Labor Market Consequences of Pay Transparency:
Evidence from the Initial Pay Ratio Disclosure

Michael Dambra
University at Buffalo, SUNY
mjdambra@buffalo.edu

Joshua A. Khavis
University at Buffalo, SUNY
jakhavis@buffalo.edu

Zhiru Lin
DePauw University
zhirulin@depauw.edu

Inho Suk
University at Buffalo, SUNY
inhosuk@buffalo.edu

Draft: February 2025

Abstract: This paper examines the labor market consequences of employee-pay transparency for rank-and-file employees. We exploit the introduction of the Pay Ratio Disclosure (PRD), where median employee pay becomes publicly disclosed for the first time, which allows for comparisons of employee mobility for firms with and without the PRD using difference-in-differences analyses. We find that employees are more likely to depart firms disclosing median employee pay and provide evidence that pay comparisons (both within and across firms) through the disclosure of *median employee pay* serve as a mechanism. Newly reporting PRD firms also experience higher employee churn. We also find evidence consistent with disclosures of median employee pay increasing labor market mobility in the aggregate across geographic locations and industries. Collectively, we provide novel evidence that increased pay transparency facilitates pay comparison within and across firms, which results in greater labor market mobility for employees.

Keywords: median employee pay, CEO-to-median employee pay ratio, pay transparency, pay comparison, employee turnover, labor market, employee mobility

1. Introduction

In 2015, under the Dodd-Frank Act, the U.S. Securities and Exchange Commission (SEC) adopted a rule requiring public firms to report the ratio of the Chief Executive Officer's (CEO's) compensation relative to their median employee pay (the Pay Ratio Disclosure, "PRD" hereafter) for reporting periods beginning January 1, 2017 (SEC 2015). The officially stated goal of the regulation was to provide *investors* with "a company-specific metric that can assist in their evaluation of a registrant's executive compensation practices" (SEC 2015).[1] While the stated goal of mandating the additional pay information (i.e., median employee pay) was to provide investors a metric to assess CEO pay (Aguilar 2015), it also provides new information to another important class of firm stakeholders—employees, and thus may have tangible labor-market consequences. Because the newly disclosed median pay information may be informative to existing and prospective firm employees, we examine the externalities of this regulation on employee labor-market mobility.

Increased pay transparency plays an important role in human capital attraction and retention (Chen, Fung, and LaViers 2023). It enables employees to compare their level of pay to a reliable benchmark and evaluate how firms allocate employee pay (Gerhart and Rynes 2003; Alterman et al. 2021). Prior to the PRD reform, the availability of pay information for private-sector public firms had been limited to executive pay, and only for the top five officers. Moreover, firms often adopt pay secrecy policies to ensure pay information on rank-and-file employees remains private. Around 60% of private-sector employees attest that their firm's pay information is secret

---

[1] While the regulation was not intended to publicly "shame" firms over their CEO compensation practices, it was nonetheless publicized as aimed at better informing investors about "runaway executive pay" and "how a company treats its average workers" (Menendez 2015; Piwowar 2015; SEC 2015). Around 87% of Americans consider the growing pay gap between CEOs and rank-and-file workers problematic (Tonti 2022). Whereas CEOs of top U.S. firms were compensated twenty times as much as the typical worker in 1965, this multiple has surged to nearly four hundred in 2020 (Bivens and Kandra 2022). This ever-widening pay disparity has sparked public debate and outrage (The Guardian 2021; 2022; Kesseler 2023).

(Hayes and Hartmann 2011). The lack of pay transparency makes it difficult for employees to ascertain how their compensation compares to those of their peers. The new SEC rule provides employees with a salient benchmark for pay comparison, which can alter employees' perceptions of their current compensation and affect their labor choices. We document the labor-market consequences of the PRD by shedding light on how firms' initial median-employee pay disclosure impacts employee labor mobility.

While the pay ratio disclosure rule has had little impact on total CEO pay (Chang, Dambra, Schonberger, and Suk 2023), its effect on employees' decisions to join or leave the firm is theoretically unclear and remains an open empirical question. Whereas employees tend to engage in social comparisons, which influence their perceptions of current positions and future opportunities (Festinger 1954; Adams 1963), it is unclear if and how PRD should impact employee labor choices. Extant relevant theories offer competing predictions. While the relative deprivation theory suggests that the PRD may lead to greater attritions by fostering feelings of deprivation among employees as they compare their pay to others, tournament theory suggests that the PRD may encourage employees to remain longer with their employer in hopes of reaping greater rewards upon being promoted.

Given that the average person believes that they are above average and engages in upward social comparisons, employees are more likely to compare themselves to those who earn higher salaries rather than to those who earn less (e.g., Peterson 2000; Dunning et al. 2004; Moore 2007). Under the relative deprivation theory, the PRD may facilitate employees' upward social comparisons, increasing their perceptions of pay inequality and resentment for employees at the lower ends of the pay distribution. This could encourage employees to leave their current

employer for "better" firms (e.g., Adam 1963; Greenberg et al. 2007; Smith et al. 2012).[2]

Alternatively, PRD may improve employee retention under tournament theory. Specifically, the PRD-revealed pay disparity could motivate lower-paid employees, leading them to remain with their employer longer in anticipation of future remuneration upon promotion (e.g., Gerhart and Rynes 2003; Faleye et al. 2013; Kacperczyk and Balachandran 2018; Chi et al. 2019). Recent literature suggests that disclosing median employee pay induces greater pay satisfaction as it serves to adjust employees' reference wages downward (Carter et al. 2025). Despite the theoretical links between PRD and employee labor choices, it is also possible that PRD has little to no effect due to its limited informativeness or extant labor-market frictions. Thus, the impact of PRD on employees' actual labor choices remains an open question.

To address this question, we examine whether and how the PRD affects labor-market mobility by exploiting variation in the implementation of the disclosure rule across firms with different fiscal year-ends. Because the SEC mandates PRD for reporting periods beginning Jan 1, 2017, firms with December (June-November) fiscal year-ends initially disclose their median employee pay in their first annual proxy statement filed following the close of the 2017 (2018) fiscal year. That is, the initial information on median employee pay is provided by the first annual proxy filed after the close of the 2017 fiscal year for firms with December but not June-November fiscal year-ends. We define treated and control firms by using December 31 as the cutoff date for the 2017 fiscal year-end and deploy a difference-in-differences (DiD) design to identify the effects of the initial PRD. Specifically, we compare firms with December fiscal year-ends ("treated") to those with June through November fiscal year-ends ("control") by examining

---

[2] Social comparison refers to employees assessing their pay by using others as a benchmark (e.g., Bloom and Michel 2002; Kacperczyk and Balachandran 2017). Central to this social comparison process is an assessment of the fairness or equity of what is observed, an inherently subjective judgment.

their employees' labor mobility during the [-6, +6] month window surrounding their first annual proxy filed after the close of their 2017 fiscal year. We use monthly-level employment data and a narrow testing window to isolate the immediate effects of the PRD on employee labor mobility.

We find that, relative to employees of control firms, employees of treated firms are more likely to leave the firm after PRD. Specifically, the PRD leads to a twelve basis points increase in the monthly departure rate for employees at treated firms relative to control firms. Our results suggest that employees seek outside employment opportunities when median employee pay is made available for the first time, which is in line with relative deprivation theory.

While our main results are consistent with relative deprivation theory, we validate that this is indeed the case by testing the theory's basic assumptions. Because employees' ability to compare themselves to a reference group is an essential underlying requirement for validating this theory in our setting, we conduct further tests to demonstrate that employee-pay comparisons indeed explain the increased employee mobility we observe.[3] Given that the PRD can facilitate pay comparisons within- and across-firms, we test for evidence of both comparison types.

The PRD enables within-firm comparisons whereby employees can assess their own salaries relative to other employees within the same firm (e.g., the median employee).[4] Individuals generally experience negative judgments about distributive fairness when making upward comparisons (Pfeffer and Langton 1993; Moore 2007). If within-firm comparisons explain the increased departure rates among employees, then employees earning lower wages and holding lower-ranking positions should be more likely to leave the firm after its initial disclosure of the

---

[3] The annual proxy filed following the close of 2017 fiscal year contains the median employee pay as well as CEO pay for treated firms. As the proxy also contains CEO pay for control firms, the observed effect of the proxy on employee turnover is likely to be attributable to the median pay rather than CEO pay.

[4] Social comparison theory suggests rank-and-file employees are more likely to see peers with similar qualifications or holding similar position as referents (Festinger, 1954). The evidence that employee satisfaction is increasing in median employee pay and largely unrelated to CEO pay challenges the idea that rank-and-file employees compare themselves to CEOs when assessing pay fairness (Green et al. 2023).

median wage. We indeed find that the increase in departures pertains more to employees earning lower pay and occupying lower-ranking positions at the firm. This supports the notion that within-firm comparisons lead to increased employee labor mobility.

The PRD may also facilitate cross-firm comparisons where employees evaluate the compensation packages offered by their current employer relative to those of other employers. The cross-firm comparisons may direct employees' job search efforts to firms offering more generous employee compensations. We indeed find that, following the PRD, employees are more likely to move to a new employer whose median employee pay is above that of their current employer. Taken together, our findings suggest that the initial median employee wage disclosure enables employee pay comparisons within *and* across firms, and that these comparisons serve as mechanisms for the increase in employee labor mobility observed after PRD.

We next conduct several of cross-sectional tests using the information surprise contained within the initial PRD as well as various characteristics of the workplace environment as moderators. We expect employee attrition will be exacerbated when firms disclose unexpectedly high median pay, leading to greater disappointment among employees experiencing greater than expected disparity between their compensation and that of the median employee. Consistent with expectations, we indeed find that the PRD's effect on employee departures intensifies when median employee pay is unexpectedly high. We also find no relation between employee departures and either the pay ratio or CEO pay, suggesting that our main results are driven by the portion of the PRD that provides "new" information. In the cross-section, we also find stronger effects in instances where (1) employees experience lower pre-existing pay transparency (i.e., firms in industries with lower labor union coverage); (2) pay comparisons are more warranted (i.e., firms with greater job similarity); (3) employees have higher turnover intentions (i.e., firms

5

with higher proportion of employees seeking outside jobs prior to the disclosure); and (4) firms experience more negative media coverage surrounding their proxy statement disclosures.

After documenting that the PRD leads to greater employee departures, our extended tests use alternative measures of job mobility. First, in tighter windows, we observe that job search increases for PRD reporting firms following the release of their proxy statement relative to non-reporting firms using Glassdoor reviews as a surrogate variable (deHaan, Li, and Zhou 2023). Second, we find evidence indicating that the PRD stimulates more hiring and employee churn overall, but not a shift in total employment. We collectively interpret our results as the PRD having a dynamic effect in reallocating labor across firms. Third, we use quarterly Census Bureau data to examine PRD's impact on aggregate labor mobility at the state and industry level. We observe that employee mobility at the state or industry level indeed increases with the proportion of Russell 3000 firms disclosing median employee pay. This is consistent with our firm-level findings suggesting PRD increases labor-market mobility. However, we are careful to acknowledge that this is descriptive evidence and only suggestive of aggregate effects of increased wage transparency.

We substantiate our main findings through a battery of additional tests. First, we observe no differential change in employee attritions between treated and control firms around the first annual proxy filed after the end of the 2016 fiscal year, indicating that the change in employee departures is attributable specifically to the initial PRD rather than to other information contained in the proxy. Second, we are able to replicate our main results using emerging growth companies (EGCs) and smaller reporting companies (SRCs) as an alternative control group. Third, we re-run our DiD specification using a stacked DiD regression with cohort-based fixed effects corresponding to the proxy statement issuance month and find similar results. Finally, we

find no evidence of pre-existing differential time trends between our treatment and control firms.

Our research contributes to the literature in several ways. First, our study broadly speaks to the labor mobility consequences of increased pay transparency. Prior literature provides conflicting evidence as to whether pay transparency influences labor mobility.[5] The largely mixed evidence from studies focusing on different types of companies and employees suggests that the effect of PRD on private sector labor mobility cannot be inferred from previous studies. Our findings are important given that private sector pay transparency remains minimal, and we demonstrate that it informs employees' labor market decisions and enhances labor-market liquidity.

Second, extant PRD research has largely focused on how *firms* mitigate PRD scrutiny. For instance, Chang et al. (2023) find that more scrutinized firms decrease the mix of CEO pay compensation that is more susceptible to external criticism, such as stock-based compensation. Boone et al. (2024) find that firms tend to provide more narrative disclosure surrounding high CEO pay ratios, while Yeung (2021) finds that firms decrease employment overall but hire more generously paid individuals *prior* to their initial disclosure of the mandated pay ratios. Our study differs from this stream of literature by focusing on *employee-level* responses to the PRD.

