 2018, with approximately 77% of the disclosures in our sample occurring in these months. For each pay ratio disclosure in our sample, we obtain the CEO's total annual compensation, the median employee's total annual compensation, and the ratio of the two compensation amounts.

We next obtain data on employee pay satisfaction from Glassdoor, a website where current and former employees can anonymously leave ratings of their employers (Hales et al., 2018). Since launching in 2008, Glassdoor has become one of the largest platforms for collecting and disseminating these kinds of ratings (Dube and Zhu, 2021). As of January 2022, over 110 million ratings

[9]Not all firms provided the disclosures in their 2017 fiscal year-end reports, as some were exempt in the first mandated year of the rule (SEC, 2015). In addition, a few pay ratio disclosures are contained in 10-K filings, instead of proxy statements. Our sample size is comparable to those of other studies. For example, Pan et al. (2022) analyze a sample of 2,307 initial pay ratio disclosures.

[10]Companies need only update their median employee every three years and wages are sticky, so relatively little new information is contained in the subsequent years' pay ratio disclosures.

[11]One disclosure is contained in a 2017 proxy statement, but we omit it from the sample, as it captures 2016 fiscal year-end data, so the pay ratio disclosure was not yet mandatory. We do not find any pay ratio disclosures that were reported in January 2018. We exclude pay ratio disclosures that were reported after January 2019 to avoid overlap with firms that would already be reporting their second mandated pay ratio disclosures.

had been posted of over 2 million companies.[12] To meet the website's quality standards, each submitted rating must satisfy the following requirements: (1) it must be contributed by a person whose employment status with the firm is verified through an email address or social networking account; (2) it must pass the site's fraud detection algorithm; and (3) it must not be flagged during a random audit by the site's quality assurance team. Glassdoor uses a "give and take" approach to collecting and disseminating ratings: if individuals want to see the ratings of other companies, they first must compose and share ratings of their own employers (current or former).

Data from Glassdoor are increasingly used in academic research (Teoh, 2018). Studies document that the ratings contain much job-relevant information that correlates strongly with future firm performance (Hales et al., 2018; Huang et al., 2020) and stock returns (Green et al., 2019; Sheng, 2021). The ratings contain individuals' overall assessment of the company, along with their reviews of the company's "Work Life Balance," "Senior Management," "Culture and Values," "Compensation and Benefits," "Career Opportunities," and "Diversity and Inclusion." Each of these ratings is made on a five-point scale, with five being the best.[13] The numeric ratings allow employee sentiment to be quantified along several different dimensions. We web-scrape Glassdoor to extract all the ratings for each firm in the sample from November 2017 to April 2019 (i.e., three months before (after) the first (last) disclosure in our sample),[14] and we aggregate the ratings along each sentiment dimension to the firm-month level.[15] Our main variable of interest is the Compensation-and-Benefits rating, as it most closely relates to employee pay satisfaction.

Merging the Glassdoor data with the pay ratio data results in 37,152 firm-month observations. We exclude firm-months that overlap with the firm's prior year's or next year's proxy statement, relative to the one with their initial pay ratio disclosures, further reducing the sample to 33,710 firm-months. We obtain quarterly fundamental financial data from Compustat, analyst forecast

[12] Source: https://www.glassdoor.com/about-us/.

[13] Some raters also share free response comments about their personal experiences at the firm.

[14] Because of this requirement, the "sample period" in our setting differs from the "event period." The event period starts in February 2018, when the first pay ratio disclosure is reported, and ends in January 2019, when the last pay ratio disclosure is reported. The sample period accounts for all the Glassdoor data and financial controls used in our regression analysis, which extends three months before the first pay ratio disclosure is reported and three months after the last pay ratio disclosure is reported. Figure A.1 in the appendix illustrates this distinction.

[15] Some academic research relies on yearly snapshots of Glassdoor ratings from internet archive providers, like Wayback Machine (Barnes, 2020), but this approach limits the granularity of the collected data. Our approach allows for a much richer dataset than what might be seen in other papers.

data from IBES, and institutional shareholding data from Thomson Reuters. Removing firm-month observations with missing control variables results in 32,969 firm-month observations across 2,026 firms. To estimate intra-firm changes in pay satisfaction, we require that firms have at least one Compensation-and-Benefits rating within both the three-month period before and the three-month period after the month of their initial pay ratio disclosures. After dropping firms that do not satisfy this requirement, our final sample consists of 1,362 unique firms and 18,690 firm-month observations. Table A.2 in the appendix describes the sample selection procedure.

Table 1 reports summary statistics for our variables. The average number of ratings posted for a given firm-month is 4.10 (i.e., $e^{1.63} - 1 = 4.10$) and 6.10, conditional on having nonmissing Compensation-and-Benefits ratings that month. Across our sample period, there are 3,921 firm-months in which no ratings are left about the company. The average Compensation-and-Benefits rating is 3.36 (on a five-point scale), which is comparable to the averages documented elsewhere.[16] *Post Disclosure* is set to one in the month a firm discloses its initial pay ratio disclosure and each subsequent month, and it is set to zero in the months before the disclosure. The average pay ratio in our sample is 197, with an average CEO pay of $8.2 million and an average median employee pay of $70,000. The average firm in our sample has total assets of $4.3 billion, contains 1.59 different business segments, and is 20 years old. We discuss our control variables and those used in cross-sectional tests in the following sections as we describe our regression specifications. All variables are defined in Table A.3 in the appendix.

In Table 1, we also report descriptive statistics related to the characteristics of the individuals who contribute reviews to Glassdoor. Details regarding a contributor's managerial status and tenure with the firm come from our web-scraped dataset. Among the contributors in our sample, 13% are in a managerial role (i.e., their reported job title includes the word "manager," "director," "executive," or "supervisor); 15% have a tenure less than one year. Using proprietary compensation data provided to use directly from Glassdoor, we are also able to provide general insights regarding the distribution of compensation among Glassdoor contributors.[17] We have compensation data for

[16]For instance, Hales et al. (2018) report an average of 3.29, and Green et al. (2019) report an average of 3.21.

[17]Unfortunately, we are unable to merge this compensation data with the data in our main sample, as the two data sets lack a common identifier.

111,944 contributors who posted reviews about our sample firms within the three-month window before or the three-month window after their firm's initial pay ratio disclosure. The average (median) reported yearly pay among these contributors was \$67,911 (\$50,000). We find that 55.6% of the contributors have above median employee pay, while 44.4% have below median employee pay (very few, only 0.0007% of contributors have yearly pay equal to that of the median employee). This suggests that individuals who leave reviews on Glassdoor come from both the left and right halves of the firm's overall compensation distribution.

In Table 2, we compare the Glassdoor ratings and firm characteristics before and after the initial pay ratio disclosures (i.e., comparing the summary statistics of firms in the pre-disclosure period, when *Post Disclosure* = 0, to the summary statistics of firms in the post-disclosure period, when *Post Disclosure* = 1). We find preliminary evidence of an increase in *Compensation-and-Benefits Rating* in the post-disclosure period, with the average rating increasing from 3.31 to 3.39. We find that several other firm characteristics differ between the pre- and post-periods, with *Log Assets*, *Book to Market*, and *Institutional Shareholding* being larger, on average, in the post-period, whereas *Intangible Asset*, *Earnings Surprise*, and *Cash Holding* are lower, on average. We control for all of these characteristics in our main tests to ensure our results are not driven by changes in these characteristics. Importantly, the bottom two rows of Table 2 show that the compensation composition of Glassdoor contributors does not meaningfully change across the pre- and post-disclosure periods. To further illustrate this point, the fraction of contributors with yearly pay above the median employee pay was 55.3% (55.9%) in the pre-disclosure (post-disclosure) period, indicating that the types of individuals who submit ratings on Glassdoor did not meaningfully change after the firms' pay ratios were first disclosed.

4 Research Design and Results

4.1 Research Design

To test whether employee pay satisfaction changes after firms disclose their pay ratios, we employ a difference-in-differences research design by exploiting the staggered timing of the disclosure of

firms' initial pay ratio information. The SEC requires companies to file their proxy statements within 120 days after their fiscal year-end.[18] Table A.1 in the appendix shows that 81% of our sample firms file by May 2018, which we would expect, given that most firms end their fiscal years on December 31. The remaining 19% are filed over the subsequent months (1%–4% of firms per month) until January 2019. The observed heterogeneity in the initial disclosure months in our sample gives us the necessary variation to identify effects using a staggered difference-in-differences design.[19] We include firm fixed effects into our regression specifications, allowing us to estimate intra-firm changes in employee pay satisfaction before and after the disclosure of pay ratio and median employee pay information.[20] We estimate the following model using ordinary least squares:

$$\begin{aligned} \mathrm{y}_{i,m} = \alpha_i &+ \beta_1 \text{Post Disclosure}_{i,m} + \beta_2 \text{Log Assets}_{i,q} + \beta_3 \text{Number of Segments}_{i,y} \\ &+ \beta_4 \text{Intangible Assets}_{i,q} + \beta_5 \text{Book to Market}_{i,q} + \beta_6 \text{Log Firm Age}_{i,q} \\ &+ \beta_7 \text{Log Analyst Coverage}_{i,y} + \beta_8 \text{Institutional Shareholding}_{i,q} \\ &+ \beta_9 \text{Earnings Surprise}_{i,q} + \beta_{10} \text{Cash Holding}_{i,y} + \text{Firm FE} + \text{Time FE} + \varepsilon_i, \end{aligned} \tag{1}$$

where $y_{i,m}$ represents the average Compensation-and-Benefits rating given to firm i by Glassdoor raters in month m. *Post Disclosure*$_{i,m}$ equals one in the month that firm i discloses its initial pay ratio and each subsequent month and zero in the months before the disclosure. With the inclusion of firm fixed effects, the coefficient β_1 captures the change in the focal firm's Compensation-and-Benefits rating after the initial disclosure of its pay ratio information, relative to changes in ratings among firms that have not yet disclosed. This design choice mitigates concerns about bias caused by time-invariant, firm-specific omitted variables. Our short sample window, from November 2017 to April 2019, also reduces the concern that unobservable, time-varying firm characteristics impact

[18] See https://www.sec.gov/files/form10-k.pdf.

[19] Importantly, we find very little evidence that firms adjust their fiscal year-end dates and proxy statement filing dates to time their pay ratio disclosures, as 98% of the firms in our sample file their proxy statement containing their initial pay ratio disclosure on month-dates that are within 30 days of the month-date of their previous year's proxy statement filing.

[20] To use this research design to detect the impact of pay ratio disclosures, the dependent variable should have variation within firms. Our measure of employee pay satisfaction, the monthly average of Glassdoor ratings, meets this requirement. Figure A.2 in the appendix reports the distribution of the within-firm standard deviations of Compensation-and-Benefits ratings. More than half of the firms have a standard deviation above 0.5, which represents 58% (0.5/0.86) of the sample-wide standard deviation and 15% (0.5/3.36) of the sample-wide mean, based on the summary statistics reported in Table 1.

employee pay satisfaction.