Third, we are able to extend the disparate evidence documented in contemporaneous work. Carter et al. (2025) find no change in total employment in response to the PRD. In contrast, we find that the PRD leads to increased employee departures *and* entries. Thus, while net employment does not change, total employee displacement does. Liang et al. (2024) ascribe an increase in employee turnover following PRD to high-CEO pay *ratios*. Using more granular data and a pre- and post-DiD design, we find that the employee response is unrelated to CEO

---

[5] For example, see Card et al. (2012); Burn and Kettler (2019); Mas (2017); and Cullen (2023).

compensation and driven specifically by the disclosure of *median employee pay*. Importantly, our study also contrasts with the extant literature by documenting where employees land a new job, providing novel evidence that employees incorporate the PRD information into their job transition decisions. Finally, we examine new employee hiring and employee churn post-PRD and complement our firm-level tests by documenting aggregate effects of the PRD on labor market mobility across geographies and industries. Thus, our collective evidence contributes to the nascent literature on how firm disclosure aimed at investors inadvertently shape employees' decisions to join or leave a firm (e.g., DeHaan, Li, and Zhou 2023; Choi, Choi, and Malik 2023).

2. Pay Transparency, Social Comparisons, and Employee Departures

Regulators enacted the PRD to provide shareholders with relevant benchmarking information on a firm's CEO compensation as they consider voting and investment decisions (SEC 2015). Indeed, recent research suggests that shareholders respond to higher CEO pay ratios in terms of trading and Say-on-Pay voting outcomes (i.e., Pan et al. 2022, Chang et al. 2023). Besides its intended goal of providing investors with a benchmark to evaluate CEO pay, the PRD, as an unintended consequence, increased pay transparency for private sector employees.

Specifically, firm-disclosed median employee pay may enable employees to better assess their salaries relative to within-firm peers. Median employee pay could also signal how well a firm remunerates its workforce relative to peer firms. Firms seemingly offering higher pay will find it easier to recruit better talent, while firms paying less than their peers will face retention issues (Shaw et al. 1998).[6] The PRD may also draw attention to a firm's vertical pay inequality,

---

[6] It is noteworthy that while median pay can offer insights into a firm's compensation practices, it is but one among many metrics that should be considered when evaluating how a firm treats employees. Factors including but not limited to benefits, work-life balance, career advancement prospects, and the broader corporate culture all shape perceptions of how a firm treats its workforce.

which is often underestimated by employees (Cullen and Perez-Truglia 2022).[7] Thus, the PRD could be informative, as private-sector firms frequently refrain from making employee pay publicly available and place various restrictions on employees to prevent them from sharing their pay information with others (O'Neil 2010; Trotter et al. 2017, Burn and Kettler 2019).[8]

We use a social-comparison framework to develop expectations on how the advent of PRD affects employees' decisions to seek outside employment. Pay transparency fosters employee compensation comparisons (Lawler 1965). The social-comparison framework revolves around employees evaluating their compensation vis-à-vis relevant reference groups. Depending on the reference group used, employees may compare their pay horizontally or vertically within an organization, or horizontally across different firms (e.g., Zenger 1992; Wade et al. 2006; Shue 2013; Gartenberg and Wulf 2017).

However, it is *ex-ante* unclear how the compensation comparisons will influence employees' labor choices at newly disclosing firms. On the one hand, relative deprivation theory posits that the PRD may lead to departures if employees begin comparing their pay to that of higher-paid employees within and outside the firm (Festinger 1954; Greenberg et al. 2007). Within-firm pay comparisons harm employee retention by evoking negative perceptions of pay disparity, resulting in reduced job satisfaction, reduced effort, and overall decreased performance levels (e.g., Breza et al. 2018; Card et al. 2012; Boone et al. 2024). Notably, prior research finds an asymmetric response to peer pay disparity. Employees paid below a revealed reference wage tend to respond more negatively, whereas employees paid above a revealed reference wage are often unaffected by pay disparity (e.g., Card et al. 2012; Gächter and Thöni 2010; Cohn et al.

---

[7] While Americans consider (believe) that 7 (30) is the ideal (actual) CEO-to-median employee pay ratio (Kiatpongsan and Norton 2014), the actual ratio is closer to 400 (Statista Research Department 2023).
[8] Firms have opposed pay transparency for a long time, asserting potential adverse effects on employee satisfaction and productivity to justify their position (O'Neill 2010).

2014). Moreover, cross-firm pay comparisons may encourage workers to seek outside employment at firms offering more generous compensation (e.g., Cullen 2023). This social-comparison framework suggests that the PRD may lead to the departure of employees who after comparing themselves to others become aware of pay disparities within their current firm and (seemingly) more generous pay practices at other firms. It is noteworthy that the *saliency* and mere presence of the newly disclosed median pay metrics may induce employees to exploit them for cross-firm comparisons, even if these metrics are ill-suited for such comparisons (Sweeney and McFarlin 2005, Boone et al. 2024).

On the other hand, the PRD may *improve* employee retention (i.e., reduce employee departures). Carter et al. (2025) show that employees become more satisfied with their pay following the PRD, and that this improvement is more pronounced for firms disclosing a lower median employee pay. Carter et al. (2025) argue that the newly disclosed median employee pay lowers employees' reference wages, which employees use to evaluate their relative pay. Furthermore, the PRD can induce upward vertical comparisons, where lower-level employees compare their pay to either a higher-compensated median employee or the CEO. Specifically, tournament theory suggests that the PRD may enhance employee morale by signaling promising future compensation prospects upon promotion (e.g., Gerhart and Rynes 2003; Faleye et al. 2013; Kacperczyk and Balachandran 2018; Chi et al. 2019). These factors may improve employee retention.

There are also compelling reasons why the PRD may have little or no effect on the employee decision to leave the firm. First, the disclosure may be uninformative as the median employee pay can be acquired from alternative sources before the disclosure regime and CEO-employee pay disparities are common knowledge. Second, the PRD metrics may be ill-suited for

making pay comparisons across firms.[9] Third, even if the disclosure changes employee

perceptions about their current employer (e.g., Carter et al. 2025), the change in perceptions may

not translate into concrete employee actions of leaving the firm due to various labor market

frictions (Mobley et al. 1978).

Taken together, it is unclear whether and how the initial PRD impacts employee retention.

While the relative deprivation theory predicts a negative effect, the downward adjustment of

reference wages argument and tournament theory both suggest a positive effect. Furthermore, the

limited informativeness of the pay metrics disclosure may render the PRD ineffective. We thus

state our main hypothesis in the null form:

H1: Employee departures are not associated with the initial Pay Ratio Disclosure (PRD).

3. Research Design

3.1. Sample Construction

We test how the initial Pay Ratio Disclosure (PRD) affects employee labor mobility using a

difference-in-differences (DiD) approach. Following Chang et al. (2023), we define firms with

December 31 fiscal year-ends as treated firms, and firms with fiscal year-ends between June 30

through December 30 as control firms.[10] Treated firms are required to initiate the PRD after the

close of their 2017 fiscal year, while control firms are not subject to the requirement until after

the end of their 2018 fiscal year. As the first annual proxy filed after the end of the 2017 fiscal

year contains the initial PRD for treated but not for control firms, we compare employee

departures around this proxy's filing month for treated and control firms to test how the initial

---

[9] Regulators indeed recognize that firms have wide discretion in identifying the median employee and warn about the limited usefulness of comparing disclosed pay information across firms (Aguilar 2015; SEC 2017).
[10] To mitigate concerns that calendar-period differences between firms with December (treated) and June-November (control) fiscal year-ends drive the results, we restrict the control sample to firms with September-November year ends only and find our main results continue to hold (untabulated).

PRD affects employee labor decisions.[11]

The inclusion of treated and control firms allows us to isolate the effects of PRD on employee labor decisions by examining how employee mobility at firms that file proxies containing information on the median employee pay compares to employee mobility of firms filing proxies that do not contain this information. We exclude any firm that discloses median pay during our sample period in the control group to avoid biased estimates.[12]

Table 1, Panel A, presents the selection criteria and steps used to construct our sample. We begin sample construction with Russell 3000 Index firms at the intersection of Compustat and CRSP databases for the 2017 fiscal year. We exclude firms exempted from the median-employee pay disclosure requirement (e.g., emerging growth companies (EGCs), smaller reporting companies (SRCs), and foreign private issuers (FPIs), firms that do not disclose their median pay despite being mandated to do so, and firms in the financial industry.[13] We require that firms have a fiscal year-end between June and December and construct a firm-month dataset using the [-6, +6] monthly testing window surrounding the month of the annual proxy filing.[14] We then obtain employment data from *Revelio Labs*, a leading employment analytics company that converts unstructured data from *LinkedIn* profiles into structured proprietary datasets. The narrow [-6, +6] monthly testing window along with the granular monthly-level employment data captures the immediate employee responses to the initial PRD. Figure 1 illustrates the testing window used for PayPal (Visa), a treated (control) firm with a December (September) fiscal year-end.

---

[11] Firms whose initial PRD is included earlier in other filing (i.e., 10-K) than in proxy filing (DEF-14A) are excluded from the sample.

[12] Unlike recent studies that use treated firms prior to treatment as the control group (Liang et al. 2024), we ensure that our control group comprises only firms that do not initiate PRD during the sample period (i.e., control firms are never treated during the sample period). We thus mitigate potential estimation biases inherent in staggered DiD designs that use early-treatment observations as controls for late-treated firms.

[13] We exclude the financial industry because median pay can be inferred from labor expense numbers that financial firms were required to disclose prior to the PRD mandate. Our main results are robust to removing this requirement.

[14] Main results are robust to using [-9, +9] or [-6, +3] monthly testing windows (untabulated).

We use quarterly financials from Compustat and monthly returns from CRSP to construct control variables. After requiring that firms have observations for both the pre- and post-periods and are not missing control variables, our final sample includes 11,615 firm-month observations—comprising 9,488 treated and 2,127 control observations—that span the period of March 2017 to May 2019.[15] The final sample is based on 895 unique firms, 731 (164) of which are treated (control) firms.

Panel B of Table 1 presents the distribution of year-months in which the first annual proxy following the close of the 2017 fiscal year is filed. This proxy is filed in September 2017 at the earliest and by September 2018 at the latest. Table 2 presents the mean monthly rate, in percentage terms, at which employees leave the firm (*EmpDepartures*) by year-month (Panel A) and by industry (Panel B).

3.2. Empirical Models

We test how the initial Pay Ratio Disclosure (PRD) affects employees' decision to leave the firm by estimating the following Difference-in-Differences (DiD) regression model:[16]

$$EmpDepartures_{i,t} = \beta_1 Treat_{Dec_i} \times Post_{Proxy_{i,t}} + \beta_2 Post_{Proxy_{i,t}} + \qquad (1)$$
$$\sum \beta_n X_{it} + \psi_t + \phi_i + \varepsilon_{it}$$

The dependent variable *EmpDepartures* is the percentage of employees departing firm i during month t. *Treat_Dec* is an indicator set to one (zero) for firms with December (June-November) fiscal year ends. *Post_Proxy* is an indicator set to one (zero) if month t is within the six months of (before) firm i filing its first annual proxy after the end of its 2017 fiscal year.[17]

---

[15] The overwhelming majority of firms in our sample (nearly 95%) file their proxy statement on time. Main results are robust to excluding late filers.

[16] The parallel trends assumption for DiD analyses is satisfied in our setting (see Section 6.3 for details).

[17] Because the proxy filing event reveals the median employee pay only for treated but not for control firms, it serves as a pseudo-event for control firms. Given that proxy statements contain a plethora of information unrelated to the median pay, using this pseudo-event of non-disclosing firms acts as an important control and allows for meaningful comparisons and drawing of inferences.

With the inclusion of year-month effects, *Post_Proxy* captures the seasonality in employee departures within a given fiscal year. The β_1 coefficient on the interaction term *Treat_Dec×Post_Proxy* is the coefficient of interest as it measures the incremental rate of employees leaving firms that initially disclose their median pay information relative to firms that do not disclose their median pay for the same fiscal year. A positive β_1 indicates increased labor departures from the reporting firm, consistent with relative deprivation theory whereby employees at newly disclosing firms should negatively respond to median employee pay reference wages.[18] In contrast, a negative β_1 would be consistent with the median pay disclosure either lowering their reference wages (Carter et al. 2025) or increasing employee satisfaction by revealing the potential for higher future wages (Kacperczyk and Balachandran 2018). **X** denotes a vector of time-varying economic determinants that may shape employee mobility at the firm, including firm size (*Size*), market-to-book ratio (*MTB*), financial leverage (*Leverage*), profitability (*ROA*), and stock returns (*RET*).[19] All continuous variables are winsorized at the top and bottom percentiles. We include firm fixed effects (ϕ_i) to mitigate concerns of time-invariant firm-specific factors driving the results and year-month fixed effects (ψ_t) to mitigate concerns that employment seasonality or time trends confound our results.[20] Standard errors are clustered by firm in all specifications, and reported *t*-statistics are based on two-tailed tests. Definitions and data sources for all variables are provided in Appendix A.

4. Empirical Results

4.1 The Pay Ratio Disclosure and Employee Mobility

---

[18] Cohn et al. (2014) argue that social comparisons induce asymmetric reactions from peers, where lower paid peers significantly respond to wage gaps while higher paid peers appear unresponsive.

[19] All control variables are measured at the close of the quarter immediately preceding year-month *t*.

[20] Given that firms differ in their proxy filing dates, the stand-alone *Post_Proxy*, which pertains to a particular year-month, varies across firms, and is therefore not subsumed by the model's year-month fixed effects.

Table 2, Panel C provides descriptive statistics for all variables used in the main analysis. The mean monthly employee-departure rate (*EmpDepartures*) is 1.15%.[21] In terms of firm characteristics, the average firm reports $2.4 billion (natural logarithm is 7.8) in total assets (*Size*), has a 4.2 market-to-book ratio (*MTB*), is 26.4% leveraged (*Leverage*), experiences a 0.3% return on assets (*ROA*), and has 23.7% annual buy-and-hold stock returns (*RET*).

Table 3 reports the main results. The coefficient on *Treat_Dec×Post_Proxy* reported in column (1) is positive and significant at the 1% level, indicating that employee departures for the treated group (i.e., firms with December fiscal year-ends) increase as compared to the control group (i.e., firms with June-November fiscal year-ends) following the release of firms' proxy filings. In plain words, the results suggest that PRD increases relative employee departures. Compared to control firms, treated firms experience a 0.11% higher increase in their monthly employee-departure rate (*EmpDepartures*) following the median-pay disclosure, which is 9.6% of the sample mean, an economically significant effect on employee mobility. *Post_Proxy* is negative and statistically significant, suggesting that employee turnover is lower during the first six than the last six months of the fiscal year.

Column (2) reports results estimated using an entropy-balanced sample that achieves covariate balance for the first two moments (mean and variance) of the control- and treated-group observations. Specifically, all independent variables in Eq. (1) are reweighted such that the differences in standardized means and variances across the control and treated groups approach 0 (untabulated).[22] The results obtained using the entropy-balanced sample in column (2)

---

[21] The average employee-departure rate for firms in our sample is similar to that of Li et al. (2022) who document an average quarterly rate of 3.33%.

[22] Control variables are measured as of the end of the quarter immediately preceding the year-month *t*. However, results are robust to entropy balancing the control variables based on their pre-treatment levels (at the end of fiscal year 2016).

corroborate our main findings. These results provide initial evidence that the disclosure of median employee pay information induces more employee departures. This suggests that the negative effects of PRD-induced social comparisons eclipse any potential benefits of higher job satisfaction achieved through employees lowering their reference wages or learning of greater tournament-based incentives (Carter et al. 2025, Kacperczyk and Balachandran 2018).[23]

4.2 Mechanism behind Increased Employee Mobility

Having demonstrated that employee mobility increases after the initial PRD, we next explore the mechanism responsible. Our theoretical framework posits that asymmetric employee-pay comparisons—particularly upward comparisons—may explain the greater rate of employee departures after the initial PRD. Specifically, employees comparing their salaries with those of other employees within the firm, or comparing their current employer's pay policies with those of other employers, may lead to disenchantment and greater departure rates. We thus empirically test for evidence of whether within- and cross-firm pay comparisons serve as the mechanism behind the greater employee departure rates observed post-PRD.

4.2.1 Within-firm Pay Comparisons

Employees' current positions and pay may play a critical role in how they perceive their employer and react to the initial disclosures of median employee pay. To the extent that revelations of pay disparity drive employees to leave their employer, lower earners should be impacted most while higher earners should be less responsive to such revelations. Similarly, employees in lower-rank positions are more likely to be bothered by revelations of pay

---

[23] Carter et al. (2025) find no evidence that Glassdoor reviews increase after the implementation of the CEO Pay Ratio disclosure in a *Post*-only staggered DiD and no descriptive evidence that average employment levels vary between pre- and post-PRD years using *Compustat*'s 'EMP' variable. Alternatively, we focus on actual employee departures using *Revelio* data, a staggered DiD with treatment and control firms, a stacked DiD, and a traditional DiD using EGCs and SRCs as control firms.

disparities within the firm than their higher-ranked peers. Given that individuals making upward comparisons have tendencies to form negative perceptions of distributive fairness (Pfeffer and Langton 1993; Moore 2007), we expect employees earning lower wages or occupying lower-rank positions to be most likely to depart the firm.

We exploit the within-firm variation in employee salaries and job-role seniority and construct two additional datasets to test the notion that within-firm pay comparisons are driving employee departures. We first conduct analysis at the firm-month-seniority level. We use the estimated seniority from *Revelio* to distinguish between employees in low- versus high-ranking positions within the firm.[24] Accordingly, we create a discrete variable *LowerRank* ranging from 1 to 4, where 1 (4) indicates the most senior (junior) job position.

We then conduct an analysis at the firm-month-salary level. The *Revelio* dataset also includes the estimated salary for each position based on U.S. Census data. We use the estimated salary to determine each job position's salary relative to the firm's initially disclosed median employee pay. Accordingly, we create an ordinal variable *BelowMedianPay* which is equal to 1 for the group of positions with an estimated pay below the median employee pay a firm initially disclosed, and zero otherwise.[25]

Using the two additional datasets described above, we adopt a triple difference-in-differences design with a moderator as follows:

$$
\begin{aligned}
EmpDepartures_{i,j,t} \\
= \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} \times PositionFactor_{i,j,t}
\end{aligned}
\tag{2}
$$

---

[24] Seniority of 1 (4) denotes lowest (highest) rank. As per *Revelio*, seniority for each job position is determined based on title, industry, company size, and other factors related to the firm's organizational structure.

[25] As per *Revelio*, salary for each position is based on role, seniority, company, and country; the prediction model has been trained using over 50 million salaries and obtains an out-of-sample root mean squared error (RMSE) of 8%. All positions are assigned into one of two groups for each sample firm based on initial disclosure of the median employee's pay. The initial disclosure for treatment (control) firms is included in the proxy statement after the end of fiscal year 2017 (2018).

$$+ \beta_2 Post\_Proxy_{i,t} \times PositionFactor_{i,j,t} + \beta_3 PositionFactor_{i,j,t} +$$
$$\beta_4 Treat\_Dec_i \times Post\_Proxy_{i,t} + \beta_6 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + \psi_t + \phi_i + \varepsilon_{i,t},$$

where *PositionFactor* assumes *LowerRank* (*BelowMedianPay*) for the firm-month-seniority

(firm-month-salary) level analysis, and all other variables are as previously defined.[26]

Table 4 presents the estimation results. In column (1), the coefficient on

Treat_Dec×Post_Proxy×LowerRank is significantly positive, indicating that the direct link

between the initial PRD and employee departures becomes increasingly pronounced among

employees holding lower-ranking positions within the firm. These results are consistent with

within-firm pay comparisons explaining lower-rank employees' decision to leave their employer.

Similarly, in column (2), the coefficient on the triple interaction term *Treat_Dec×Post_Proxy×*

BelowMedianPay is significantly positive, indicating that the effect of the initial PRD on

employee departures is more pronounced for employees earning lower wages within the firm.

This is consistent with within-firm pay comparisons explaining lower-paid employees' decision

to leave their employer.

Our results in Table 4 provide some evidence that the median-pay disclosure facilitated

within-firm comparisons and raised awareness of pay disparities particularly among the

employees most likely to respond to disparities—lower-paid and lower-ranked employees (Cohn

et al. 2014). However, besides revealing pay disparities *within* the firm, the PRD may also shed

light on pay disparities *across* firms. As such, we next examine which firms do the departing

employees join following the PRD.

---

[26] We do not include the two-way interaction *Treat_Dec× PositionFactor* in Eq. (2) due to multicollinearity issues stemming from the inclusion of both three-way and two-way interaction indicators when samples have an unbalanced weighting of treatment observations. Indeed, including *Treat_Dec× PositionFactor* in Eq. (2) results in a variance inflation factor score above 30 on our triple interaction term (untabulated).

4.2.2 Cross-firm Pay Comparisons

Employees' job-transition decisions are driven by both assessments of their current employer's pay practices and the pay practices at other firms. Firms with a higher median pay may be perceived to have more generous or equitable pay practices. We therefore predict that, following the initial PRD, employees are more likely to depart their current employer for firms offering higher median pay.

We test for evidence of cross-firm comparisons by examining where employees departing from firms that disclose median pay information begin a new job relative to employees departing firms that have not yet disclosed their median pay. Specifically, we examine the likelihood that a departing employee joins a new employer with relatively higher median pay. First, we identify a sample of all individual employee departures from firms used in our main analysis occurring during the [-6, +6] month window surrounding the proxy filing. We then exclude employees working outside the U.S. and job transitions with employment gaps of more than 12 months as both of these are unlikely to be influenced by the median-pay disclosure.[27] To allow for meaningful comparisons between employee departures, we require that the original and destination firms be public and make their initial median-pay disclosure for either the 2017 or 2018 fiscal year. We test whether the disclosure affects the departing employee's decision to join a seemingly more generous new employer by estimating the following OLS regression model:

$$
\begin{aligned}
HigherEmpPay_{i,j,t} \\
= \beta_1 Treat\_Dec_{i,j,t} \times Post\_Proxy_{i,j,t} + \beta_2 Post\_Proxy_{i,j,t} \\
+ \beta_3 Salary_{i,j,t} + \beta_4 Seniority_{i,j,t} + \psi_t + \phi_i + \varepsilon_{i,j,t}
\end{aligned}
\tag{3}
$$

---

[27] We exclude employees outside the U.S. because the regulation permits firms to exclude certain non-U.S. employees from median-pay calculations. We also exclude job transitions where the gap in employment is over 12 months because these are more likely to be driven by personal life decisions (e.g., pursuing further education) rather than by the newly publicized information on median pay.

where individual employee *j* departs firm *i* in month *t*. In testing for evidence of cross-firm pay comparisons, we ascertain whether departing employees join a seemingly more generous (equitable) new employer, as captured by *HigherEmpPay*, an indicator that equals one if the disclosed median pay is higher at the destination firm than at the departing firm, and zero otherwise.[28] *Post_Proxy* is an indicator that equals one for employees departing during or after the filing month of the event proxy, and zero otherwise. We also control the estimated salary (*Salary*) and seniority (*Seniority*) of the employee's original position since these may influence the employee's decision to leave the firm. The model includes firm- and year-month fixed effects, and standard errors are clustered at the firm-level.[29] The coefficient on *Treat_Dec×Post_Proxy* captures the incremental increase in the likelihood of joining a new employer with a higher median employee pay for employees departing from treated firms relative to employees departing from control firms.

   In Table 5 Panel A, we presents descriptive statistics for the panel deployed in Eq. (3). Table 5 Panel B presents our empirical results. In column (1), the coefficient on *Treat_Dec ×Post_Proxy* is significantly positive when *HigherEmpPay* is the dependent variable. This indicates that, compared to employees who depart control firms, employees departing treated firms are more likely to join employers with higher median-employee pay. Specifically, compared to employees of control firms, employees of treated firms are one-percentage point more likely to join a new employer having median employee pay that is above that of their former employer. This represents an economically significant 18% increase in likelihood relative

---

[28] *HigherEmpPay* is constructed using the initially disclosed median-employee pay by treatment [control] firms for the fiscal year 2017 [2018]. We note that no control firm discloses during our [-6, +6] month testing window surrounding the annual proxy filing for the 2017 fiscal year.
[29] Results are robust to controlling for the destination firm fixed effects and clustering standard errors at the employee level.

to the sample mean. These results suggest that the PRD has an economically significant impact on employees' cross-firm mobility.

Having demonstrated that disclosure of median pay helps determine departing employees' destination firms, we examine whether the job transitions stem from the employee's decision to leave their employer or the employer's decision to terminate employment. Employers may attempt to lower their pay ratio by pre-emptively replacing lower-paid employees with higher-quality employees *before* the regulation is adopted (Yeung 2021). While we cannot fully isolate employee responses from firm responses in our setting, we attempt to provide evidence that employee responses are likely to explain at least part, if not all, of the employee departures *following* the initial PRD. We do so by exploiting the time it takes for a departing employee to begin working for the new employer. Intuitively, individuals voluntarily leaving their employer would have secured a new job before submitting their resignation and should thus have shorter gaps between consecutive employments than employees who are terminated. We use *LinkedIn* employees' job start and end dates, where available, to measure the number of months it takes a departing employee to begin a new job. We define departure as 'voluntary' if an employee's job start date at the new firm is within three months of the date they depart from the former employer, and 'involuntary' otherwise.

We report the results for the two mutually exclusive sub-samples capturing voluntary and involuntary employee departures in Panel B of Table 5. Columns (2) [(3)] presents results for [in]voluntary departures. Notably, the coefficient on *Treat_Dec×Post_Proxy* is significantly positive in column (2) but not in column (3). This suggests that the increase in employee mobility shortly after PRD documented in column (1) reflects employees' rather than employers' responses. Taken together, our analyses of individual employees' cross-firm movements suggest

that the disclosure of median pay triggers employees to migrate to new employers who appear more generous in remunerating their workforce.