We control for several observable firm characteristics that may impact employees' responses to the disclosure of pay ratio information. We control for the size of the firm via *Log Assets*$_{i,q}$, as large firms have more sophisticated pay structures. Similarly, we control for *Number of Segments*$_{i,y}$ and *Intangible Assets*$_{i,q}$, which proxy for the complexity of the firm's operating environment and the importance of human capital to the firm. As employees are potentially more likely to tolerate lower wages if they work in firms with high growth opportunities, we control for *Book to Market*$_{i,q}$ and *Log Firm Age*$_{i,m}$. *Log Analyst Coverage*$_{i,y}$ and *Institutional Shareholding*$_{i,q}$ proxy for the external monitoring performed by financial intermediaries, as this monitoring can affect how firms treat employees (Adhikari, 2016; Chen et al., 2020). We also control for earnings surprises, *Earnings Surprise*$_{i,q}$, since studies find that financial performance influences employees' evaluation of their jobs and facilitates their job-search decisions (deHaan et al., Forthcoming; Choi et al., 2022). Finally, as employees are largely paid with cash, we control for *Cash Holding*$_{i,q}$ to account for the firm's financial slack in its ability to compensate its employees. All continuous variables are winsorized at the 1% and 99% levels, with the exception of Glassdoor ratings, as they do not have outliers by design. We double cluster standard errors by firm and year-month in all our regression specifications.[21]

4.2 Main Effects of Pay Ratio Disclosure on Pay Satisfaction

Table 3 reports estimates of the relation between pay ratio disclosures and employee pay satisfaction. In Column (1), we report the univariate relation between *Post Disclosure* and pay satisfaction. The positive, significant coefficient suggests that firms that have disclosed their pay ratio information receive greater Compensation-and-Benefits ratings than do those that have not yet disclosed. In Column (2), we include firm fixed effects to net out time-invariant firm-specific confounding factors and year-month fixed effects to capture time trends, such as the possibility that pay satisfaction might simply increase over time. We continue to find a positive, significant coefficient on *Post Disclosure*, implying that a firm's average Compensation-and-Benefits rating increases after the

[21]Note, due to differences in the frequency of data availability across publicly available datasets, some of our control variables are calculated based on the most recent fiscal-quarter-end, denoted by the subscript q, whereas others are calculated based on the most recent fiscal-year-end, denoted by the subscript y.

initial disclosure of its pay ratio information. The effect is significant at the 5% level, and the magnitude (0.049) represents 6% of the sample standard deviation (0.86 from Table 1). The economic significance is comparable to that of prior studies that use Glassdoor ratings data as an outcome variable of interest (Liu et al., 2023).

The inclusion of firm fixed effects in Column (2) allows for a within-firm interpretation of the changes in pay satisfaction, differentiating our analyses from concurrent working papers that estimate cross-sectional differences in pay satisfaction across firms (Green and Zhou, 2019; Boone et al., 2021). In Column (3), the estimate on *Post Disclosure* is essentially unchanged after adding firm-level control variables. The estimated effects of the other control variables are statistically insignificant, consistent with the notion that firm fundamentals remain stable within a short event window, thus having relatively little explanatory power for changes in employee pay satisfaction.[22] The findings in Table 3 show that employees' pay satisfaction improves when their firms disclose pay ratios and median employee pay information. These results suggest that workers' reference wages decreased after the disclosure of their firms' pay ratio and median employee pay information, causing their own pay to appear more favorable than it did before they knew the median employee's pay.

A critical assumption of the difference-in-differences methodology is that of parallel trends. Treated and control groups must have similar pre-trends in the outcome of interest. To demonstrate the validity of this assumption in our setting, we follow previous studies and re-estimate Equation (1), replacing $\textit{Post Disclosure}_{i,m}$ with binary variables that capture monthly time leads and lags, relative to the month of the firm's initial pay ratio disclosure (Fowlie et al., 2018; Sandvik et al., 2021). For instance, $\textit{Post Disclosure}_{i,-t}$ equals one for observations that are t months before the month of the initial pay ratio disclosure of firm i, whereas $\textit{Post Disclosure}_{i,+n}$ equals one for observations that are n months after the month of the initial pay ratio disclosure. In Figure 1, we plot the coefficients on $\textit{Post Disclosure}_{i,-t}$ for different values of t and on $\textit{Post Disclosure}_{i,+n}$ for different values of n, as well as 90% confidence intervals. We use the month before the disclosure as the baseline for comparison, with Period 0 (i.e., $t = n = 0$) referring to the month in which the

[22] The differences between pre- and post-disclosure period firm summary statistics observed in Table 2 are likely caused by the change in weighting across firms between the pre- and post-period.

firm first reports its mandatory CEO pay ratio disclosure. The coefficients in the months before the disclosure are not significantly different from zero, which demonstrates parallel pre-trends in monthly Compensation-and-Benefits ratings between treatment and control firms. This result bolsters confidence in the causal inference drawn from the results in Table 3.

4.3 Robustness Tests

To ensure that our results are robust, we examine a variety of alternative specifications. First, a critique of staggered difference-in-differences estimations is that researchers could falsely incorporate the comparison between already treated firms with just treated firms (Barrios, 2021; Baker et al., 2022). This unintended comparison can bias the coefficients, especially when the effect is dynamic (Baker et al., 2022). This issue is more serious when the treatment is a mandatory regulation because the sample does not contain a group of firms that are never treated. We rely on the stacked regression design, proposed by Cengiz et al. (2019), to alleviate these concerns (see their Section III.C. on page 1,443). The idea is that, through estimating event-specific "clean 2 $\times$ 2 DID" (i.e., single event difference-in-differences without using early treated observations as controls) in a staggered dataset, researchers equivalently apply variance weighting to combine the treatment effects efficiently. Because the controls are purposely selected from the "clean" not-yet-treated group, the above bias is avoided (Baker et al., 2022). Practically, we group firms being treated in the same year-month and match them with not-yet-treated firms (with replacement).[23] Then all the matched groups are appended together. By using a set of fully saturated fixed effects (i.e., time-specific treatment groups multiplied by firm fixed effects and time-specific treatment groups multiplied by year-month fixed effects), we can estimate a weighted average of treatment effects that is not contaminated by the problematic comparisons.[24] Column (1) of Table 4 reports the results. Overall, our inference holds: within-firm pay satisfaction increases after the pay ratio

[23]In our sample, all firms eventually get treated. Therefore, when adding not-yet-treated observations as controls, we omit firms' own post-disclosure-period observations. For example, for firms treated in April, the data of firms treated in November are only used as controls up through October. As a result of this matching design, firms treated in January 2019 do not have controls.

[24]The estimation with this fixed structure is equivalent to estimating separate difference-in-differences treatment effects in each group and then taking the average of the acquired treatment effects. Because this method constructs a control group by drawing firms with replacement, the sample size used is larger than that used in our main test in Table 3.

disclosure.

Another potential concern is that, because most firms disclose their initial pay ratios in March or April 2018 (see Table A.1), the number of treated firm-months (66% of the firm-month observations) exceeds the number of control firm-months in our sample. To ensure that our findings are unaffected by such an imbalance, we conduct a fixed-window difference-in-differences regression. We expand the sample period to include firm-months in a fixed window period, up to 12 months before and 12 months after the initial pay ratio disclosures.[25] This approach increases the sample size by including more firm-months of data, and it yields a more balanced sample, with 52% treatment firm-months and 48% control firm-months. We then re-estimate Equation (1). As reported in Column (2) of Table 4, the coefficient on *Post Disclosure* is essentially the same as our main result in Table 3, and it is statistically significant at the 5% level.

As a final robustness test, we re-estimate our model after back-filling firm-months with missing Glassdoor ratings data. As we mention in Section 3, 17% of the firm-month observations do not have Compensation-and-Benefits ratings. This occurs whenever the firm does not have any employees rate them in a given month. Our main specification uses a sample filtering condition that requires firms to have ratings data at least once in the three months before and once in the three months after the initial pay ratio disclosure. However, they are not required to have nonmissing ratings data for all firm-months. Thus we conduct an additional test using a sample that fills in the missing firm-month ratings values with the most recently available data from the previous months (only when the value of *Post Disclosure* is the same for the two firm-month observations). The implicit assumption is that no new ratings imply the persistence of the old ratings. This increases the sample size, relative to the sample used in our main test, because of the missing ratings data being back-filled. Column (3) of Table 4 presents the regression results. The results continue to support our main finding that within-firm employee pay satisfaction increases after the pay ratio disclosure.

[25] In untabulated analyses, we also examine a three-month window before and after the pay ratio disclosure using an industry fixed effects model and find that our main results still hold. Because of insufficient intertemporal variation within a firm, we cannot infer significance in shorter window analyses when using firm fixed effects.

4.4 Ruling Out Alternative Explanations

The results thus far show that employee pay satisfaction changes after the pay ratio disclosure. To further demonstrate that employees are responding to the increased pay transparency and not some other factor, we perform several tests to rule out alternative explanations. First, we address the possibility that employees are responding to other information in firms' proxy statements. To do this, we construct a falsification test that examines whether ratings change after the filing of proxy statements two years before those that contained the initial pay ratio.[26] If other information in the proxy statements drives our results and not median employee pay information, we should observe similar employee pay satisfaction responses in the years before the ratio's disclosure. In Column (1) of Table 5, we find an insignificant effect of *Post Disclosure* on the Compensation-and-Benefits ratings, suggesting that other information reported in the proxy statement does not drive our main findings. To strengthen this conclusion, we also use an alternative window from November 2016 to February 2018, using the 2017 proxy statements of firms that disclosed their pay ratios in either March or April (77% of our sample firms). The results are tabulated in Column (2) of Table 5. We do not find a significant effect on pay satisfaction around these pre-disclosure proxy statement filings, further suggesting that other proxy statement information does not drive our main results.

To ensure we are not capturing a general increasing trend in employee satisfaction, we consider the effect of the pay ratio disclosure on an alternative Glassdoor rating that is unrelated to pay satisfaction. We examine Work-Life-Balance ratings under the assumption that an employee's satisfaction with workload, schedule, and the ability to enjoy leisure time is unlikely to change when that person learns about median employee pay information. We expect an insignificant estimate on *Post Disclosure* when a firm's Work-Life-Balance rating is the dependent variable in an estimation of Equation (1). Column (3) of Table 5 reports results that align with this expectation. The estimate on *Post Disclosure* is statistically insignificant, and the magnitude of the coefficient is small, providing increased confidence in the inferences drawn from our main results.

Next, our results might be explained by external pressure on employees, from managers, to post positive ratings on Glassdoor after the pay ratio disclosure. Managers might have encouraged

[26]The two-year lag enables us to avoid any proxy statements that may overlap with the other event windows. Some firms voluntarily disclosed pay ratio information before the mandated time, but this occurs in less than 1% of the firms in our sample.

employees to post positive ratings to prevent negative publicity. This explanation would imply an increase in the number of ratings submitted after the pay ratio disclosure. To test this, we estimate the impact of the pay ratio disclosure on the number of ratings posted each month. Column (4) of Table 5 reports the regression results. We do not observe a significant change in the number of posts after the initial pay ratio disclosures, which mitigates this concern.