4.3. Cross-sectional Analyses

4.3.1 Information Surprise of the Pay Ratio Disclosure

Having shown that the initial PRD determines employee departures and the destination of such departures, we next consider what specific information contained within the PRD are departing employees responding to. We do so by examining how employee responses vary with the unexpected informational component contained within each of the three metrics included in the PRD (i.e., median employee pay, pay ratio, and CEO pay).

We deploy a triple difference regression design to test how employee responses to the initial PRD vary with the information surprise contained within each disclosed metric. Specifically, we estimate the following modified Eq. (1) that includes a triple-interaction term and retains all lower-order interactions and main effects:

$$EmpDepartures_{it} = \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} \times Moderator_{it} + \tag{4}$$
$$\beta_2 Post\_Proxy_{i,t} \times Moderator_{it} + \beta_3 Moderator_{it} + \beta_4 Treat\_Dec_i \times Post\_Proxy_{i,t} +$$
$$\beta_5 Post\_Proxy_{i,t} + \sum \beta_n Controls_{it} + \psi_t + \phi_i + \varepsilon_{i,t}$$

where *Moderator* assumes the values of *EmpPaySurp*, *PayRatioSurp*, or *CEOPaySurp*. *EmpPaySurp* (*PayRatioSurp*) {*CEOPaySurp*} is an indicator that equals one if the information surprise contained within the firm's disclosed median employee pay (pay ratio) {CEO Pay} is in the top tercile of the sample distribution, and zero otherwise. We calculate the surprise contained within the disclosed median employee pay (pay ratio) {CEO pay} by first estimating its expected value, following Rouen (2020) and Boone et al. (2024), and then subtracting it from the

disclosed actual.[30]

Table 6 presents the results of estimating Eq. (4) using the three information surprise metrics. Out of the three tests, only the *EmpPaySurp* test yields a significant coefficient on the interaction term of interest (i.e., *Treat_Dec×Post_Proxy×EmpPaySurp*). These results suggest that employees respond specifically to the disclosed median pay but not to the pay ratio or CEO pay. Notably, the coefficient loading on *Treat_Dec×Post_Proxy×EmpPaySurp* is significantly positive, indicating that employees respond to the PRD by departing the employer particularly when the reported median pay is higher than expected. This is consistent with our findings that the PRD impacts employees in lower-ranked positions with lower pay engaging in upward social comparisons.

4.3.2 Workplace Environment

Our theoretical framework used to explain employees' reaction to the initial PRD rests upon the premise that PRD improves pay awareness and facilitates comparisons among employees. To test this premise, we use settings with varying degrees of pay transparency, conduciveness of pay comparisons, employees' prior turnover intentions, and media sentiment surrounding the proxy statement release to examine whether employees react to the disclosure in expected ways.

If the PRD indeed increases pay transparency, then departures should be more pronounced when pay transparency is low to begin with. Given that labor unions are associated with higher pay transparency (Freeman and Medoff 1984), we use the presence of labor union coverage within the industry to proxy for pay transparency. We test our prediction by estimating Eq. (4) where the *Moderator* assumes *LowUnion*—an indicator that equals one for firms in industries with below the median labor union coverage, and zero otherwise. As expected, in column (1) of

---

[30] We estimate the expected values using the median employee pay and the CEO pay ratio initially disclosed by the firms in our sample.

Table 7, the coefficient on the *Treat_Dec×Post_Proxy×LowUnion* is significantly positive. This

suggests that, after the initial PRD, employees are more likely to leave employers in industries

with lower pre-existing pay transparency, as captured by lower labor union coverage.

Given that employees engage in pay comparisons (e.g., Gartenberg and Wulf 2017), we

expect the PRD to allow for more meaningful pay comparisons when the median employee is

more representative of the firm's workforce, and thus the median employee pay is viewed as a

meaningful benchmark by more employees within the firm. We test this prediction by measuring

the representativeness of the disclosed median pay using the number of unique employee

occupations within the firm. We capture the representativeness of the median employee within

the firm with *LowOccupationNum*—an indicator that equals one for firms with below the sample

median number of unique employee occupations, and zero otherwise.[31] Table 7 column (2)

presents results of estimating Eq. (4) where *Moderator* assumes the values of

LowOccupationNum. The significantly positive coefficient on the *Treat_Dec×Post_Proxy×*

LowOccupationNum is in line with our expectations as it indicates that the employee response to

PRD is more pronounced for firms where the median employee is more likely to be

representative of the workforce.

If the disclosure of the median pay indeed contributes to an employee's decision to leave the

firm, then intuitively its effect should be more pronounced for employees already harboring

intentions to depart from their employer but who have not yet done so. We next measure

employee-departure intentions at the firm by examining employees' job search activity as

captured by Glassdoor.com reviews (deHaan et al. 2023). Specifically, we measure the

proportion of a firm's workforce performing online job searches prior to PRD as the number of

---

[31] The unique number of occupations are based on the 150 job-role categories determined by *Revelio Labs* and calculated for each firm as of Dec 2017.

employee reviews submitted to Glassdoor.com by current employees during the 2016 calendar year divided by the number of employees at the firm six months before the annual proxy filing. We capture high departure intentions with *HiTurnoverIntent*—an indicator that equals one for firms with higher than the median proportion of employees engaged in online job-search activity, and zero otherwise. We then re-estimate Eq. (4) using *HiTurnoverIntent* as the moderator and report results in column (3) of Table 7. The coefficient on *Treat_Dec×Post_Proxy× HiTurnoverIntent* is significantly positive as expected. This suggests the PRD increases the likelihood that employees, who may have been merely thinking about leaving the firm, act upon their intentions and actually depart the firm.

Finally, we examine whether the media sentiment around the release of the proxy statement influences the relation between employee departures and the disclosure of median employee pay. Under the expectation that the media serves as a conduit to transmit information to rank-and-file employees (i.e., Lee, Ng, Shevlin, and Venkat 2021), we expect that firms that receive more unfavorable media coverage around a pay ratio disclosure may incur higher employee departures. *NegMediaTone* is an indicator variable equal to one if a firms' composite sentiment score (CSS) from Ravenpack was less than the sample median in the +/- five-day window surrounding the release of a given year's proxy statement, and zero otherwise. In column (4) of Table 7, the coefficient on *Treat_Dec×Post_Proxy× LowMediaTone* is significantly positive, suggesting that the media is one potential channel through which employees receive proxy statement related information.

5. Extended Analyses

5.1 Job Search

Our tests thus far provide focus upon employee turnover as our dependent variable of

interest, and that this relation varies in the cross-section in intuitive ways. We start our set of

extended analyses by utilizing an alternative measure of job displacement from deHaan et al.

(2023) and the number of Glassdoor reviews.

$$JobSearch_{it} = \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} + \beta_2 Post\_Proxy_{i,t} + \psi_t + \phi_i + \varepsilon_{i,t} \qquad (5)$$

Where *JobSearch* is the number of new Glassdoor reviews submitted during for firm *i* during

week *t*. Other variables are defined as previously. We expect that job searches will increase for

pay ratio reporting firms relative to non-disclosing firms in the weeks following the release of

the proxy statement. We start with a nine-week window [-4, +4] surrounding the proxy

statement. In Table 8 column (1), we find evidence that relative to non-PRD disclosing firms,

PRD disclosing firms experience higher job search activities in the four weeks following their

proxy statement disclosure. We continue to observe this relationship in column (2) when we

extend the window to thirteen weeks [-6, +6] similar to deHaan et al. (2023). We find similar,

albeit weaker, evidence in column (3) when we double the job search window to twenty-five

weeks [-12,+12].[32]

5.2 Employee Replacement

Thus far, we provide evidence of increased labor-market mobility due to voluntary

employee departures following the initial PRD, suggesting that the median pay disclosure leads

to human capital retention issues for the average disclosing firm. Given that firms may need to

replace departing employees with new hires, for completeness, we conduct an additional analysis

to examine whether the PRD impacts disclosing firms' ability to attract and recruit new talent

(i.e., hiring activity to replace departing employees).

---

[32] The attenuation in our results with a longer window is consistent with Carter et al. (2025), who find no evidence
that monthly Glassdoor reviews increase using an approximate +/- 8 month sample (22,565 firm months/1,360
firms) ÷ 2.

Specifically, we examine employee replacements within the firm by re-estimating Eq. (1) using several different dependent variables: (1) *EmpHires*, the monthly percentage of employees hired during the month, as the dependent variable; (2) *Net Outflows*, which is the difference between *EmpDepartures* and *EmpHires;* and (3) *Churn*, which is the difference between worker flows (i.e., the sum of employee hires and departures) and job flows (i.e., the absolute value of change in employees) scaled by the number of accounting employees for the month at the firm (Burgess, Lane, and Stevens 2000; Grinza 2021). In plain terms, *Churn* captures the replacement rate of employees within the firm while holding the expansion and contraction of the firm's workforce constant.

Column (1) of Table 9 presents the estimation results using the sample covering the monthly window around the proxy filing, the same window used for our main tests of employee departures. We find that *Treat_Dec×Post_Proxy* is positive and statistically significant, suggesting that employee hiring activity increases during the six months following the disclosure, perhaps to offset the loss in talent. In column (2) of Table 9, similar to Carter et al. (2025), we find no statistical evidence of net change in aggregate employment, as the increases in hiring appear to largely offset the departures that the company experienced. Finally, in column (3) of Table 9 we observe that *Churn* increases following PRD reform. The collective results in Table 9 provide further evidence of a dynamic effect of the pay ratio disclosure that leads to employee displacement, which we expand on in the next section.

5.2 Aggregate Employee Mobility

Our study has thus far documented how the PRD affects employee mobility at the employee- and firm-level distributionally between disclosing and non-disclosing firms. However, the PRD reform's aggregate effects on labor market mobility remain unclear. We

attempt to provide evidence on the PRD's aggregate effects by exploiting the cross-state

variation in the share of Russell 3000 firms disclosing median pay from 2017 to 2018. Using

quarterly U.S. Census Bureau data from 2017 Q1 to 2018 Q4, we examine PRD's aggregate

impact on labor-market mobility at the state level by estimating the following model:[33]

$$J2J\_Rate_{i,t} = \beta_0 + \beta_1 PRD\_Pct_{i,t} + \theta_t + \gamma_i + \varepsilon_{it} \tag{6}$$

where *J2J_Rate* is the job-to-job change rate for state *i* during quarter *t*. *PRD_Pct* assumes

values of either *Firm_Pct* or *CumFirm_Pct*, which capture employee pay transparency at the

state level. *Firm_Pct* (*CumFirm_Pct*) is the percentage (cumulative percentage) of Russell 3000

firms located in state *i* that issue (have issued) PRD during (by) quarter *t*. State fixed effects (γ_i)

and quarter fixed effects (θ_t) are included in the model. Panel A of Table 10 presents the results

of estimating Eq. (5) using state-quarter level observations. The coefficients on *Firm_Pct* and

CumFirm_Pct are both significantly positive, indicating that overall job mobility within a state

increases with the proportion of public firms disclosing median employee pay.

As an alternative approach, we measure aggregate labor-market mobility by industry using

NAICS sectors. In this setting, *J2J_Rate* is the national job-to-job change rate for industry sector

i during quarter *t*. For this analysis, *Firm_Pct* (*CumFirm_Pct*) is defined as the percentage

(cumulative percentage) of Russell 3000 firms operating in industry sector *i* that issue (have

issued) PRD during (by) quarter *t*. Panel B of Table 10 presents the results of re-estimating Eq.

(6) to conduct this industry-quarter level analysis. The coefficients on *Firm_Pct* and

CumFirm_Pct are also both significantly positive, again providing additional evidence that

increased employee pay transparency is directly associated with overall industry-level labor

market mobility. While Table 10 results suggest that PRD has wider aggregate impacts on

---

[33] We set 2018 Q4 as the sample period end because it is the quarter by which most mandated firms were required to disclose their median pay.

employee mobility at the state and industry level, we acknowledge that our tests only provide descriptive evidence based on associations and caution against drawing causal inferences.

6. Robustness Tests

6.1 Placebo Tests

As neither the treated nor the control group disclosed their median pay for the 2016 fiscal year, we should not expect to observe any differences in employee departures between treated (December fiscal year-end) and control (June-November fiscal year-end) firms around filings of the first annual proxy after the close of the 2016 fiscal year if our results are driven by the initial PRD. We use these proxy filing events to conduct a placebo test and report results in Table 11. We find no evidence that employee departures occurring during the [-6, +6] month window around these proxy filings differ for treated and control firms. This finding provides evidence that the changes in employee mobility are attributable specifically to the initial disclosure of median pay as opposed to other information disclosed during the annual proxy filing.