The null effects on Work-Life-Balance ratings in Column (3) and on the number of ratings in Column (4) also assuage the concern that the attrition of disgruntled employees drives our main results. In the pre-disclosure period, Work-Life-Balance and Compensation-and-Benefits ratings are highly correlated ($\rho = 0.51$), so if the positive effect on Compensation-and-Benefits ratings is simply due to there being fewer unhappy employees, then we would expect to see changes in Work-Life-Balance ratings as well, which we do not observe. Further, deHaan et al. (Forthcoming) document that Glassdoor submissions increase when employees leave the firm, as they have to post about their experience at their former employer to access data on other employers. The stability in the number of ratings submitted before and after the pay ratio disclosure suggests that there was not an increase in the number of employees seeking to leave after the pay ratio disclosure. While we cannot completely dismiss the turnover of dissatisfied employees as a possible explanation for a portion of the estimated treatment effect, these results suggest that attrition is not the main driver of our findings.

Finally, because private firms are not subject to the new pay ratio disclosure requirements, we examine trends in the Compensation-and-Benefits ratings of private firms to rule out the possibility that general increases in pay satisfaction are driving our results. We identify private firms with revenues greater than $100 million from Capital IQ. Matching them to Glassdoor data, we perform an analysis akin to our main tests with two modifications. First, because these firms do not have a clear post-disclosure period, as they do not provide a pay ratio disclosure, we create an indicator for ratings after April 2018, as the vast majority of public firms disclosed in either March or April of 2018. Second, as we do not have comprehensive financial data for private firms, we are unable to include control variables in these analyses, but we do include firm fixed effects. As shown in Column (5) of Table 5, we do not document a significant coefficient on *Post*[27]. These findings

[27]This private firms results are also robust to the use a May indicator instead of an April one.

provide additional assurance that our main results are not driven by secular trends in ratings but instead driven by the pay ratio disclosures.

5 Mechanism Analysis

We conjecture that the increases in pay satisfaction following the pay ratio disclosures are driven by employees using the newly disclosed pay information to recalculate their reference wages. Our finding that pay satisfaction increased suggests that reference wages fell after the disclosure. While we cannot directly observe employees' reference wages, the following tests provide suggestive evidence about which information in the pay ratio is driving the effects documented in Section 4 and about how an employee's pre-existing pay information environment moderates their pay satisfaction response to the disclosures.

5.1 Decomposing the Pay Ratio

We first examine which elements of the pay ratio are informative to employees. We do this by decomposing the pay ratio into its two parts: the median employee pay and the CEO pay. Since the median employee's pay information is the only strictly new part of the disclosure, we predict that it should have the largest impact on pay satisfaction. If changes in employees' reference wages drive our pay-satisfaction findings, then we should see that, the lower the median employee's pay is, the greater the impact the pay ratio disclosure has on employee pay satisfaction. To test this, we re-estimate Equation (1) including the interaction of *Post Disclosure* and *Low Median Employee Pay*, which equals one for firms with below sample-wide median employee pay and zero otherwise. We report the results in Column (1) of Table 6. We find a positive and significant estimate on *Post Disclosure* $\times$ *Low Median Employee Pay*, indicating that the increase in pay satisfaction was greatest among employees of firms with a relatively low value of median employee pay. The estimate on *Post Disclosure* is positive, but it is statistically insignificant, which suggests that employees of firms with a relatively high value of median employee pay had muted responses to the disclosure. These findings are consistent with the notion that observed increases in pay satisfaction are driven by employees whose reference wages decrease when they incorporate median employee pay into the

calculation.

Theoretically, disclosing a new, lower level of CEO pay could also increase employee pay satisfaction by lowering reference wages. We argue that this is less likely, because (1) CEO pay has been disclosed for decades, meaning that it was likely already impounded in reference wage calculations, and (2) recent academic evidence shows that CEO pay did not decrease in response to the pay ratio disclosures (Chang et al., 2022). To verify our belief that median employee pay information and not CEO pay information is driving our results, we re-estimate Equation (1) by simultaneously including into the model *Post Disclosure* × *Low Median Employee Pay* and *Post Disclosure* × *Low CEO Pay*, where *Low CEO Pay* equals one for firms with below sample-wide median CEO compensation and zero otherwise. Column (2) of Table 6 reports the results. We continue to estimate a positive and significant coefficient on *Post Disclosure* × *Low Median Employee Pay*, and the magnitude of the effect resembles that in Column (1). In contrast, we find an insignificant estimate on *Post Disclosure* × *Low CEO Pay*. These findings confirm that it is the disclosure of median employee pay information, not CEO pay information, that drives our main results.

An alternative explanation could be that lower median employee pay is associated with a higher pay ratio, and high pay ratios could drive the observed increase in pay satisfaction, as suggested by the tournament theory literature (Eriksson, 1999; Mueller et al., 2017). As a result, our findings in Columns (1) and (2) of Table 6 could be driven by high pay ratios rather than by low median employee pay levels. To help rule out this explanation, we include in the model *Post Disclosure* × *High Pay Ratio*, where *High Pay Ratio* equals one for firms with above sample-wide median pay ratios and zero otherwise, in addition to *Post Disclosure* × *Low Median Employee Pay*. Column (3) of Table 6 displays the results. The small, insignificant estimate on *Post Disclosure* × *High Pay Ratio* indicates that the magnitude of the pay ratio has no effect on how employees respond to the pay ratio disclosure. The estimate on *Post Disclosure* × *Low Median Employee Pay*, on the other hand, continues to be positive and statistically significant. This further indicates that our main results are driven by employees' responses to median employee pay information, which provides them with a new factor to use when calculating their reference wages.

5.2 Heterogeneity in Pay Information Environments

After decomposing the pay ratio into its parts, we examine how an employee's pay information environment before the disclosure affects their response to the pay ratio disclosure. We predict that employees who have relatively lower (higher) levels of pay information at their disposal will find the new information relatively more (less) informative. We test this prediction in two ways. First, we examine firm-level factors that impact an employee's pay information environment. Second, we examine employee-level factors.

5.2.1 Firm-Level Differences in Pay Information Environments

We use four firm-level measures to proxy for the amount of pay information employees had access to before the disclosure. First, we count the number of Compensation-and-Benefits ratings posted about the employees' companies over the three months before the initial pay ratio disclosure is publicized, and we take the logarithm of one plus this value, *Log Employee Ratings*.[28] The intuition here is that more ratings on Glassdoor provide employees with more information about the firm's compensation practices, rendering the pay ratio disclosure less informative.

Second, the media may play an important role in disseminating labor-related information to rank-and-file employees. If labor-related news abounded before the initial pay ratio disclosure was publicized, this will attenuate employees' responses to the disclosure. We identify labor-related news volume using RavenPack, which provides detailed data on the media coverage of the firms in our sample,[29] and we create the variable *Log Media Coverage*, which equals the logarithm of one plus the number of labor-related news articles published about the company in the year before the firm's disclosure.

Third, employees may learn about local pay practices by interacting with workers from same-industry firms. To account for this, we count the number of same-industry firms with headquarters located within 20 miles of the headquarters of the focal firm, and we take the logarithm of one plus

[28] We use the logarithm transformation due to skewness in the distribution of the raw data.

[29] We require relevance scores (i.e., a measure that RavenPack provides to quantify how strongly the company relates to the underlying news story) to be equal to 100 for group types classified as "labor-issues," and we delete press releases and tabular material that are less likely to contain editorial content (Bonsall IV et al., 2020).

this value, *Log Industry Firms*. We extract firm location (zip code) and industry data (three-digit SIC) from EDGAR and use the NBER zip code distance database to calculate the distance between firms.[30]

Fourth, we count the number of peer firms that disclose their pay ratio information *before* the focal firm. An employee's pay information environment might also expand as they learn about the median employee pay of peer firms. We identify each company's compensation peers using data from Institutional Shareholder Services (ISS), which provides a list of firms chosen as the compensation benchmarks when setting executive pay.[31] We construct a continuous variable, *Log Peer Announcers*, which equals the logarithm of one plus the number of compensation peers that disclosed their initial pay ratio information before the focal firm.

We interact each of these four variables with *Post Disclosure* and separately re-estimate Equation (1) with the inclusion of each interaction term. Table 7 reports the regression results. Across all four columns, *Post Disclosure* is statistically significant, which captures our main finding that pay satisfaction increases in response to the pay ratio disclosure. In Column (1), the negative and statistically significant estimate on *Post Disclosure* × *Log Employee Ratings* suggests that the pay satisfaction effect is attenuated among firms that have more pre-disclosure ratings on Glassdoor. Similarly, Columns (2) and (3) show that the effect shrinks among firms with more pre-disclosure labor-related news coverage and those with more close-proximity same-industry firms, respectively. The estimate on the interaction term in Column (4), which considers the differential effect among firms with more compensation peers who have already disclosed their pay ratio information, is also negative but statistically insignificant.[32] This perhaps speaks to the fact that pay ratio disclosures are not easily comparable between firms, even peers, because the human capital structures of firms can be quite different (LaViers et al., 2022). Taken together, the results in Table 7 are consistent with our prediction that the pay satisfaction effect should be weaker among employees who had relatively more information about their firm's compensation practices. These findings bolster our

[30] Source: https://www.nber.org/research/data/zip-code-distance-database.

[31] This data only covers firms in the S&P 1500, so our sample size decreases from 18,690 to 15,382.

[32] *Log Employee Ratings*, *Log Media Coverage*, and *Log Industry Firms* are not time-varying, so they are subsumed by firm fixed effects in Columns (1)–(3). *Log Peer Announcers* is time-varying, which is why we see *Log Peer Announcers* as a control in Column (4). Our results are similar if we use an alternative non-time-varying measure of *Log Peer Announcers* that captures the number of peers that have disclosed their pay ratios at the time of the focal firm's disclosure.

inference that the mechanism behind the observed increase in pay satisfaction relates to changes in reference wages as a result of employees incorporating median employee pay information into their reference wage calculations.

5.2.2 Employee-Level Differences in Pay Information Environments

Next, we use two employee-level measures to proxy for the amount of pay information employees had access to before the disclosure: managerial status and tenure. First, we posit that managers will have a more muted reaction to the pay ratio disclosures than will rank-and-file employees, as managers already have access to information about their direct-reports' compensation levels, whereas rank-and-file employees usually do not have access to information about other workers' pay levels. To distinguish the Glassdoor raters as either managers or non-managers (i.e., rank-and-file workers), we search the raters' job titles for "manager," "executive," "director," or "supervisor." We then aggregate firm-month average values of Compensation-and-Benefits ratings separately for managers and non-managers.

Second, employees newer to the firm are likely at a disadvantage when it comes to information about compensation practices. They are still learning about and fitting into their new work environment and, consequently, may have less informed reference wages. As a result, they will be impacted more strongly by the information in the pay ratio disclosures. Therefore, we expect that the impact of pay ratio disclosures will be stronger for short-tenured employees compared to long-tenured employees. We split raters into two groups, those with tenure less than one year and those with tenure greater than or equal to one year, and we aggregate firm-month average pay satisfaction values within each group.

Having formed subsamples of firm-month data based on the managerial status and tenure of each rater, we re-estimate Equation (1) within each subsample. Table 8 reports the results of these subsample estimations.[33] Consistent with our predictions, we find that the effect of the pay ratio disclosures on employee pay satisfaction is significant among non-managers and short-tenured employees, whereas the effect is not significant among managers and long-tenured employees. These

[33]The subsamples are smaller than 18,690, the size of the sample used in our main tests, because (1) not all reviews possess the relevant information needed to partition the data, and (2) some firm-months do not have ratings from both types of employees.

findings bolster the argument that the disclosure of median employee pay information can offer useful information to calculate reference wages, especially for employees who had with relatively less pay information available to them before the disclosure.

5.3 Effect on Ratings Dispersion

As a final test to help substantiate our interpretation that the pay ratio disclosures affected employee pay satisfaction, we examine the change in ratings dispersion after the new disclosure. Increased information about the pay of workers in the firm should improve the precision of employees' estimated reference wages. While we cannot observe those estimates, we can observe how the pay ratio disclosure affected the dispersion of pay satisfaction ratings. Intuitively, two similarly paid employees should have more similar estimates of reference wages after median employee pay is disclosed, and they should therefore have more similar pay satisfaction ratings. As a result, the dispersion of Compensation-and-Benefits ratings should decrease.

To test this, we re-estimate our main regression specifications with an alternative dependent variable, the standard deviation of the firm's Compensation-and-Benefits ratings in a given month.[34] The results are tabulated in Table 9, where Columns (1) and (2) employ the same specification used in Columns (2) and (3) of Table 3, respectively, and where Column (3) employs the same difference-in-differences robustness specification used in Column (1) of Table 4. We find that within-firm ratings dispersion decreases after firms first disclose their pay ratio and median employee pay information. This finding is consistent with the pay ratio disclosures improving the information environment of workers, causing greater similarity in their calculation of reference wages.

6 Conclusion

We document the benefits of increased pay transparency for employee pay satisfaction. Models of employee utility suggest that a comparison of one's pay to a reference wage determines pay satisfaction. We posit that, when additional pay information becomes available to employees through

[34] As standard deviations cannot be estimated in firm-months with only a single rating, our sample size decreases from 18,690 to 14,062.

the pay ratio disclosure, employees recalculate their reference wages, causing their pay satisfaction to rise. We test this using market-wide pay satisfaction data from Glassdoor and document a significant increase in within-firm employee pay satisfaction after the first year's disclosure of pay ratio information. We conjecture that this increase in pay satisfaction is likely the result of workers adjusting their reference wages downward after they learn about the pay of the median employee in their firm.

To provide evidence that reductions in employees' reference wages led to the observed increase in pay satisfaction, we show that the level of median employee pay, not the level of CEO pay or the magnitude of the pay ratio itself, drives our results. We also document that the increase in pay satisfaction is stronger for employees who have lower levels of pay information available to them prior to the disclosure. This finding is consistent with the notion that employees with relatively less compensation-related information prior to the disclosure likely relied more on the new median employee pay information as they re-estimated their reference wages. We find that the pay satisfaction effect is driven by ratings from employees who work at firms with less overall transparency about pay and from non-managers and short-tenured employees. We also show that, in addition to increasing within-firm pay satisfaction, the pay ratio disclosures led to reduced dispersion in the pay satisfaction among employees, likely because employees could all include the same median employee pay information into their reference wage calculations.

While our results contribute to both the pay transparency and CEO pay ratio literatures, they do have limitations. First, many of the companies we study are international, with employees on multiple continents. Glassdoor is based in the U.S. and is written in English, which potentially limits the number of international employees who can submit ratings. Second, we cannot directly observe the reference wages of employees before or after pay ratio disclosure. Though theory predicts and our empirical tests indicate that the mechanism behind the pay satisfaction effect is the reduced reference wages of employees, future research on pay transparency changes should be conducted in settings where reference wages are directly measurable.

Our findings are important to academics, practitioners, and regulators. They shed light on the effects of mandatory pay transparency regulation in the marketplace, and they show that the effects of increased pay transparency depend on the pay information environment that employees

are already operating in. In addition, our findings can help managers anticipate what the effects may be of increased pay transparency in their firms. Last, our findings can inform regulators who are currently debating the merits of increased human capital management disclosures, as our results highlight the effects of increased pay transparency on the labor market.

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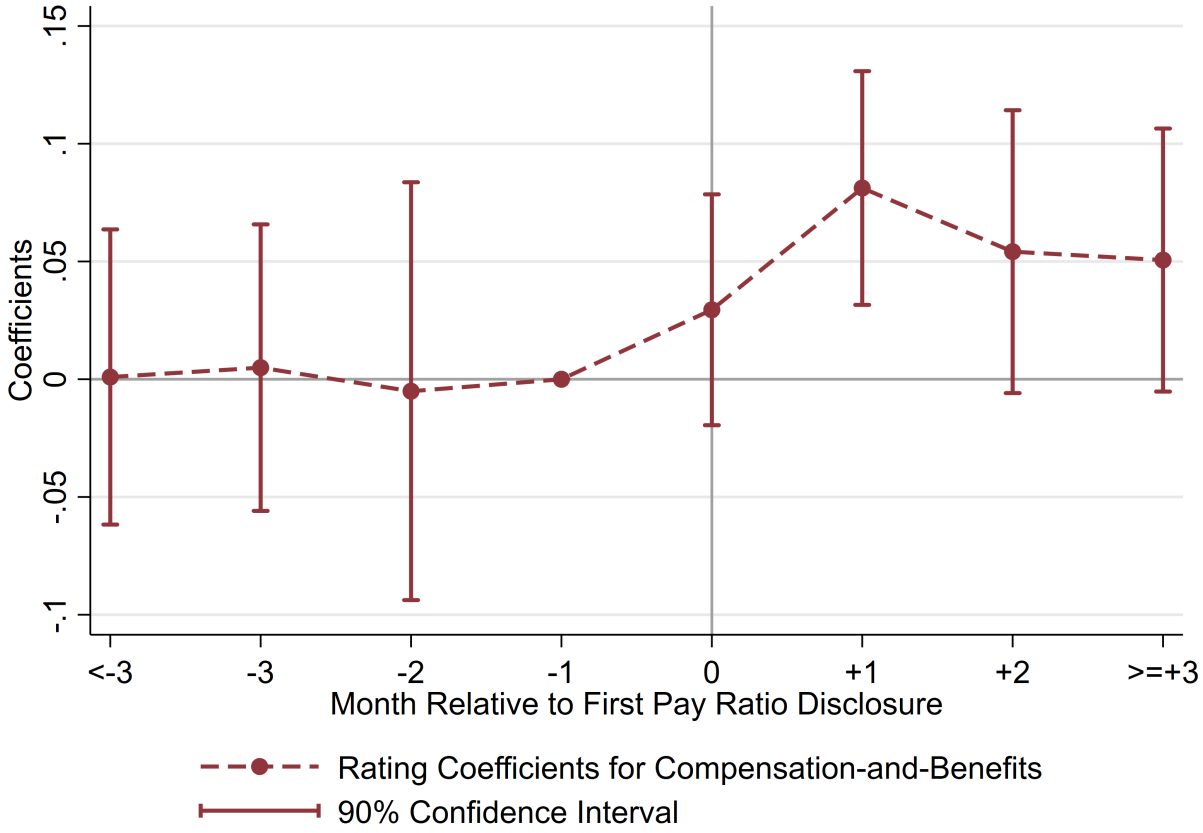
Figures and Tables

Figure 1: **Parallel Trends**



Notes. This figure examines the parallel trends assumption underlying the results in Table 3. We re-estimate Equation (1), replacing *Post Disclosure*$_{i,m}$ with binary variables that capture monthly time leads and lags relative to the month of the firm's initial pay ratio disclosure (Fowlie et al., 2018; Sandvik et al., 2021). For instance, *Post Disclosure*$_{i,-t}$ equals one for observations that are t months before the month of the initial pay ratio disclosure of firm i, whereas *Post Disclosure*$_{i,+n}$ equals one for observations that are n months after the month of the initial pay ratio disclosure. We then plot the coefficients on *Post Disclosure*$_{i,-t}$ for different values of t and on *Post Disclosure*$_{i,+n}$ for different values of n, as well as 90% confidence intervals. We use the month before the disclosure as the baseline for comparison, with Period 0 (i.e., $t = n = 0$) referring to the month in which the firm discloses its initial CEO pay ratio information.

Table 1: **Summary Statistics**

	Observations	Mean	St. Dev.	P25	P50	P75
Glassdoor Outcomes:						
Log Number of Ratings	22,611	1.63	1.21	0.69	1.39	2.40
Compensation-and-Benefits Rating	18,690	3.36	0.86	2.95	3.43	4.00
Work-Life-Balance Rating	18,675	3.26	0.93	2.75	3.32	4.00
Non-manager Comp-and-Benefits Rating	18,112	3.35	0.88	2.90	3.40	4.00
Manager Comp-and-Benefits Rating	8,476	3.49	1.03	3.00	3.67	4.00
Short Tenure Comp-and-Benefits Rating	9,381	3.42	1.13	2.80	3.50	4.00
Long Tenure Comp-and-Benefits Rating	16,180	3.34	0.93	2.86	3.40	4.00
Compensation-and-Benefits Dispersion	14,062	1.09	0.44	0.82	1.13	1.36
Initial Pay Ratios:						
Post Disclosure	22,611	0.66	0.47	0.00	1.00	1.00
Pay Ratio	1,362	196.75	361.40	53.00	99.00	194.00
CEO Pay (000s)	1,362	8,196	8,490	3,377	6,093	10,501
Median Employee Pay	1,362	69,305	45,495	40,814	59,519	89,851
Control Variables:						
Log Assets	8,813	8.37	1.59	7.24	8.26	9.38
Num of Segments	8,813	1.59	0.91	1.00	1.00	2.00
Intangible Asset	8,813	0.24	0.23	0.03	0.18	0.40
Book to Market	8,813	0.44	0.38	0.18	0.36	0.62
Log Firm Age	8,813	5.49	0.84	5.00	5.61	6.05
Log Analyst Coverage	8,813	2.43	0.77	1.95	2.48	3.00
Institutional Shareholding	8,813	0.80	0.19	0.72	0.84	0.93
Earnings Surprise	8,813	-0.00	0.04	-0.01	0.00	0.01
Cash Holding	8,813	0.14	0.16	0.03	0.07	0.18
Variables for Cross-sectional Tests:						
Log Number of Previous Ratings	22,611	1.72	1.17	0.98	1.54	2.37
Log Previous Media Coverage	22,611	1.06	0.94	0.00	1.10	1.61
Log Same-Industry Firms	22,611	0.81	1.14	0.00	0.00	1.39
Log Cumulative Peer Announcers	18,287	0.73	1.17	0.00	0.00	2.08
Glassdoor Contributor Details:						
Managerial Status	244,947	0.13	0.34	0.00	0.00	0.00
Short Tenure Status	244,947	0.15	0.36	0.00	0.00	0.00
Yearly Pay	111,944	67,911	615,409	28,000	50,000	84,000
Yearly Pay / Median Employee Pay	111,944	1.74	12.17	0.71	1.09	1.85
Variables for Performance Test:						
Δ ROA	59,098	0.04	2.51	-0.51	0.01	0.54
Δ Compensation-and-Benefits	64,236	0.01	0.76	-0.35	0.00	0.38

Notes. This table reports the summary statistics of the data we used. Variable definitions are provided in Table A.3 in the Appendix.