6.2 Alternative Specifications

To bolster our inferences, we deploy two alternative specifications. We first introduce an alternative control group comprising Russell 3000 firms that are not subject to the PRD requirement, specifically, emerging growth companies (EGCs) and smaller reporting companies (SRCs), who are exempt from the PRD. In this test, we require that control firms have the same fiscal year-ends as treated firms (i.e., December), eliminating the possibility that differences associated with timing in fiscal year-ends confound our results. We also restrict our treated group to non-EGC and non-SRC firms with revenues below $1 billion to ensure that our treated group is comparable to the control group. We obtain an unbalanced panel comprising 4,655 firm-month observations—3,670 (985) observations for 283 (76) unique treated (control) firms—and

compare employee mobility between the treated and control groups during the [-6, +6] months

surrounding the filing month of the first annual proxy after the close of the 2017 fiscal year.[34]

Table 12 presents the results of re-estimating Eq. (1) with *Treat_NonEGCSRC*, an indicator

that equals one if a firm belongs to the treated group, and zero if a firm belongs to the alternative

control group (i.e., SRC or EGC firm). Consistent with our main results, treated firms exhibit

greater employee departures following the initial PRD as compared to EGCs and SRCs.

We acknowledge that the treatment effects estimated by a traditional staggered DiD can be

biased because it is a weighted average of different treatment effects. In response, our final test,

deploys a "stacked regression" approach (e.g., Gormley and Matsa 2011; Baker et al. 2022; and

Dambra, Mihov, and Sanz 2024) that estimates treatment effects within cohorts. We group firms

into cohorts based on the month in which the annual proxy statement with PRD is first reported.

For each cohort, we define firms that issue their initial annual proxy statement with the PRD as

treat firms and deploy one of two alternative control groups where one control group consists of

non-disclosers only and the second consists of non-disclosers *and* late disclosers. Specifically,

the first control group comprises all pubic firms (covered by Compustat and CRSP) that do not

disclose median pay for either fiscal year 2017 or 2018. The second control group includes firms

from the first group (i.e., those firms that do not disclose median pay for either fiscal year 2017

or 2018) along with all firms disclosing median pay at least 6 months after the treatment firms in

the cohort. In Table 13, we document similar results utilizing a stacked-DiD with either control

group in that employee departures appear to increase within a cohort for newly disclosing firms.

6.1 Parallel Trends Assumption

Our analyses rely on the parallel (common) trends assumption being valid, a prerequisite for

---

[34] Four observations are omitted from the regression due to the control of fixed effects.

implementing a difference-in-differences design (Angrist and Pischke 2008). Specifically,

employee departures should exhibit parallel trends for treated (December fiscal year-end) and

control (June-November fiscal year-end) firms during the pre-disclosure period if we are to

attribute any changes in employee departures to the initial PRD. To test the parallel trends

assumption, we estimate the following regression for the pre-proxy filing period:

$$
\begin{aligned}
EmpDepartures_{i,t} \\
= \beta_1 Treat\_Dec_i \times Time_{i,t} + \beta_2 Time_{i,t} + \sum \beta_n Controls_{it} + \psi_t + \phi_i \\
+ \varepsilon_{it}
\end{aligned}
\tag{8}
$$

where *Time* ranges from 1 to 6 corresponding to each month in the pre-period relative to the

proxy filing month (e.g., *Time* takes a value of 1 [6] for the month that falls six [one] months -

before the filing month). If there are parallel trends during the pre-disclosure period, then the

coefficient on *Treat_Dec × Time* should be insignificantly different from zero. As per Appendix

B, the coefficient on *Treat_Dec × Time* is insignificantly different from zero, which indicates

that treatment and control groups do not display differing trends in employee departure rates

during the pre-disclosure period. This evidence, coupled with our placebo tests in Table 11

jointly support the parallel trends assumption in our setting.

7. Conclusion

To better understand the effects of employee-pay transparency on labor market mobility, we

exploit the introduction of the PRD. Specifically, we document an increase in employee

departures following the initial PRD, suggesting increased pay transparency facilitates pay

comparisons among employees and informs their labor market decisions.

In line with relative deprivation theory, we find that employees in lower-ranked positions

within the firm and employees who are paid less, are most likely to depart their employer.

Departing employees are also more likely to join new employers with seemingly more generous

pay practices. We also find that employee responses are driven specifically by the unexpected information contained in the disclosed median employee pay, but not that contained in the pay ratio or CEO pay.

Finally, we find evidence of increases in firm-level employee churning and aggregate-level labor-market mobility as firms become subject to the PRD. In summary, we provide some of the first evidence that the PRD shapes the employee's departure decision, and that pay comparisons serve to increase employee mobility. In doing so, we provide novel evidence that the PRD informs employees' labor market decisions and shapes their labor choices, which is important given human capital's increasingly important role in facilitating firm success (Zingales 2000). Our findings further imply that labor reallocation might serve as one mechanism through which PRD affects firm value (Pan et al. 2022).

Our study documents the unintended consequences of PRD, highlighting pay transparency's role in human capital management and its implications for employees, employers, and regulators. While greater pay transparency may benefit labor market participants by allowing them to incorporate new information into their job transition decisions, pay transparency policies are also more likely to make attracting and retaining employees more difficult for certain firms. Recent regulation aims to enhance cross-firm pay transparency by mandating disclosure of pay ranges in job postings (Arnold et al. 2023; Vinopal 2023). Our study illustrates how mandating greater pay transparency leads to unintended labor-market consequences, underscoring the need to thoroughly assess the potential implications of pay transparency-based regulations.

Reference

Adams, J. S. 1963. "Towards an Understanding of Inequity." *Journal of Abnormal and Social Psychology* 67(5): 422.

Aguilar, Luis. 2015. "The CEO Pay Ratio Rule: A Workable Solution for Both Issuers and Investors." Securities and Exchange Commission (SEC). Available at: https://www.sec.gov/news/statement/statement-open-meeting-pay-ratio-aguilar

Alterman, V., P. A. Bamberger, M. Wang, J. Koopmann, E. Belogolovsky, and J. Shi. 2021. "Best not to Know: Pay Secrecy, Employee Voluntary Turnover, and the Conditioning Effect of Distributive Justice." *Academy of Management Journal* 64(2): 482-508.

Angrist, J. D., and J. S. Pischke. 2009. "Mostly Harmless Econometrics: An Empiricist's Companion." Princeton University Press.

Arnold, D., S. Quach, and B. Taska. 2022. "The Impact of Pay Transparency in Job Postings on the Labor Market." Available at SSRN: https://ssrn.com/abstract=4186234

Baker, A.C., Larcker, D.F. and Wang, C.C. 2022. "How Much Should We Trust Staggered Difference-in-Differences Estimates?." *Journal of Financial Economics* 144(2): 370-395.

Biven, J., and J. Kandra. 2022. "CEO Pay Has Skyrocketed 1,460% since 1978. Economic Policy Institute." Available at: https://www.epi.org/publication/ceo-pay-in-2021/

Bloom, M. and J. G. Michel. 2002. "The Relationships among Organizational Context, Pay Dispersion, and among Managerial Turnover." *Academy of Management Journal* 45(1): 33-42.

Boone, A., A. Starkweather, and J. T. White. 2024. "The Saliency of the CEO Pay Ratio." *The Review of Finance* 28(3): 1059-1104.

Breza, E., S. Kaur, and Y. Shamdasani. 2018. "The Morale Effects of Pay Inequality." *The Quarterly Journal of Economics* 133(2): 611-663.

Burgess, S., Lane, J. and Stevens, D. 2000. "Job Flows, Worker Flows, and Churning." *Journal of Labor Economics* 18(3): 473-502.

Burn, I. and Kettler, K., 2019. The More You Know, the Better You're Paid? Evidence from Pay Secrecy Bans for Managers. *Labour Economics*, *59*, pp.92-109.

Card, D., A. Mas, E. Moretti, and E. Saez. 2012. "Inequality at Work: The Effect of Peer Salaries on Job Satisfaction." *American Economic Review* 102(6): 2981-3003.

Carter, M. E., L. LaViers, J. Sandvik, and D. Xu. 2025. "Employee Responses to Increased Pay Transparency: An Examination of Glassdoor Ratings and the CEO Pay Ratio Disclosure." Available at SSRN: Available at SSRN: https://ssrn.com/abstract=4430286

Chang, W., M. Dambra, B. Schonberger, and I. Suk. 2023. "Does Sensationalism Affect Executive Compensation? Evidence from Pay Ratio Disclosure Reform." *Journal of Accounting Research* 61(1): 187-242.

Chen, C. X., V. Fung, and L. LaViers. 2023. "Labor Market Participants' Reactions to Salary Range Disclosures." Available at SSRN: https://ssrn.com/abstract=4599573

Chi, W., H. Liao, L. Wang, R. Zhao, and Q. Ye. 2019. "Incentives to Move up: Effects of Pay Gaps between Levels on Employee Performance." *Human Resource Management Journal* 29(2): 238-253.

Choi, B. G., J. H. Choi, and S. Malik. 2023. "Not just for investors: The role of earnings announcements in guiding job seekers." *Journal of Accounting and Economics* 76(1): 101588.

Cohn, A., E. Fehr, B. Herrmann, and F. Schneider. 2014. "Social Comparison and Effort Provision: Evidence from a Field Experiment." *Journal of the European Economic Association* 12(4): 877-898.

Cullen, Z. B., and B. Pakzad-Hurson. 2023. "Equilibrium Effects of Pay Transparency." *Econometrica* 91(3): 765-802.

Cullen, Z., and R. Perez-Truglia. 2022. "How Much Does Your Boss Make? The Effects of Salary Comparisons." *Journal of Political Economy* 130(3): 766-822.

Cullen, Z.B. 2023. "Is Pay Transparency Good?" (No. w31060). *National Bureau of Economic Research*.

Dambra, M., Mihov, A. and Sanz, L. 2024. "Unintended Real Effects of EDGAR: Evidence from Corporate Innovation." *The Accounting Review* 99(6): 75-99.

Dehaan, E., N. Li, and F. S. Zhou. 2023. "Financial Reporting and Employee Job Search." *Journal of Accounting Research* 61(2): 571-617.

Dunning, D., C. Heath, and J. M. Suls. 2004. "Flawed Self-assessment: Implications for Health, Education, and the Workplace." *Psychological Science in the Public Interest* 5(3): 69-106.

Faleye, O., E. Reis, and A. Venkateswaran. 2013. "The Determinants and Effects of CEO–Employee Pay Ratios." *Journal of Banking and Finance* 37(8): 3258-3272.

Festinger, L. 1954. "A Theory of Social Comparison Processes." *Human Relations* 7(2): 117-140.

Freeman, R. B. and J. L. Medoff, 1984. "What Do Unions Do." *Industrial and Labor Relations Review* 38: 244.

Gächter, S., and C. Thöni. 2010. "Social Comparison and Performance: Experimental Evidence on the Fair Wage–Effort Hypothesis." *Journal of Economic Behavior and Organization* 76(3): 531-543.

Gartenberg, C., and J. Wulf. 2017. "Pay Harmony? Social Comparison and Performance Compensation in Multibusiness Firms." *Organization Science* 28(1): 39-55.

Gerhart, B., and S. Rynes. 2003. "Compensation: Theory, Evidence, and Strategic Implications." Sage.

Gormley, T. A., and D. A. Matsa. 2011. Growing out of trouble? Corporate responses to liability risk. *Review of Financial Studies* 24 (8): 2781–2821.

Green, T., S. Markov, and D. Zhou. 2023. "Pay Inequality and Job Satisfaction: Evidence from Glassdoor." Available at SSRN: https://ssrn.com/abstract=3415937

Greenberg, J., C. E. Ashton-James, and N. M. Ashkanasy. 2007. "Social Comparison Processes in Organizations." *Organizational Behavior and Human Decision Processes* 102(1): 22-41.

Hayes, J., and H. Heidi. 2011. "Women and Men Living on the Edge: Economic Insecurity after the Great Recession." Washington, DC: Institute for Women's Policy Research, (IWPR# C386). Available at: https://iwpr.org/wp-content/uploads/2020/09/C386.pdf

Hirsch, B. T., and D. A. Macpherson. 2003. "Union Membership and Coverage Database from the Current Population Survey: Note." *Industrial and Labor Relations Review* 56(2): 349-354.

Kacperczyk, A., and C. Balachandran. 2018. "Vertical and Horizontal Wage Dispersion and Mobility Outcomes: Evidence from the Swedish Microdata." *Organization Science* 29(1): 17-38.

Kesseler., 2023. "Does It Help to Know How Much the Boss Makes?. " *The New York Times*. Available at: https://www.nytimes.com/2023/07/01/business/ceo-pay-worker-salary.html

Kiatpongsan, S., and M. I. Norton. 2014. "How Much (More) Should CEOs Make? A Universal Desire for More Equal Pay." *Perspectives on Psychological Science* 9(6): 587-593.

Lawler, E. E. 1965. "Managers' Perceptions of Their Subordinates' Pay and of Their Superiors' Pay." *Personnel Psychology* 18(4): 413-422

Lee, Y., Ng, S., Shevlin, T. and Venkat, A. 2021. "The Effects of Tax Avoidance News on Employee Perceptions of Managers and Firms: Evidence from Glassdoor. com ratings." *The Accounting Review* 96(3): 343-372.