Table 2: **Univariate Comparison**

	Post Disclosure = 0		Post Disclosure =1			
	Mean	Std	Mean	Std	Mean Diff	Sig.
Compensation-and-Benefits Rating	3.31	0.86	3.39	0.87	-0.08	***
Work-Life-Balance Rating	3.23	0.92	3.28	0.94	-0.04	***
Compensation-and-Benefits Dispersion	1.08	0.43	1.09	0.44	0.00	
Log Assets	8.29	1.59	8.43	1.59	-0.14	***
Num of Segments	1.59	0.90	1.60	0.92	0.00	
Intangible Asset	0.25	0.23	0.24	0.23	0.01	***
Book to Market	0.42	0.36	0.45	0.39	-0.04	***
Log Firm Age	5.49	0.87	5.51	0.83	-0.03	**
Log Analyst Coverage	2.43	0.77	2.43	0.77	0.00	
Institutional Shareholding	0.79	0.20	0.80	0.18	-0.01	***
Earnings Surprise	0.00	0.04	0.00	0.04	0.00	***
Cash Holding	0.14	0.16	0.13	0.16	0.01	***
Yearly Pay	65,731	496,215	70,275	722,770	4,545	
Yearly Pay / Median Employee Pay	1.69	9.53	1.80	14.49	0.11	

Notes. This table compares the variables used in our subsequent empirical tests between treatment and control. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively. Variable definitions are provided in Table A.3 in the Appendix.

Table 3: **CEO Pay Ratio Disclosure and Employee Pay Satisfaction Ratings**

Dependent Variable =	(1) Compensation-and-Benefits	(2) Compensation-and-Benefits and-Benefits	(3) Compensation-and-Benefits and-Benefits
Post Disclosure	0.076***	0.050**	0.049**
	(6.74)	(2.69)	(2.66)
Log Assets			0.060
			(1.21)
Number of Segments			0.021
			(0.54)
Intangible Asset			-0.123
			(-0.51)
Book to Market			0.033
			(0.59)
Log Firm Age			0.065
			(0.29)
Log Analyst Coverage			0.019
			(0.44)
Institutional Shareholding			0.077
			(1.16)
Earnings Surprise			0.211
			(1.42)
Cash Holding			-0.139
			(-0.94)
Observations	18690	18690	18690
R-squared	0.00	0.34	0.34
Firm Fixed Effect	No	Yes	Yes
Year-Month Fixed Effect	No	Yes	Yes

Notes. This table reports results from a difference-in-differences regression of firms' Compensation-and-Benefits ratings on the indicator variable *Post Disclosure*, which equals one in the month that the firm discloses its initial CEO pay ratio and each subsequent month, and equals zero in the months before the disclosure. We also control for several additional firm characteristics, as well as time (year-month) and firm fixed effects. Variable definitions are provided in Table A.3 in the Appendix. We double cluster standard errors by year-month and firm, and we report t-statistics in parentheses. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Table 4: **Robustness Tests**

Dependent Variable =	(1) Compensation-and-Benefits	(2) Compensation-and-Benefits	(3) Back-Filled Compensation-and-Benefits
Post Disclosure	0.045**	0.050**	0.065**
	(2.46)	(2.66)	(2.27)
Size	0.058	0.013	0.079
	(1.61)	(0.29)	(1.21)
Number of Segments	0.083***	-0.021	0.049
	(2.93)	(-0.66)	(1.10)
Intangible Asset	-0.178	-0.116	-0.245
	(-1.32)	(-0.64)	(-0.95)
Book to Market	0.125***	0.004	-0.051
	(2.60)	(0.10)	(-0.75)
Firm Age	0.136	0.050	0.162
	(0.91)	(1.34)	(0.58)
Analyst Coverage	0.126***	-0.005	-0.054
	(2.65)	(-0.11)	(-0.69)
Institutional Shareholding	0.078*	0.135**	0.079
	(1.73)	(2.16)	(0.94)
Earnings Surprise	-0.075	0.127	0.225
	(-0.73)	(1.05)	(1.64)
Cash Holding	-0.358***	0.022	-0.057
	(-3.16)	(0.16)	(-0.29)
Observations	43776	27211	24756
R-squared	0.39	0.33	0.35
Group × Firm Fixed Effect	Yes	No	No
Group × Year-Month Fixed Effect	Yes	No	No
Firm Fixed Effect	No	Yes	Yes
Year-Month Fixed Effect	No	Yes	Yes

Notes. This table reports the results of three robustness tests. In Column (1), we follow the approach in Cengiz et al. (2019) by estimating a staggered difference-in-differences regression that addresses the potential for bias due to the comparison between the previously treated groups and the newly treated groups. Because this method selects from the sample of control firms with replacement, the sample size increases. In Column (2), we use a fixed window sample around CEO pay ratio disclosure dates, so twelve months of pre- and post-disclosure data are included for all disclosing firms, leading to a sample that is larger than the sample used in our main test. In Column (3), we use a sample that accounts for back-filling in the missing firm-month ratings values with the most recently available data from the previous month (only when the two firm-months are both non-treated or treated). The implicit assumption is that no new ratings imply the persistence of the old ratings. This allows us to retain more of the data, again leading to a sample that is larger than the sample used in our main test. Variable definitions are provided in Table A.3 in the Appendix. We double cluster standard errors by year-month and firm, and we report t-statistics in parentheses. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Table 5: **Ruling Out Alternative Explanations**

Dependent Variable =	(1) Compensation-and-Benefits	(2) Compensation-and-Benefits	(3) Work-Life-Balance	(4) Log Number of Ratings	(5) Compensation-and-Benefits
Post Disclosure	0.014	0.025	0.007	-0.017	-0.011
	(0.77)	(1.13)	(0.35)	(-1.21)	(-0.72)
Log Assets	-0.059	-0.031	0.036	0.187***	
	(-0.68)	(-0.38)	(0.54)	(4.58)	
Number of Segments	0.039	0.027	0.039	0.020	
	(0.98)	(0.28)	(0.75)	(0.95)	
Intangible Asset	-0.090	0.412	-0.061	-0.273*	
	(-0.36)	(1.44)	(-0.22)	(-1.81)	
Book to Market	-0.004	-0.084	-0.030	0.031	
	(-0.05)	(-1.36)	(-0.57)	(1.10)	
Log Firm Age	0.108	-0.238	0.008	0.113	
	(1.64)	(-0.96)	(0.03)	(0.97)	
Log Analyst Coverage	-0.087	-0.060	-0.074	0.048	
	(-1.52)	(-0.71)	(-1.66)	(1.49)	
Institutional Shareholding	-0.114*	-0.118	0.063	0.097*	
	(-1.86)	(-1.10)	(0.64)	(1.93)	
Earnings Surprise	0.136	0.031	0.142	-0.074	
	(0.61)	(0.09)	(0.81)	(-1.01)	
Cash Holding	0.019	0.492	0.076	-0.359**	
	(0.10)	(1.46)	(0.36)	(-2.70)	
Observations	14942	10588	18675	22611	22448
R-squared	0.43	0.41	0.27	0.87	0.33
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year-Month Fixed Effect	Yes	Yes	Yes	Yes	No

Notes. The results in this table help to rule out several alternative explanations of our main results in Table 3. Column (1) reports the results from a falsification test that examines whether ratings change after the filing of proxy statements two years before those which contained the initial CEO pay ratio. Column (2) reports the results from a falsification test that uses March or April filers' previous proxy statement dates as the pseudo-events. The sample is constrained to a period from November 2017 to February 2018. Column (3) reports the results from a difference-in-differences regression of firms' Work-Life-Balance ratings, instead of Compensation-and-Benefits ratings, on *Post Disclosure*, using our main sample period. Column (4) reports the results from a difference-in-differences regression in which we replace the dependent variable with the logarithm of the number of ratings posted about the firm each month. Column (5) reports the results by using a sample of private firms whose *Post Disclosure* is set as May 2018 when most of our sample firms had announced their initial pay ratio disclosures. Variable definitions are provided in Table A.3 in the Appendix. We double cluster standard errors by year-month and firm, and we report t-statistics in parentheses. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Table 6: **Decomposing The CEO Pay Ratio**

Dependent Variable =	(1) Compensation-and-Benefits	(2) Compensation-and-Benefits	(3) Compensation-and-Benefits
Post Disclosure × Low Median Employee Pay	0.041*	0.042*	0.047*
	(1.79)	(1.82)	(1.91)
Post Disclosure × Low CEO Pay		-0.006	
		(-0.18)	
Post Disclosure × High Pay Ratio			-0.027
			(-1.01)
Post Disclosure	0.025	0.027	0.039
	(1.02)	(1.03)	(1.36)
Log Assets	0.063	0.064	0.059
	(1.27)	(1.27)	(1.20)
Number of Segments	0.020	0.020	0.020
	(0.51)	(0.51)	(0.51)
Intangible Asset	-0.121	-0.120	-0.124
	(-0.50)	(-0.50)	(-0.51)
Book to Market	0.031	0.031	0.032
	(0.56)	(0.55)	(0.58)
Log Firm Age	0.077	0.081	0.055
	(0.34)	(0.35)	(0.25)
Log Analyst Coverage	0.022	0.021	0.020
	(0.49)	(0.49)	(0.45)
Institutional Shareholding	0.072	0.071	0.073
	(1.08)	(1.03)	(1.09)
Earnings Surprise	0.215	0.215	0.213
	(1.45)	(1.44)	(1.43)
Cash Holding	-0.141	-0.140	-0.146
	(-0.95)	(-0.94)	(-0.99)
Observations	18690	18690	18690
R-squared	0.34	0.34	0.34
Firm Fixed Effect	Yes	Yes	Yes
Year-Month Fixed Effect	Yes	Yes	Yes

Notes. In this table, we examine which information in the CEO pay ratio disclosures drives our main results. In Column (1), we include into the model the interaction between *Post Disclosure* and *Low Median Employee Pay*, which equals one for firms with below sample-wide median employee compensation, and zero otherwise. In Column (2), we also include into the model the interaction between *Post Disclosure* and *Low CEO Pay*, which equals one for firms with below sample-wide median CEO compensation, and zero otherwise. In Column (3), we remove the *Post Disclosure* × *Low CEO Pay* term and include the interaction between *Post Disclosure* and *High Pay Ratio*, which equals one for firms with above sample-wide median CEO pay ratios, and zero otherwise. Variable definitions are provided in Table A.3 in the Appendix. We double cluster standard errors by year-month and firm, and we report t-statistics in parentheses. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Table 7: **Firm-Level Differences in Pay Information Environments**