Liang, C., B. Lourie, H. Qu, and F. Y. Wang. 2024. "Pay Ratio Disclosure and Employee Turnover." Available at SSRN: https://ssrn.com/abstract=4702852

Mas, A., 2017. "Does transparency lead to pay compression?." *Journal of Political Economy 125*(5): 1683-1721.

Menendez, B. 2015. "Menendez Reacts to SEC Vote Approving CEO-to-Worker Pay Ratio Rule." Available at: https://www.menendez.senate.gov/newsroom/press/menendez-reacts-to-sec-vote-approving-ceo-to-worker-pay-ratio-rule

Mobley, W. H., S. O. Horner, and A. T. Hollingsworth. 1978. "An Evaluation of Precursors of Hospital Employee Turnover." *Journal of Applied Psychology* 63(4): 408.

Moore, D. A. 2007. "Not so above Average after all: When People Believe They Are Worse than Average and its Implications for Theories of Bias in Social Comparison." *Organizational Behavior and Human Decision Processes* 102(1): 42-58.

O'Neill, B. P. 2010. "Pay Confidentiality: a Remaining Obstacle to Equal Pay after Ledbetter." *Seton Hall Law Review* 40: 1217.

Pan, Y., E. S. Pikulina, S. Siegel, and T. Y. Wang. 2022. "Do Equity Markets Care about Income Inequality? Evidence from Pay Ratio Disclosure." *The Journal of Finance* 77(2): 1371-1411.

Peterson, C., 2000. "The Future of Optimism." *American Psychologist* 55(1): 44.

Pfeffer, J., and N. Langton. 1993. "The Effect of Wage Dispersion on Satisfaction, Productivity, and Working Collaboratively: Evidence from College and University Faculty." *Administrative Science Quarterly* 38(3): 382–407.

Piwowar, M. S. 2015. "Additional Dissenting Comments on Pay Ratio Disclosure. Securities and Exchange Commission (SEC) Statement of Commissioner." Available at: https://www.sec.gov/news/statement/additional-dissenting-statement-pay-ratio-disclosure#_ftnref16

Rouen, E., 2020. "Rethinking Measurement of Pay Disparity and Its Relation to Firm Performance." *The Accounting Review* 95(1): 343-378.

Securities and Exchange Commission (SEC). 2015. "Pay Ratio Disclosure." Release Nos. 33-9877, 34-75610. Washington, DC: SEC. Available at: https://www.sec.gov/files/rules/final/2015/33-9877.pdf

Shaw, J. D., J. E. Delery, G. D. Jenkins Jr, and N. Gupta. 1998. "An Organization-level Analysis of Voluntary and Involuntary Turnover." *Academy of Management Journal* 41(5): 511-525.

Shue, K. 2013. "Executive Networks and Firm Policies: Evidence from the Random Assignment of MBA Peers." *The Review of Financial Studies* 26(6): 1401-1442.

Smith, H. J., T. F. Pettigrew, G. M. Pippin, and S. Bialosiewicz. 2012. "Relative Deprivation: A Theoretical and Meta-analytic Review." *Personality and Social Psychology Review* 16(3): 203-232.

Statista Research Department, 2023. "U.S. CEO-to-Worker Compensation Ratio of Top Firms 1965-2021." Available at: https://www.statista.com/statistics/261463/ceo-to-worker-compensation-ratio-of-top-firms-in-the-us/

Sweeney, P. D., and D. B., McFarlin. 2005. Wage Comparisons with Similar and Dissimilar Others. *Journal of Occupational and Organizational Psychology* 78(1): 113-131.

The Guardian. 2021. "US Millionaire CEOs Saw 29% Pay Raise while Workers' Pay Fell, Report Finds." Available at: https://www.theguardian.com/business/2021/may/11/us-millionaire-ceos-saw-29-pay-raises-while-workers-had-decreases-report-says

The Guardian. 2022. "Wage Gap between CEOs and US Workers Jumped to 670-to-1 Last Year, Study Finds." Available at: https://www.theguardian.com/us-news/2022/jun/07/us-wage-gap-ceos-workers-institute-for-policy-studies-report

Tonti, J. 2022. "Survey Report: Companies Should Reduce Income Inequality by Raising Minimum Wage to Living Wage and Capping CEO Compensation." Available at: https://justcapital.com/wp-content/uploads/2022/05/JUST-Capital_Worker-CEO-Pay-Survey-Analysis_May-2022-min.pdf

Trotter, R. G., S. R. Zacur, and L. T. Stickney. 2017. "The New Age of Pay Transparency." *Business Horizons* 60(4): 529-539.

Vinopal, C. 2023. "Knowing Everyone's Salaries Can Light a Fire Under Workers." *The Wall Street Journal*. Available at: https://www.wsj.com/articles/knowing-everyones-salaries-can-light-a-fire-under-workers-3fb0da17

Wade, J. B., C. A. O'Reilly III, and T. G. Pollock. 2006. "Overpaid CEOs and Underpaid Managers: Fairness and Executive Compensation." *Organization Science* 17(5): 527-544.

Yeung, K. K. S. 2021. "Real Effects of the CEO Pay Ratio Disclosure Mandate."

Zenger, T. R. 1992. "Why do Employers only Reward Extreme Performance? Examining the Relationships among Performance, Pay, and Turnover." *Administrative Science Quarterly*:198-219.

Zingales, L. 2000. "In Search of New Foundations." *The Journal of Finance* 55(4): 1623-1653.

Figure 1
Difference-in-Differences Research Design

Firms are required to provide Pay Ratio Disclosure (PRD) for reporting periods beginning on or after Jan 1, 2017. We use December 31, 2017 as the cutoff date to differentiate between treated and control firms and deploy a difference-in-differences research design. Firms with December fiscal year-ends represent the first cohort affected by the disclosure mandate and are assigned to the treated group. Firms with June through November fiscal year-ends are assigned to the control group. This figure illustrates our research design using the treated firm PayPal Holdings, Inc. (with December fiscal year-end) and the control firm Visa, Inc. (with September fiscal year-end). While PayPal is subject to the PRD rule when filing its annual proxy statement after the close of its 2017 fiscal year, Visa is not. We examine the [-6, +6] month window surrounding the first annual proxy filing that follows the close of the 2017 fiscal year.
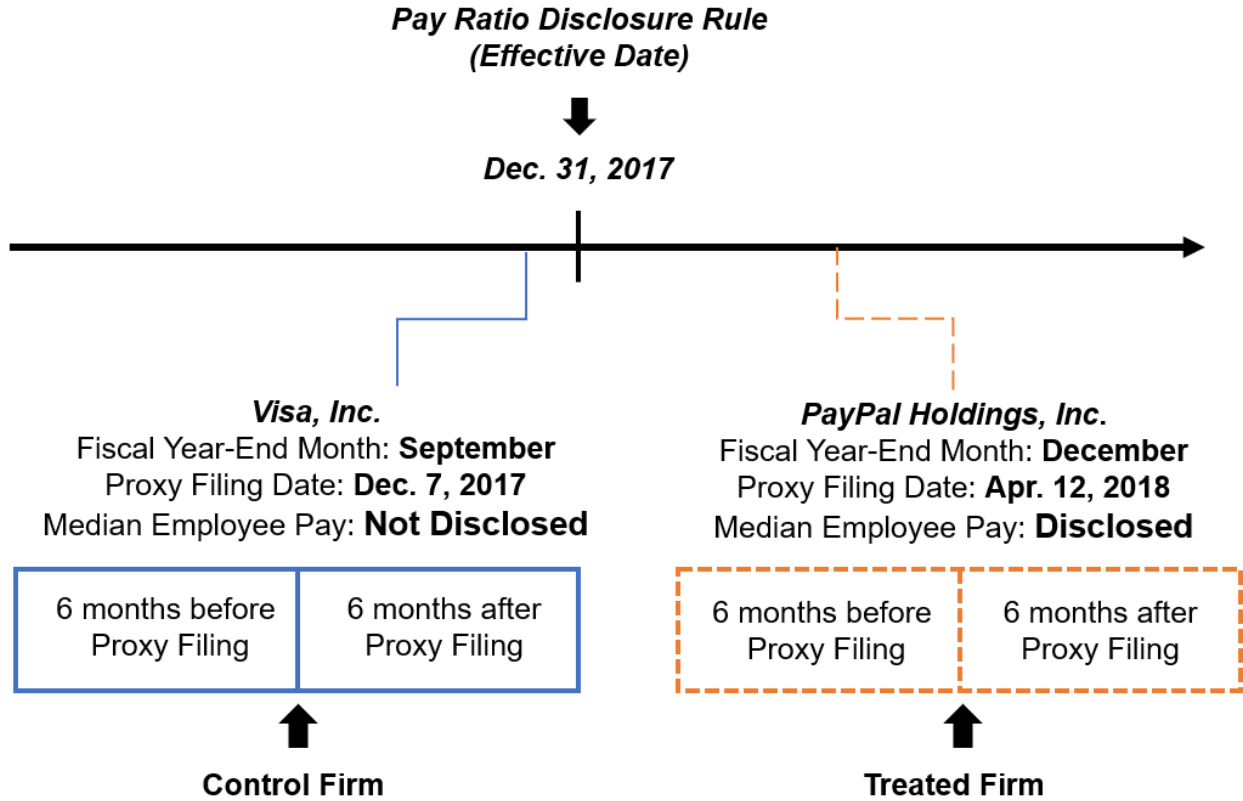
Table 1

Sample Description

Panel A: Sample selection process

	Observations
Russell 3000 firms at the intersection of Compustat and CRSP for fiscal year 2017	2,813
Less: firms that are exempted from the requirement (i.e., EGCs, SRCs, and FPIs)	(154)
Less: firms that do not disclose the pay ratio despite the mandate	(385)
Less: firms with the fiscal year ending between January and May	(259)
Less: Firms in the financial industry	(547)
Less: firms without an identifiable proxy filing date, employment data, or control variables	(573)
Total number of unique firms in the final sample (731 treated and 164 control)	**895**
Firm-month observations during [-6, +6] months surrounding the annual proxy filing month	**11,615**

Panel B: Distribution of filing month of annual proxy following the close of the fiscal year 2017

Year-Month	Total	Treat	Control
2017-9	26	0	26
2017-10	32	0	32
2017-11	13	0	13
2017-12	32	0	32
2018-1	37	0	37
2018-2	18	3	15
2018-3	310	307	3
2018-4	408	405	3
2018-5	8	7	1
2018-6	3	2	1
2018-7	4	3	1
2018-8	3	3	0
2018-9	1	1	0
Total	895	731	164

Table 2

Sample Distribution

Panel A: By Calendar year-month

Year-Month	N	*EmpDepartures (%)*
2017-03	26	1.009
2017-04	58	1.065
2017-05	71	1.081
2017-06	102	1.228
2017-07	139	1.047
2017-08	157	1.443
2017-09	468	1.029
2017-10	875	0.957
2017-11	883	0.827
2017-12	886	1.306
2018-01	887	1.571
2018-02	890	0.946
2018-03	890	1.153
2018-04	868	1.060
2018-05	837	1.092
2018-06	824	1.169
2018-07	792	1.140
2018-08	755	1.637
2018-09	737	1.111
2018-10	427	1.105
2018-11	19	0.715
2018-12	11	1.096
2019-01	8	1.124
2019-02	4	0.865
2019-03	1	0.880
Total	**11,615**	**1.153**

Panel B: By Fama-French 12-industry (Except for the financial industry)

Industry	N	*EmpDepartures(%)*
Consumer Non-Durables	676	1.053
Consumer Durables	429	0.983
Manufacturing	1,947	0.986
Energy	481	0.990
Chemicals	481	1.083
Business Equipment	2,145	1.323
Telecommunications	299	1.330
Utilities	559	0.796
Wholesales and Retails	954	1.005
Healthcare	1,811	1.526
Other	1,833	1.060
Total	**11,615**	**1.153**

Panel C: Summary Statistics for Variables Used in the Main Difference-in-Difference Analyses

Variables	Mean	Median	P25	P75	SD	N
EmpDepartures(%)	1.153	0.998	0.700	1.425	0.727	11,615
Treat_Dec	0.817	1.000	1.000	1.000	0.387	11,615
Post_Proxy	0.539	1.000	0.000	1.000	0.498	11,615
Size	7.797	7.718	6.597	8.853	1.609	11,615
MTB	4.159	2.896	1.795	5.436	7.043	11,615
Leverage	0.264	0.251	0.092	0.384	0.218	11,615
ROA	0.003	0.011	-0.002	0.023	0.048	11,615
RET	0.237	0.169	-0.037	0.412	0.463	11,615

Table 3

Main Analysis of Employee Departures

This table presents results from estimating the difference-in-differences model:

$$EmpDepartures_{i,t} = \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} + \beta_2 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + \psi_t + \phi_i + \varepsilon_{it}$$

EmpDepartures is the percentage of employees departing firm *i* during month *t*. *Treat_Dec* is an indicator that equals 1 for firms with a December 31 fiscal year-end, and 0 otherwise. *Post_Proxy* is an indicator that equals one for the six-month period following the filing of the first annual proxy after the close of the 2017 fiscal year, and zero otherwise. Columns (1) and (2) are estimated using ordinary least squares (OLS) regressions and weighted regression with weights for treated and control firms assigned via entropy balancing (EB), respectively. All models include firm fixed effects and year-month fixed effects. Year-month fixed effects do not subsume *Post_Proxy* because the proxy filing month varies across firms. Appendix A provides detailed variable definitions along with data sources; *t*-statistics, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)	(2)
	OLS	*EB*
	EmpDepartures	*EmpDepartures*
Treat_Dec×Post_Proxy	0.111***	0.100***
	(3.970)	(3.496)
Post_Proxy	-0.154***	-0.137***
	(-5.502)	(-5.044)
Size	-0.131**	-0.071
	(-2.304)	(-1.474)
MTB	0.001	0.003
	(0.636)	(1.161)
Leverage	-0.086	-0.046
	(-0.498)	(-0.411)
ROA	0.088	0.082
	(0.288)	(0.252)
RET	-0.003	-0.005
	(-0.134)	(-0.229)
Firm FE	Yes	Yes
Year-month FE	Yes	Yes
N	11,615	11,615
Adjusted-R^2	0.55	0.55

Notes: The constant term is excluded from the table throughout the paper.

Table 4

Within-firm Pay Comparisons and Employee Departures

This table presents the results of estimating the triple difference-in-differences model:

$$EmpDepartures_{i,j,t}$$
$$= \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} \times PositionFactor_{i,j,t} + \beta_2 Post\_Proxy_{i,t} \times PositionFactor_{i,j,t}$$
$$+ \beta_3 PositionFactor_{i,j,t} + \beta_4 Treat\_Dec_i \times Post\_Proxy_{i,t}$$
$$+ \beta_5 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + \psi_t + \phi_i + \varepsilon_{i,t}$$

EmpDepartures is the percentage of employees in group *j* departing firm *i* during month t. *Treat_Dec* is an indicator that equals 1 for firms with a December 31 fiscal year-end, and 0 otherwise. *Post_Proxy* is an indicator that equals one for the six-month period following the filing of the first annual proxy after the close of the 2017 fiscal year, and zero otherwise. *PositionFactor* assumes values of either *LowRank or BelowMedianPay. LowJobRank* is the juniority level (1, 2, 3, or 4) for each group of positions, where 4 represents the most junior group. *BelowMedianPay* is an indicator variable that equals one for the group of positions with estimated pay below the median employee pay a firm initially disclosed, and zero otherwise. Firm fixed effects and year-month fixed effects are included in the model. Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)	(2)
PositionFactor =	*LowRank*	*BelowMedianPay*
Treat_Dec×Post_Proxy×PositionFactor	0.103***	0.308***
	(7.190)	(3.549)
Post_Proxy× PositionFactor	0.004	-0.219***
	(0.282)	(-2.675)
PositionFactor	0.106***	-0.154***
	(14.888)	(-5.878)
Treat_Dec×Post_Proxy	-0.028	-0.066
	(-0.851)	(-1.016)
Post_Proxy	-0.161***	-0.044
	(-4.639)	(-0.713)
Size	-0.106*	-0.120**
	(-1.936)	(-1.978)
MTB	0.000	-0.000
	(0.185)	(-0.089)
Leverage	0.082	-0.214
	(0.532)	(-1.046)
ROA	0.168	-0.124
	(0.523)	(-1.133)
RET	-0.016	-0.008
	(-0.596)	(-0.298)
Firm FE	Yes	Yes
Year-month FE	Yes	Yes
N	46,460	18,185
Adjusted-R^2	0.29	0.28

Table 5

Destination of Individual-Employee Departures

This table presents analyses of individual employees' cross-firm movement outcomes. Panel A presents summary statistics for variables. Panel B presents results from estimating the following difference-in-differences model:

$$HigherEmppay_{i,j,t}$$
$$= \beta_1 Treat\_Dec_{i,j,t} \times Post\_Proxy_{i,j,t} + \beta_2 Post\_Proxy_{i,j,t} + \beta_3 Salary_{i,j,t} + \beta_4 Seniority_{i,j,t}$$
$$+ \psi_t + \phi_i + \varepsilon_{i,j,t}$$

HigherEmpPay is an indicator that equals one if employee *j* departing firm *i* joins a destination firm having median pay above firm *i*, and zero otherwise. *Treat_Dec* is an indicator that equals one if the original firm *i* from which employee *j* departs has a December 31 fiscal year-end, and 0 otherwise. *Post_Proxy* is an indicator that equals one for employees departing during or after the month in which the first annual proxy is filed after the close of the 2017 fiscal year, and zero otherwise. Firm fixed effects and year-month (when an employee departs) fixed effects are included in the model. Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

Panel A: Summary Statistics

	Mean	P50	P25	P75	SD	N
HigherEmpPay	0.055	0.000	0.000	0.000	0.228	52,081
Treat_Dec×Post_Proxy	0.447	0.000	0.000	1.000	0.497	52,081
Treat_Dec	0.770	1.000	1.000	1.000	0.421	52,081
Post_Proxy	0.569	1.000	0.000	1.000	0.495	52,081
Salary	10.966	11.040	10.498	11.418	0.615	52,081
Seniority	2.031	2.000	1.000	2.000	1.255	52,081

Panel B: Regression Results

Sample	Full Sample	Voluntary Departures	Involuntary Departures
	(1)	(2)	(3)
	HigherEmpPay	*HigherEmpPay*	*HigherEmpPay*
Treat_Dec×Post_Proxy	0.010***	0.013***	0.003
	(2.822)	(2.853)	(0.429)
Post_Proxy	0.006	0.008	-0.002
	(1.201)	(1.577)	(-0.243)
Salary	0.022***	0.026***	0.008
	(3.765)	(4.314)	(1.038)
Seniority	-0.000	-0.001	-0.001
	(-0.301)	(-0.398)	(-0.306)
Firm FE	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
N	52,028	35,898	16,013
Adjusted-R^2	0.04	0.04	0.04

Notes: The total observation from the two sub-samples does not equal the full sample, as some observations are omitted due to the control of fixed effects.

Table 6

Information Surprise of PRD and Employee Departures

This table presents the results of estimating the triple difference-in-differences model:

$$EmpDepartures_{it} = \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} \times Moderator_{it} + \beta_2 Post\_Proxy_{i,t} \times Moderator_{it} + \beta_3 Moderator_{it} + \beta_4 Treat\_Dec_i \times Post\_Proxy_{i,t} + \beta_5 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + \pounds_i + \mu_t + \varepsilon_{i,t}$$

EmpDepartures is the percentage of employees departing firm *i* during month t. *Moderator* assumes values of *EmpPaySurp*, *PayRatioSurp*, or *CEOPaySurp*. *EmpPaySurp [PayRatioSurp] {CEOPaySurp}* is an indicator that equals one for firms with the unexpected median-employee pay [pay ratio] {CEO pay} in the top tercile, and zero otherwise. *Treat_Dec* is an indicator that equals 1 for firms with a December 31 fiscal year-end, and 0 otherwise. *Post_Proxy* is an indicator that equals one for the six-month period following the filing of the first annual proxy after the close of the 2017 fiscal year, and zero otherwise. Firm fixed effects and year-month fixed effects are included in the model. Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)	(2)	(3)
Moderator =	*EmpPaySurp*	*PayRatioSurp*	*CEOPaySurp*
Treat_Dec×Post_Proxy×Moderator	0.228*	0.031	0.053
	(1.670)	(0.366)	(0.485)
Post_Proxy× Moderator	-0.170	-0.016	-0.071
	(-1.273)	(-0.186)	(-0.666)
Treat_Dec×Post_Proxy	0.090	0.134**	0.136**
	(1.609)	(2.260)	(2.298)
Post_Proxy	-0.148***	-0.180***	-0.173***
	(-2.859)	(-3.119)	(-2.925)
Size	-0.145**	-0.139**	-0.139**
	(-2.409)	(-2.280)	(-2.281)
MTB	0.001	0.001	0.001
	(0.496)	(0.467)	(0.396)
Leverage	-0.075	-0.075	-0.075
	(-0.393)	(-0.386)	(-0.383)
ROA	-0.025	-0.019	-0.019
	(-0.080)	(-0.059)	(-0.059)
RET	-0.004	-0.004	-0.005
	(-0.164)	(-0.162)	(-0.179)
Firm FE	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
N	10,150	10,150	10,150
Adjusted-R2	0.54	0.54	0.54

Table 7

Workplace Environment and Employee Departures

This table presents the results of estimating the triple difference-in-differences model:

$$EmpDepartures_{it} = \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} \times Moderator_{it} + \beta_2 Post\_Proxy_{i,t} \times Moderator_{it} + \beta_3 Moderator_{it} + \beta_4 Treat\_Dec_i \times Post\_Proxy_{i,t} + \beta_5 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + £_i + \mu_t + \mathcal{E}_{i,t}$$

EmpDepartures is the percentage of employees departing firm *i* during month t. In panel A, *Moderator* assumes the values of *LowUnion*, *LowOccupationNum*, *HiTurnoverIntent*, or *LowMediaTone*. *LowUnion* is an indicator that equals one for firms in industries with a union coverage below the sample median, and zero otherwise; *LowOccupationNum* is an indicator that equals one for firms with the number of occupation types below the sample median, and zero otherwise. *HiTurnoverIntent*—an indicator variable capturing high employee turnover intentions immediately prior to PRD—equals one for firms with the number of Glassdoor.com reviews submitted by current employees during the 2016 calendar year (scaled by the number of employees 6 months before the proxy filing) that is above the sample median, and zero otherwise. *LowMediaTone* is an indicator that equals one for firms with a composite sentiment score (*CSS*) of media coverage below the sample median, and zero otherwise. *Treat_Dec* is an indicator that equals one for firms with a December 31 fiscal year-end, and zero otherwise. *Post_Proxy* is an indicator that equals one for the six-month period following the filing of the first annual proxy after the close of the 2017 fiscal year, and zero otherwise. Firm fixed effects and year-month fixed effects are included in the model. Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)	(2)	(3)	(4)
Moderator =	*LowUnion*	*LowOccupationNum*	*HiTurnoverIntent*	*NegMediaTone*
Treat_Dec×Post_Proxy×Moderator	0.117**	0.123***	0.068*	0.061*
	(2.272)	(2.923)	(1.665)	(1.801)
Post_Proxy×Moderator	-0.075	-0.125***	-0.042	-0.095**
	(-1.650)	(-3.479)	(-1.273)	(-2.535)
Treat_Dec×Post_Proxy	0.071	0.052*	0.066**	0.083**
	(1.480)	(1.744)	(2.282)	(2.326)
Post_Proxy	-0.130**	-0.095***	-0.117***	-0.109**
	(-2.732)	(-3.157)	(-3.974)	(-2.592)
Size	-0.127**	-0.131**	-0.067	-0.140**
	(-2.527)	(-2.303)	(-1.142)	(-2.710)
MTB	0.000	0.001	-0.000	0.001
	(0.342)	(0.654)	(-0.006)	(0.713)
Leverage	-0.056	-0.089	-0.127	-0.083
	(-0.449)	(-0.517)	(-0.630)	(-0.735)
ROA	0.164	0.097	0.153	0.084
	(0.501)	(0.317)	(0.464)	(0.278)
RET	-0.003	-0.004	-0.036	-0.002
	(-0.077)	(-0.171)	(-1.393)	(-0.047)
Firm FE	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
N	9,924	11,614	8,760	11,614
Adjusted-R2	0.55	0.54	0.60	0.54

*Note: Firm fixed effects absorb the coefficients on the stand-alone moderator variable (i.e., *LowUnion, LowOccupationNum, HiTurnoverIntent*) as it is constructed at the firm level and has little temporal within-firm variation.

Table 8

Job Search around the Disclosure

This table presents the results of difference-in-difference tests for job search following Eq. (5).

$$Search_{it} = \beta_0 + \beta_1 Treat\_Dec_i \times Post\_Proxy =_{it} + \beta_2 Post\_Proxy_{it} + \psi_t + \phi_i + \varepsilon_{it}$$

JobSearch is the number of new employee reviews submitted by current employees of firm *i* during week *t*. *Treat_Dec* is an indicator that equals 1 for firms with December fiscal-year end, and 0 otherwise. *Post_Proxy* is an indicator that equals 1 for weeks in or after the proxy filing week. Columns (1), (2), and (3) use (-4, +4), (-6, +6), and (-12, +12) weeks around the proxy filing week as the testing window, respectively. All models include firm fixed effects and week fixed effects. Appendix A provides detailed variable definitions along with data sources. *T-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)	**(2)**	**(3)**
	JobSearch	*JobSearch*	*JobSearch*
*Treat_Dec*Post_Proxy*	0.094**	0.091***	0.033*
	(2.251)	(2.684)	(1.803)
Post_Proxy	0.036	-0.020	-0.015
	(1.057)	(-0.689)	(-1.015)
Firm FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
N	6,265	11,635	20,585
Adjusted-R2	0.62	0.63	0.62

Notes: Results are robust when using the logarithm form of *JobSearch* as the dependent variable.