Dependent Variable =	(1) Compensation-and-Benefits	(2) Compensation-and-Benefits	(3) Compensation-and-Benefits	(4) Compensation-and-Benefits
Post Disclosure × Previous Employee Comments	-0.022**			
	(-2.80)			
Post Disclosure × Previous Media Coverage		-0.016*		
		(-1.97)		
Post Disclosure × Same Industry Firm (Within 20 Miles)			-0.017**	
			(-2.22)	
Post Disclosure × Log Cumulative Peer Announcers				-0.009
				(-0.37)
Post Disclosure	0.093***	0.068***	0.063***	0.071**
	(3.49)	(2.92)	(3.06)	(2.39)
Log Cumulative Peer Announcers				0.005
				(0.16)
Log Assets	0.054	0.054	0.064	0.092*
	(1.07)	(1.07)	(1.28)	(1.93)
Number of Segments	0.023	0.021	0.022	0.033
	(0.58)	(0.54)	(0.55)	(0.69)
Intangible Asset	-0.130	-0.129	-0.129	-0.071
	(-0.54)	(-0.53)	(-0.53)	(-0.28)
Book to Market	0.031	0.034	0.030	0.022
	(0.55)	(0.61)	(0.54)	(0.43)
Log Firm Age	0.045	0.049	0.097	0.196
	(0.20)	(0.22)	(0.42)	(0.65)
Log Analyst Coverage	0.020	0.021	0.022	0.036
	(0.46)	(0.48)	(0.50)	(0.75)
Institutional Shareholding	0.071	0.075	0.074	0.072
	(1.07)	(1.15)	(1.11)	(1.33)
Earnings Surprise	0.208	0.210	0.217	0.171
	(1.39)	(1.41)	(1.45)	(0.93)
Cash Holding	-0.139	-0.135	-0.150	-0.133
	(-0.95)	(-0.91)	(-1.01)	(-0.73)
Observations	18690	18690	18690	15382
R-squared	0.34	0.34	0.34	0.34
Firm Fixed Effect	Yes	Yes	Yes	Yes
Year-Month Fixed Effect	Yes	Yes	Yes	Yes

Notes. The results in this table highlight the impact that employees' pay information before the disclosure has on their pay-satisfaction response to the CEO pay ratio. We use four measures to proxy for the level of employees' pay information. First, we count the number of Glassdoor ratings posted about the employees' companies over the three-month period before the initial pay ratio disclosure is publicized, and we take the logarithm of one plus this value, *Log Employee Ratings*. Second, we create the variable *Log Media Coverage*, which equals the logarithm of one plus the number of labor-related news articles published about the company in the year before the disclosure. Third, we count the number of same-industry firms with headquarters that are located within 20 miles of the headquarters of the focal firm, and we take the logarithm of one plus this value, *Log Industry Firms*. Fourth, we construct *Log Peer Announcers*, which equals the logarithm of one plus the number of compensation peers that disclosed their initial CEO pay ratio information before the focal firm. We interact each of these four variables with *Post Disclosure*, and then we separately re-estimate Equation (1) with the inclusion of each interaction term. Variable definitions are provided in Table A.3 in the Appendix. We double cluster standard errors by year-month and firm, and we report t-statistics in parentheses. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Table 8: **Employee-Level Differences in Pay Information Environments**

	Managerial Status		Tenure in Role	
	(1)	(2)	(3)	(4)
Dependent Variable =	Non-manager	Manager	Short Tenure	Long Tenure
Post Disclosure	0.042**	0.035	0.077*	0.032
	(2.15)	(0.93)	(1.86)	(1.11)
Log Assets	0.129**	-0.202**	0.160	0.009
	(2.75)	(-2.20)	(1.58)	(0.14)
Num of Segments	0.046	-0.045	0.041	0.038
	(1.05)	(-0.89)	(0.64)	(0.86)
Intangible Asset	-0.261	0.835*	0.448	0.155
	(-1.21)	(2.05)	(1.16)	(0.49)
Book to Market	-0.001	-0.042	-0.023	0.078
	(-0.01)	(-0.41)	(-0.21)	(1.10)
Log Firm Age	-0.041	0.182	0.228	-0.173
	(-0.19)	(0.50)	(0.51)	(-0.74)
Log Analyst Coverage	0.005	0.194	0.059	0.039
	(0.09)	(1.47)	(0.57)	(0.88)
Institutional Shareholding	0.072	-0.054	-0.069	0.007
	(0.90)	(-0.47)	(-0.54)	(0.08)
Earnings Surprise	0.139	0.847***	-0.064	0.412**
	(0.83)	(3.25)	(-0.29)	(2.28)
Cash Holding	-0.193	0.253	0.041	0.105
	(-1.52)	(0.78)	(0.12)	(0.42)
Observations	18112	8476	9381	16180
R-squared	0.33	0.34	0.34	0.33
Firm Fixed Effect	Yes	Yes	Yes	Yes
Year-Month Fixed Effect	Yes	Yes	Yes	Yes

Notes. The results in this table highlight the impact that employees' pay information has on their pay-satisfaction response to the CEO pay ratio disclosure. The dependent variables are Compensation-and-Benefits ratings partitioned based on whether the ratings are from non-managers (versus managers) and short-tenured employees (versus long-tenured employees). We define managers based on the keywords search of "manager," "executive," "director," and "supervisor" in the job title. Short-tenured (long-tenured) employees are defined as employees who work in the firm for less (more) than one year. Variable definitions are provided in Table A.3 in the Appendix. We double cluster standard errors by year-month and firm, and we report t-statistics in parentheses. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

Table 9: **Effect on Ratings Dispersion**

Dependent Variable =	(1) Compensation-and-Benefits Dispersion	(2) Compensation-and-Benefits Dispersion	(3) Compensation-and-Benefits Dispersion
Post Disclosure	-0.034***	-0.035***	-0.027**
	(-2.99)	(-3.01)	(-2.36)
Log Assets		0.039	0.028
		(1.11)	(1.14)
Num of Segments		0.031	0.037**
		(1.63)	(2.45)
Intangible Asset		-0.117	-0.117
		(-0.81)	(-1.13)
Book to Market		0.020	0.104***
		(0.55)	(4.06)
Log Firm Age		0.071	-0.029
		(0.41)	(-0.25)
Log Analyst Coverage		0.072	0.103***
		(1.57)	(2.91)
Institutional Shareholding		0.103*	0.041
		(1.81)	(1.44)
Earnings Surprise		0.018	0.101**
		(0.24)	(2.11)
Cash Holding		-0.177	-0.335***
		(-1.65)	(-3.71)
Observations	14062	14062	33839
R-squared	0.18	0.18	0.23
Firm Fixed Effect	Yes	Yes	No
Year-Month Fixed Effect	Yes	Yes	No
Group × Firm Fixed Effect	No	No	Yes
Group × Year-Month Fixed Effect	No	No	Yes

Notes. This table presents the results of regressing Compensation-and-Benefit rating dispersion, defined based on standard deviation, on the indicator variable *Post Disclosure*, which equals one in the month that the firm discloses its initial CEO pay ratio and each subsequent month, and equals zero in the months before the disclosure. Column (1) reports the basic regression following our main difference-in-difference research designs without control variables, whereas column (2) reports the regression results of the fully controlled model. Column (3) implements the staggered difference-in-differences regression that addresses the potential for bias due to the comparison between the previously treated groups and the newly treated groups. Variable definitions are provided in Table A.3 in the Appendix. We double cluster standard errors by year-month and firm, and we report t-statistics in parentheses. Statistical significance is denoted by ***, **, and * for 1%, 5%, and 10%, respectively.

A Appendix: Tables and Figures

Table A.1: **Initial Pay Ratio Disclosure Year-months**

	Frequency	Percentage	Cumulative Percentage
2018 February	11	0.87%	0.87%
2018 March	481	35.32%	36.12%
2018 April	561	41.19%	77.31%
2018 May	48	3.52%	80.84%
2018 June	38	2.79%	83.63%
2018 July	26	1.91%	85.54%
2018 August	23	1.69%	87.22%
2018 September	33	2.42%	89.65%
2018 October	30	2.20%	91.85%
2018 November	16	1.17%	93.02%
2018 December	47	3.45%	96.48%
2019 January	48	3.52%	100.00%
Total	1,362		

Notes. This table presents the event year-month distributions of the final sample.

Table A.2: **Initial Pay Ratio Disclosure Year-months**

	Number of Firms	Number of Firm-months
Hand-collected initial pay ratio data	2,237	NA
Expand the firms into firm-months (from Nov.2017 to Apr. 2019)	2,064	37,152
Delete firm-months overlapping with next or previous proxy statements	2,064	33,710
Keep firms without loosing controls	2,026	32,969
Require active ratings in both pre- and post periods	1,362	22,611
Final sample without missing Compensation-and-Benefits ratings	1,362	18,690
Alternative samples used in robustness tests		
(1) Staggered regression sample	1,362	43,776
(2) Fixed window sample	1,449	27,211
(3) Filled out missing months sample	1,524	24,756

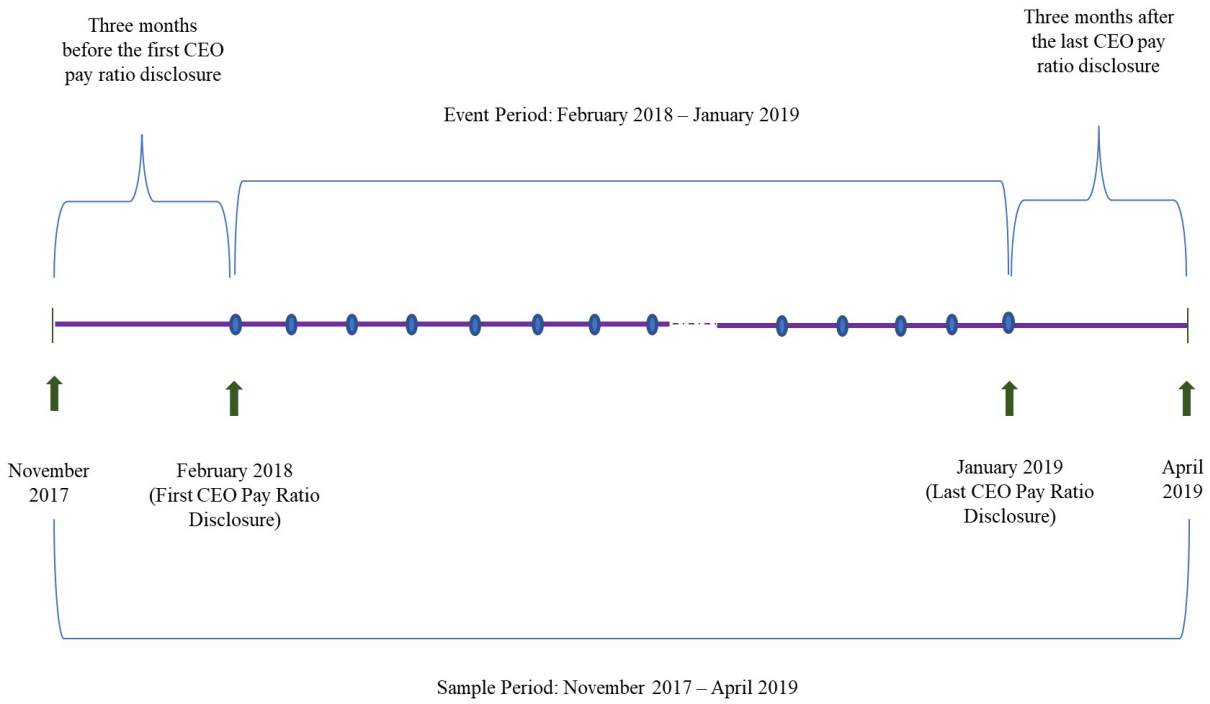
Notes. This table presents our sample selection. Below the final sample of 1,362 firms and 18,690 firm-months, we also report the three alternative samples that are used in the robustness tests in Table 4.