Table 9

Additional Analysis: Employee Replacement

This table presents the results of estimating the difference-in-differences model:

$$Emp_{i,t} = \beta_0 + \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} + \beta_2 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + \psi_t + \phi_i + \varepsilon_{it}$$

EmpHire is the percentage of employees joining firm *i* during month *t*. *EmpNetOut* is the number of employees departing a firm minus the number of employees joining and divided by the average employee count of firm *i* during month *t*. *EmpChurning* is excess worker flow, calculated as the difference between worker flows (i.e., departures and hires) and job flows (i.e., the absolute value of change in employee counts, which reflect the gross creation and destruction of jobs) divided by the average number of employees. *Treat_Dec* is an indicator that equals one for firms with a December 31 fiscal year-end, and zero otherwise. *Post_Proxy* is an indicator that equals one for the six-month period following the filing of the first annual proxy after the close of the 2017 fiscal year, and zero otherwise. Firm fixed effects and year-month fixed effects are included in the model. Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)	(2)	(3)
	EmpHires	*EmpNetOut*	*EmpChurning*
Treat_Dec×Post_Proxy	0.093**	0.023	0.144***
	(2.179)	(0.521)	(3.005)
Post_Proxy	-0.148***	-0.001	-0.191***
	(-3.180)	(-0.025)	(-3.720)
Size	0.123	-0.221***	-0.206**
	(1.571)	(-2.808)	(-2.160)
MTB	-0.002	0.003*	0.000
	(-1.115)	(1.915)	(0.180)
Leverage	-0.330	0.164	-0.431
	(-0.984)	(0.643)	(-1.155)
ROA	0.327	-0.273	0.386
	(0.702)	(-0.598)	(0.657)
RET	0.037	-0.057	0.044
	(0.839)	(-1.351)	(0.896)
Firm FE	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
N	11,614	11,614	11,614
Adjusted-R^2	0.60	0.38	0.60

Table 10

Aggregate Labor-Market Mobility

This table presents the results of estimating the model:

$$J2J\_Rate_{i,t} = \beta_0 + \beta_1 PRD\_Pct_{i,t} + \psi_t + \phi_i + \varepsilon_{it}$$

Panel A (Panel B) uses a sample of observations at the state-quarter (national industry-quarter) level. *J2J_Rate* is the job-to-job change rate in state (industry) *i* during quarter *t*. *PRD_Pct* assumes either *Firm_Pct* or *CumFirm_Pct*. *Firm_Pct* is the percentage of Russell 3000 firms located in state (industry) *i* that issue PRD during quarter *t. CumFirm_Pct* is the cumulative percentage of Russell 3000 firms located in state (industry) *i* that have issued PRD by quarter *t*. State (industry sector) fixed effects and quarter fixed effects are included in the model; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

Panel A: State-level Employee Mobility	(1) *J2J_Rate*	(2) *J2J_Rate*
Firm_Pct	0.002** (2.635)	
CumFirm_Pct		0.002*** (9.554)
State FE	Yes	Yes
Quarter FE	Yes	Yes
N	383	383
Adjusted-R^2	0.909	0.918

Panel B: Industry-level Employee Mobility	(1) *J2J_Rate*	(2) *J2J_Rate*
Firm_Pct	0.004*** (4.704)	
CumFirm_Pct		0.002*** (5.376)
State FE	Yes	Yes
Industry FE	Yes	Yes
Quarter FE	Yes	Yes
N	192	192
Adjusted-R^2	0.986	0.986

Table 11

Placebo Tests

This table presents the results of estimating the difference-in-differences model:

$$EmpDepartures_{i,t} = \beta_1 Treat\_Dec_i \times Post\_Proxy_{i,t} + \beta_2 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + \psi_t + \phi_i + \varepsilon_{it}$$

EmpDepartures is the percentage of employees departing firm *i* during month t. *Treat_Dec* is an indicator that equals one for firms with a December 31 fiscal year-end, and zero otherwise. *Post_Proxy* is an indicator that equals one for the six-month period following the filing of the first annual proxy after the close of the 2016 fiscal year, and zero otherwise. Firm fixed effects and year-month fixed effects are included in the model. Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)	(2)
	OLS	*EB*
	EmpDepartures	***EmpDepartures***
Treat_Dec×Post_Proxy	0.047	0.033
	(1.506)	(1.030)
Post_Proxy	-0.105***	-0.105***
	(-3.442)	(-3.722)
Size	-0.086	-0.077
	(-1.351)	(-1.179)
MTB	0.000	-0.001
	(0.079)	(-0.726)
Leverage	-0.038	-0.489***
	(-0.145)	(-2.746)
ROA	-0.553	-0.129
	(-1.467)	(-0.224)
RET	-0.038	-0.013
	(-1.465)	(-0.334)
Firm FE	Yes	Yes
Year-month FE	Yes	Yes
N	11,527	11,527
Adjusted-R^2	0.52	0.50

Table 12

EGC Setting

This table presents results from estimating the following difference-in-differences model.

$$EmpDepartures_{i,t} = \beta_1 Treat\_NonEGC_i \times Post\_Proxy_{i,t} + \beta_2 Post\_Proxy_{i,t} + \sum \beta_n X_{it} + \psi_t + \phi_i + \varepsilon_{it}$$

EmpDepartures is the percentage of employees departing firm *i* during month *t*. *Treat_NonEGCSRC* is an indicator that equals 1 for firms that are not emerging growth company (EGC), smaller reporting company (SRC), or foreign private issuer (FPI), and zero otherwise. *Post_Proxy* is an indicator that equals one for the six-month period following the filing of the first annual proxy after the close of the 2017 fiscal year, and zero otherwise. Firm fixed effects and year-month fixed effects are included in the model. Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)
	EmpDepartures
Treat_NonEGCSRC×Post_Proxy	0.151*
	(1.800)
Post_Proxy	-0.091
	(-0.855)
Size	-0.194
	(-1.504)
MTB	0.001
	(0.455)
Leverage	-0.163
	(-0.712)
ROA	0.695
	(1.531)
RET	0.041
	(0.724)
Firm FE	Yes
Year-month FE	Yes
N	4,651
Adjusted-R^2	0.41

Table 13

Stacked Difference-in-Difference

This table presents the results of stacked difference-in-difference tests for employee departure following Eq. (7).

$$EmpDeparture_{it} = \beta_0 + \beta_1 Treat_i^j \times Post_t^j + \sum \beta_n Controls_{it} + \psi_{tj} + \phi_{ij} + \varepsilon_{it}$$

EmpDeparture is the percentage of employees departing firm *i* during month *t*. *Treat* is an indicator that equals 1 for firm *i* that initiate pay ratio disclosure in a particular cohort *j* (defined by the year-month when the disclosure is initiated). Control firms for column (1) includes firms that have not yet initiated the pay ratio disclosure by fiscal year 2018. Control firms for column (2) includes firms that initiated their pay ratio disclosure at least 6 months later than treated firms in the cohort or have not yet initiated the pay ratio disclosure by the fiscal year 2018. *Post* is an indicator that equals 1 for month t in or after the disclosing month for treat firms in cohort *j*, and zero otherwise. The sample includes 6 months before and 6 months after the disclosing month for each cohort. All models include firm-cohort fixed effects and year-month-cohort fixed effects. Appendix A provides detailed variable definitions along with data sources. *T-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

Control group	Non-disclosing firms	Late- and non-disclosing firms
	(1)	(2)
	EmpDeparture	*EmpDeparture*
Treat×Post	0.068**	0.053**
	(2.361)	(2.154)
Size	-0.106***	-0.119***
	(-3.682)	(-4.370)
MTB	-0.000	-0.001
	(-0.322)	(-0.991)
Leverage	0.280***	0.248***
	(3.714)	(3.445)
ROA	-0.274***	-0.266***
	(-2.997)	(-3.049)
RET	-0.028***	-0.024***
	(-3.094)	(-2.685)
Firm cohort FE	Yes	Yes
Year-month cohort FE	Yes	Yes
N	68,998	75,159
Adjusted-R^2	0.38	0.38

Appendix A

Variable Definitions

Variable	Definitions	Sources
Main Analyses		
Treat_Dec	Indicator variable that equals one for firms with a December 31 fiscal year-end, and 0 otherwise.	SEC Analytics
Post_Proxy	Indicator that equals 1 for the six-month period following the filing of the first annual proxy after the close of the 2017 fiscal year, and 0 otherwise.	SEC Edgar
EmpDepartures	The percentage of employees departing a firm during a month.	Revelio Lab
Size	Firm size, measured by the natural logarithm of the firm's assets as of the quarter-end closest to a month.	Compustat
BTM	Book-to-Market value, measured by the ratio of book-value to market-value of equality as of the quarter-end closest to a month.	Compustat
Leverage	Leverage, measured by the total debt divided by the total assets as of the quarter-end closest to a month.	Compustat
ROA	Net income scaled by total assets as of the quarter-end closest to a month.	
RET	The 12-month buy-and-hold return as of the quarter-end closest to a month.	CRSP
Within-firm Comparison Analyses		
BelowMedianPay	An indicator that equals one for job positions with (estimated) employee pay below the median employee's pay initially disclosed by a firm, and zero otherwise.	Equilar; Revelio Lab
LowRank	The juniority level (1, 2, 3, or 4) for each group of positions, where 4 represents the most junior group.	Revelio Lab
Cross-firm Movement Outcome Analyses		
HigherEmpPay	An indicator that equals one for cross-firm movement where the destination firm has a higher median employee pay than the original firm, and zero otherwise.	Equilar; Revelio Lab
Salary	The logarithm of the estimated salary of a position.	Revelio Lab
Seniority	The seniority level (1, 2, 3, or 4) for each group of positions, where 4 represents the most senior group.	Revelio Lab
Cross-sectional Analyses on Information Surprise of the PRD		
EmpPaySurp	An indicator that equals one for firms with an unexpected median employee pay in the top tercile of the sample distribution, and zero otherwise.	Author's calculation
PayRatioSurp	An indicator that equals one for firms with an unexpected pay ratio in the top tercile of the sample distribution, and zero otherwise.	Author's calculation
CEOPaySurp	An indicator that equals one for firms with an unexpected CEO pay in the top tercile of the sample distribution, and zero otherwise.	Author's calculation
Cross-sectional Analyses on Workplace Environment		
LowUnion	An indicator that equals one for firms in industries with labor union coverage below the sample median, and zero otherwise (Hirsch and Macpherson 2003).	unionstats.com
LowOccupationNum	An indicator that equals one for firms with the number of occupation types below the sample median, and zero otherwise.	Revelio Lab
HiTurnoverIntent	An indicator that equals one for firms with the number of Glassdoor.com reviews submitted by current employees during the 2016 calendar year (scaled by the number of employees 6 months before the proxy filing) above the sample median, and zero otherwise.	Glassdoor.com / Revelio Lab
LowMediaTone	An indicator that equals one for firms with a composite sentiment score of (*CSS*) below the sample median, and zero otherwise. *CSS* is measured based on full articles and news flashes published within a 5-day window centered on the proxy filing date for firm i. These articles, tracked by RavenPack via its web edition on the WRDS platform, must have a relevance score of 90 or higher. The sentiment score ranges from 0 (most negative) to 100 (most positive), with 50 indicating a neutral tone.	RavenPack
State & Industry-level Analyses		
J2J_Rate	The quarterly job-to-job change rate at the state (industry) level.	U.S. Census Bureau
Firm_Pct	The proportion of Russell 3000 firms that disclose median employee pay, calculated as the number of firms that disclose median employee pay divided by the total number of firms, per state (industry) i during quarter t.	Equilar
CumFirm_Pct	The cumulative proportion of Russell 3000 firms disclosing median employee pay, calculated as the cumulative number of firms that have disclosed median employee pay divided by the total number of firms, per state (industry) i as of quarter t.	Equilar

Appendix B
Testing for Parallel Trends Assumption

This table reports estimation results for the following model:

$$EmpDepartures_{i,t} = \beta_1 Treat\_Dec_i \times Time_{i,t} + \beta_2 Time_{i,t} + \sum \beta_n Controls_{it} + \psi_t + \phi_i + \varepsilon_{it}$$

EmpDepartures is the percentage of employees departing firm *i* during month t. *Treat_Dec* is an indicator that equals one for firms with a December 31 fiscal year-end, and zero otherwise. *Time* is a count variable ranging from 1 to 6 corresponding to each month in the pre-period relative to the proxy filing month (e.g., *Time* takes a value of 1 (6) for the month six (one) months prior to the filing month). Appendix A provides detailed variable definitions along with data sources; *t-statistics*, computed using standard errors clustered by firm, are in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% levels (two-tailed test), respectively.

	(1)
	EmpDepartures
Treat_Dec×Time	0.013
	(0.613)
Controls	Yes
Firm FE	Yes
Year-month FE	Yes
N	5,351
Adjusted-R2	0.53

Note: *Time* is omitted from the regression as it is collinear with the fixed effects.