Table A.3: Variable Definitions

Variable	**Variable Definition**	**Data Source**
Glassdoor:		
Log Number of Ratings	The logarithm of one plus the number of employee ratings.	Glassdoor.com
Compensation-and-Benefits Rating	Month average ratings of compensation-and-benefits from Glassdoor.com.	Glassdoor.com
Work-Life-Balance Rating	Month average ratings of work-life balance from Glassdoor.com.	Glassdoor.com
Manager Compensation-and-Benefits	Month average ratings of compensation-and-benefits from employees whose job title descriptions contain keywords "manager", "director", "executive", and "supervisor."	Glassdoor.com
Non-manager Compensation-and-Benefits	Month average ratings of compensation-and-benefits from employees whose job title descriptions do not contain keywords "manager", "director", "executive", and "supervisor."	Glassdoor.com
Long Tenure Compensation-and-Benefits	Month average ratings of compensation-and-benefits from employees who had worked for the firm for more than one year at the moment of review.	Glassdoor.com
Short Tenure Compensation-and-Benefits	Month average ratings of compensation-and-benefits from employees who had worked for the firm for less than one year at the moment of review.	Glassdoor.com
Compensation-and-Benefits Dispersion	Monthly standard deviation of compensation-and-benefits ratings from former employees from Glassdoor.com.	Glassdoor.com
Pay Ratio:		
Post Disclosure	1 if the firm has provided the initial pay ratio, and 0 otherwise.	DEF-14A
Low Median Employee Pay	1 if the firm has a median employee pay that is below the sample median, and 0 otherwise.	DEF-14A
Low CEO Pay	1 if the firm has a CEO pay that is below the sample median, and 0 otherwise.	DEF-14A
High Pay Ratio	1 if the firm has a CEO pay ratio that is above the sample median, and 0 otherwise.	DEF-14A
Controls:		
Log Assets	The logarithm of total assets.	Compustat
Number of Segments	The logarithm of one plus the number of operating industries.	Compustat
Intangible Asset	Intangible assets divided by total assets.	Compustat
Book to Market	Book value of equity divided by the market capitalization.	Compustat

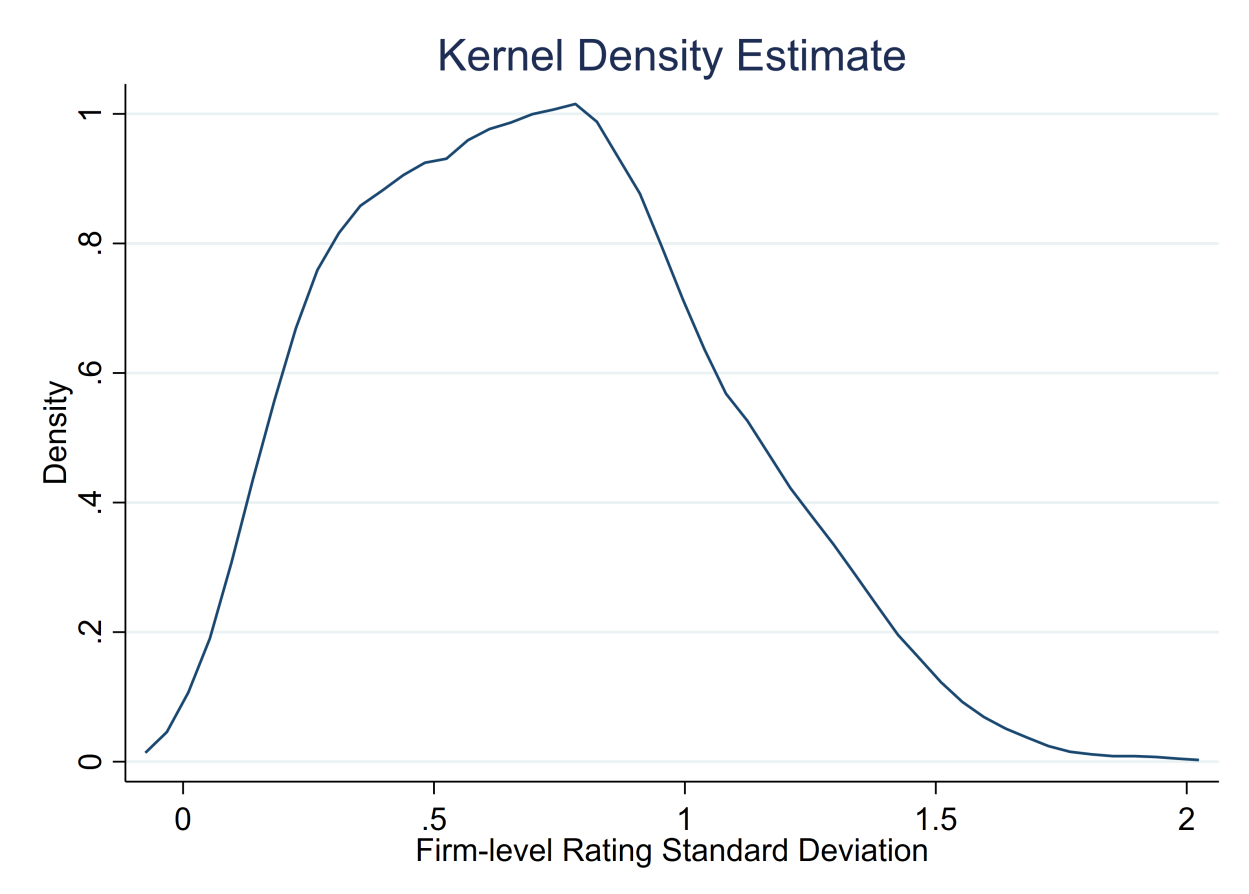
Log Firm Age	The logarithm of one plus the number of months when the firm show up in CRSP.	CRSP
Log Analyst Coverage	The logarithm of one plus the number of analysts making EPS forecasts for the firm during the year.	IBES
Institutional Shareholding	The percentage of institutional shareholding.	Thompson Reuters 13-F
Earnings Surprise	Seasonal difference in quarterly earnings before extraordinary items divided by stock price.	Compustat
Cash Holding	Cash and cash equivalent divided by total assets.	Compustat
Cross-sectional Tests:		
Log Employee Ratings	The logarithm of one plus the number of employee ratings three months before the initial pay ratio disclosures.	Glassdoor.com
Log Media Coverage	The logarithm of one plus the number of labor-related news articles one year before the initial pay ratio disclosure. We require the relevance score to be 100 and only use flash and full articles.	RavenPack
Log Industry Firms	The logarithm of one plus the number of firms headquartered within 20 miles of the focal firm's headquarters and specialized in the same industry.	EDGAR and NBER Website
Log Peer Announcers	The logarithm of one plus the number of peer firms who have already disclosed their initial pay ratios.	DEF-14A
Performance Test:		
Δ ROA	Seasonal difference in return on asset, defined as net income over total assets. Then, we multiply it by 100 for the ease of interpretation.	Compustat
Δ Compensation-and-Benefits	Quarterly difference in Compensation-and-Benefits ratings.	Glassdoor.com

Figure A.1: **Event Period Versus Sample Period**



Notes. This figure illustrates the distinction between the event period and the sample period. The event period starts in February 2018, when the first CEO pay ratio disclosure is reported, and ends in January 2019, when the last CEO pay ratio disclosure is reported. The sample period accounts for all the Glassdoor data and financial controls used in our regression analysis, which extends three months before the first CEO pay ratio disclosure is reported and three months after the last CEO pay ratio disclosure is reported.

Figure A.2: **Within-Firm Variation of Monthly Rating**



Notes. This figure displays the distribution of within-firm variation of monthly average of Compensation-and-Benefits ratings.

B Appendix: Analytical Example

It is helpful to step through an analytical example of when an employee's reference wage is impacted by the median employee pay information reported in the CEO pay ratio disclosure. To do this, we assume that the employee's reference wage before the CEO pay ratio disclosure (i.e., in the pre-disclosure period) is captured by the following linear function:

$$\mathrm{R}_1 = w_1^C \text{CEO Pay} + w_1^O \text{Other Pay},$$

where R_1 is the employee's reference wage in the pre-disclosure period, *CEO Pay* is the annual compensation of the CEO (or more generally some aggregation of the pay of the CEO, CFO, and the three other top earners in the company, which are publicly disclosed), and *Other Pay* is some aggregation of all the other relevant pay information available to the employee. The values w_1^C and w_1^O are the weights placed on *CEO Pay* and *Other Pay*, respectively, and they sum to 1.[35] After the CEO pay ratio disclosure (i.e., in the post-disclosure period), we incorporate a third compensation value into the model, the median employee's pay (*Median Employee Pay*), along with its corresponding weight, w_2^M. While *CEO Pay* and *Other Pay* do not change in the post-disclosure period, their weights might, so we denote the post-disclosure period weights as w_2^C and w_2^O, respectively. Thus, the post-disclosure reference wage, R_2, becomes:

$$\mathrm{R}_2 = w_2^C \text{CEO Pay} + w_2^O \text{Other Pay} + w_2^M \text{Median Employee Pay}.$$

Thus, our prediction that the employee's pay satisfaction will increase in response to the CEO pay ratio disclosure is driven by the notion that $R_2 < R_1$, in which case their pay will appear more favorable relative to their reference wage in the post-disclosure period than it was in the pre-disclosure period. To assess how likely this inequality is to hold, we calculate estimates of R_2 and R_1 using the 25th, 50th, and 75th percentile CEO and median employee pay values in our sample to capture different values of *CEO Pay* and *Other Pay*, along with values of w_1^C that vary from 0% to 5% (where $w_1^O = 1 - w_1^C$). For a given combination of *CEO Pay*, *Other Pay*, and w_1^C, we estimate different values of R_2 by varying *Median Employee Pay* (again based on the quartile values in our sample) and by varying w_2^M between 0%, 50%, and 100%, such that the ratio w_1^O/w_1^C equals w_2^O/w_2^C and $w_2^C + w_2^O + w_2^M = 1$.

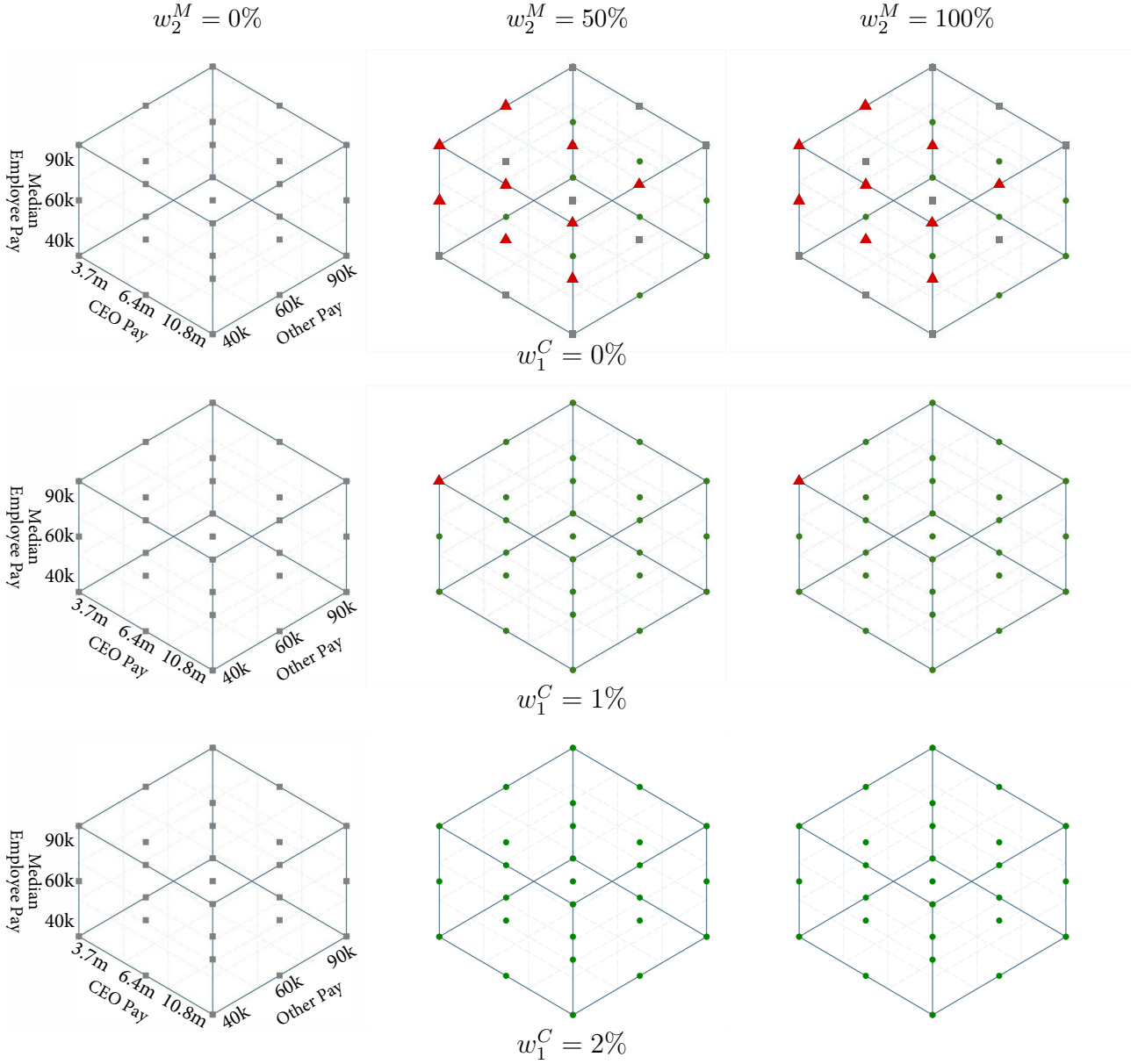
We show in Figure B.1 that the inequality $R_2 < R_1$ is often satisfied. Green circles in the graphs indicate instances in which the employee's reference wage decreases, i.e., when $R_2 < R_1$, whereas red triangles indicate instances in which the employee's reference wage increases, i.e., when $R_2 > R_1$. Gray squares indicate instances in which the employee's reference wage stays the same, i.e., when $R_2 = R_1$. The graphs in the leftmost column display gray squares for every combination of *CEO Pay*, *Other Pay*, and *Median Employee Pay* because in these scenarios the employee places zero weight on the median employee's pay when calculating R_2. As such, their reference wage remains the same in the pre- and post-disclosure periods. The graphs in the top row all show an even number of instances in which reference wages decrease, increase, or stay the same. In these scenarios, the employee places zero weight on the CEO's pay when calculating R_1 and R_2. Therefore, changes in the employee's reference wage depend solely on whether or not the median employee's pay is greater than, less than, or equal to the other pay information level.

We posit that employees will likely place a non-zero weight on both the CEO's and the median

[35] This model can be easily adapted to decompose other pay information that an employee might factor into their reference wage calculation simply by incorporating additional compensation values and corresponding weights into the model.

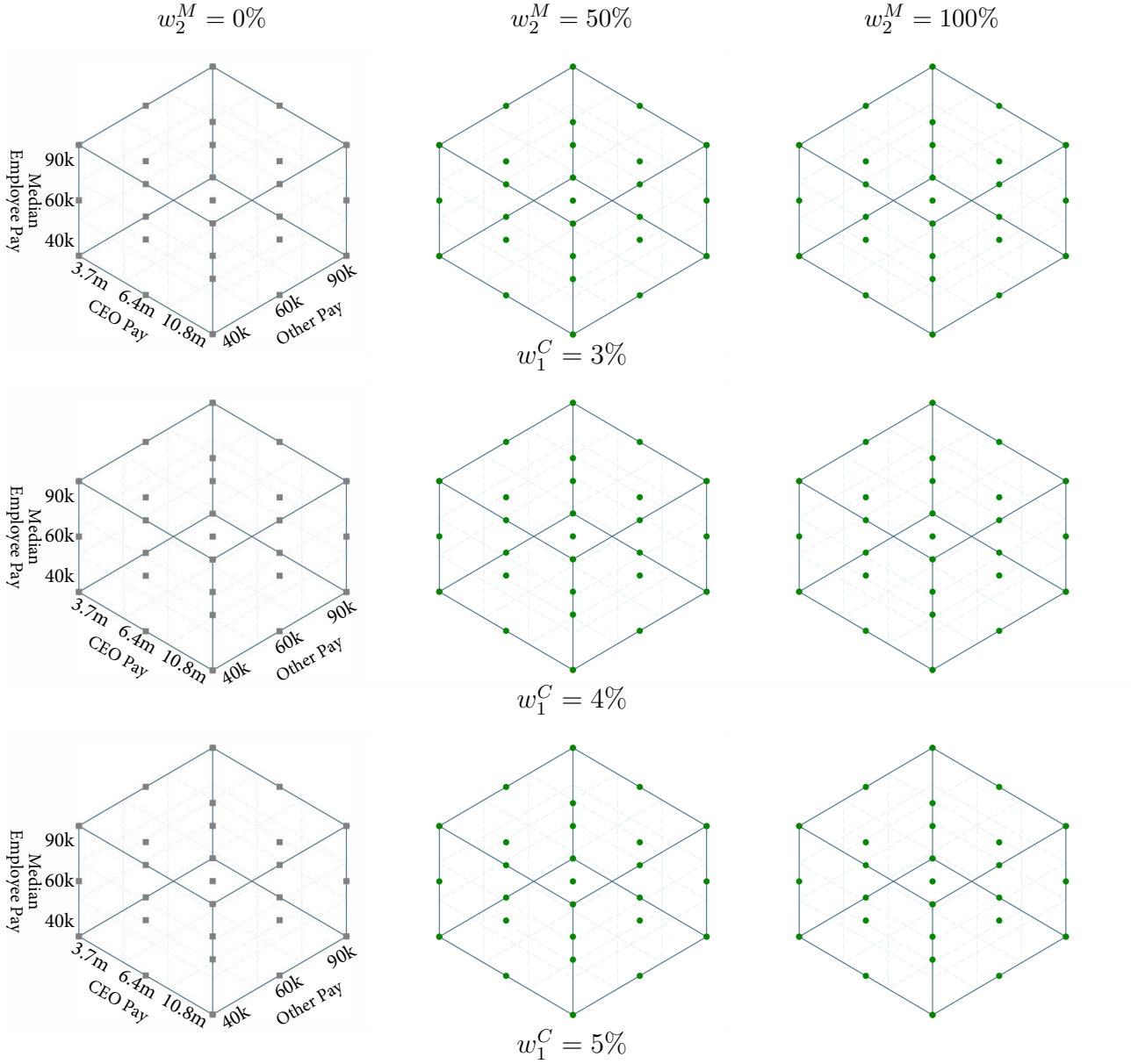
employee's pay when making their reference wage calculations. Thus, the most informative graphs in Figure B.1 are those in which some weight is placed on both of these sources of pay information, captured by the graphs in the rightmost columns and the five bottom rows. These show that in nearly every scenario, reference wages decrease when median employee pay information is included into the reference wage calculations. The only exception to this comes from the scenarios in which the weight on *CEO Pay* is only 1%, *CEO Pay* and *Other Pay* are both in the bottom quartile of the wage distribution, and *Median Employee Pay* is in the top quartile of the wage distribution. The key insight from the analytical example in Figure B.1 is that because CEO pay is significantly higher than the pay of rank-and-file employees, placing at least some weight on CEO pay in an employee's reference wage calculation will generally cause their pre-disclosure period reference wage to be larger than the median employee's pay. Because of this, any weight given to the median employee's pay when calculating the post-disclosure period reference wage will cause it to be lower than the pre-disclosure period reference wage. And lower references wages will lead to increased pay satisfaction.

Figure B.1: Analytical Example



Notes: These graphs illustrate how an employee's reference wage would change given different levels of *CEO Pay*, *Other Pay*, and *Median Employee Pay* and given different weights placed on each component of pay in the reference wage calculation. Green circles in the graphs indicate instances in which the employee's reference wage decreases, i.e., when $R_2 < R_1$, whereas red triangles indicate instances in which the employee's reference wage increases, i.e., when $R_2 > R_1$. Gray squares indicate instances in which the employee's reference wage stays the same, i.e., when $R_2 = R_1$. We calculate estimates of R_2 and R_1 using the 25th, 50th, and 75th percentile CEO and median employee pay values in our sample to capture different values of *CEO Pay* and *Other Pay*, along with values of w_1^C that vary from 0% to 5% (where $w_1^O = 1 - w_1^C$). For a given combination of *CEO Pay*, *Other Pay*, and w_1^C, we estimate different values of R_2 by varying *Median Employee Pay* (again based on the quartile values in our sample) and by varying w_2^M between 0%, 50%, and 100%, such that the ratio w_1^O/w_1^C equals w_2^O/w_2^C and $w_2^C + w_2^O + w_2^M = 1$.

Figure B.1: Analytical Example (continued)



Notes: These graphs illustrate how an employee's reference wage would change given different levels of *CEO Pay*, *Other Pay*, and *Median Employee Pay* and given different weights placed on each component of pay in the reference wage calculation. Green circles in the graphs indicate instances in which the employee's reference wage decreases, i.e., when $R_2 < R_1$, whereas red triangles indicate instances in which the employee's reference wage increases, i.e., when $R_2 > R_1$. Gray squares indicate instances in which the employee's reference wage stays the same, i.e., when $R_2 = R_1$. We calculate estimates of R_2 and R_1 using the 25th, 50th, and 75th percentile CEO and median employee pay values in our sample to capture different values of *CEO Pay* and *Other Pay*, along with values of w_1^C that vary from 0% to 5% (where $w_1^O = 1 - w_1^C$). For a given combination of *CEO Pay*, *Other Pay*, and w_1^C, we estimate different values of R_2 by varying *Median Employee Pay* (again based on the quartile values in our sample) and by varying w_2^M between 0%, 50%, and 100%, such that the ratio w_1^O/w_1^C equals w_2^O/w_2^C and $w_2^C + w_2^O + w_2^M = 1$.