Do Repurchases Facilitate Shareholder Exit?

Evidence from CEO Pay Ratio Disclosure

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Abstract

We examine whether managers influence dissenting shareholders' choice between voice and exit by facilitating exit through share repurchases. We exploit the mandatory disclosure of CEO-to-median employee pay ratio, which exposed inequality and prompted socially conscious investors to decide between activism or divestment. We then leverage two sources of variation in this disclosure's impact: staggered initial disclosure timing and pay ratio relative to peers. In the months after initial disclosures, high-pay-ratio firms increase repurchases 34% more than peer low-pay-ratio firms, which promotes the exit of socially conscious short-term investors. Furthermore, firms that repurchase after disclosing high pay ratios neither substantially adjust pay ratios nor face more future proposals related to CEO compensation or from socially conscious investors. Overall, our results suggest that firms use repurchases to facilitate shareholder exit, thus promoting manager-shareholder alignment and obviating the need to revise contentious corporate policies. Our findings cast doubts upon the effectiveness of the mandate in promoting pay equity within firms.

Keywords: Share repurchases; CEO pay ratio; Disclosure; Pay inequality; Socially responsible investing

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1 Introduction

When shareholders disagree with managers, they can either intervene directly in corporate decisions and use their voting power ("voice"), or they can sell their shares and exit the firm ("exit") (Hirschman, 1970). While shareholder voice often leads to corporate reform like changes to board composition or increased monitoring, shareholder exit allows managers to retain and consolidate control. Thus, all else equal, managers likely prefer that dissenting shareholders exit rather than voice their disagreement. The primary downside to shareholder exit, however, is that these sales can potentially depress stock prices (Admati and Pfleiderer, 2009; Edmans, 2009). Declining stock prices negatively impact managers directly through drops in stock-based compensation but also indirectly through increased risks of litigation and dismissal (Donelson and Hopkins, 2016; Jenter and Lewellen, 2021).

This study examines whether managers exert influence over dissenting shareholders' choice between voice and exit by facilitating exit through share repurchases. In addition to concentrating ownership among investors more accepting of current corporate practices (Huang and Thakor, 2013), share repurchases potentially mitigate the main drawback of shareholder exit by offsetting selling pressure. Repurchases generate demand for company stock, which provides liquidity and stabilizes prices (e.g., Brav, Graham, Harvey, and Michaely, 2005; Busch and Obernberger, 2017; De Cesari, Espenlaub, and Khurshed, 2011; Dudley and Manakyan, 2011; Hillert, Maug, and Obernberger, 2016; Hong, Wang, and Yu, 2008). In sum, share repurchases encourage dissatisfied shareholders to exit while shielding managers against the bothersome lawsuits or career concerns associated with price declines.

Even so, share repurchases are not without substantial costs, nor are they the only option to address shareholder disagreement. To finance repurchases, a firm must exhaust cash reserves

or debt capacity, which impacts its capital structure and risk profile (Bonaimé, Öztekin, and Warr, 2014). Furthermore, repurchases may come at the expense of more lucrative investments (Almeida, Fos, and Kronlund, 2016; Wang, Yin, and Yu, 2021). These concerns are especially pronounced for financially constrained firms, but even unconstrained firms may worry that buying out dissenting shareholders could generate negative press. In lieu of repurchasing, firms could proactively engage with shareholders in an effort to address their concerns. This approach, however, requires managerial effort with no guarantee of shareholder retention. Finally, firms may choose to acquiesce to investor demands. While adjusting corporate policies may help dissuade shareholders from selling their stock, such changes could risk managerial disagreement and lead to loss of talent. Overall, firms must determine whether the net benefits to repurchasing outweigh those of the alternatives of inaction or appeasing shareholders.

To pinpoint whether firms use repurchases to facilitate exit, we exploit a change in mandatory corporate disclosure that brought to light a polarizing issue about which many managers and shareholders often disagree: pay inequality. Fueled by corporate pay practices (e.g., Song, Price, Guvenen, Bloom, and Von Wachter, 2019), pay inequality has been rising in the U.S. for decades (e.g., Piketty and Saez, 2003). Institutional investors are known to engage with firms on issues related to excessive compensation (Ertimur, Ferri, and Muslu, 2011; McCahery, Sautner, and Starks, 2016), and advocates for equal pay have long called for legislative action. In 2015, as part of the Dodd-Frank Act, the U.S. Securities and Exchange Commission (SEC) mandated that public companies disclose the ratio of the chief executive officer (CEO) pay to that of the median employee in their 2017 annual proxy statements (SEC, 2015). The mandate triggered unprecedented public engagement in SEC rulemaking and generated extensive debate even before it took effect (Bank and Georgiev, 2019).[1,2] Although companies were already

[1]See Bank and Georgiev (2019) for details on the institutional background for the pay ratio regulation.

[2]There has also been growing momentum behind legislative efforts to narrow compensation divides. For

required to disclose their executive compensation prior to the mandate, the pay ratio itself and the median employee's pay were new information under the CEO pay ratio rule, rendering pay inequality more salient to investors. Indeed, investors reacted negatively to high pay ratios following initial disclosures (e.g., Chang, Dambra, Schonberger, and Suk, 2023; Pan, Pikulina, Siegel, and Wang, 2022).

If managers exhibit a preference for shareholder exit over voice, then firms disclosing high pay ratios may cope with the backlash from inequality-averse shareholders by buying back shares on the open market. We therefore predict that high-pay-inequality firms will increase share repurchases after they are required to disclose pay ratios. Repurchasing shares benefits shareholders (both those who leave and those who remain) through price stabilization and reduces concerns over future conflicts between managers and shareholders. By providing dissatisfied shareholders with a less costly opportunity to tender shares and leave, managers sidestep investor disputes on compensation issues—without instituting changes in corporate policies. Alternatively, firms may choose to ignore disapproving shareholders, which may prompt them to voice their discontent more loudly, or to respond to shareholder disapproval of pay inequality by altering compensation.

Using hand-collected monthly data on open market repurchase transactions from quarterly filings submitted to the SEC by U.S. companies, we test whether firms increase repurchases to buy out dissenting shareholders after the disclosure of pay ratios. This detailed dataset enables us to more precisely measure actual repurchase activities and the dynamics surrounding disclosure events.[3] We focus on the first reporting year, when public scrutiny and investor attention

example, in 2016, the City of Portland, Oregon, implemented the first tax penalty targeting corporations in which CEO pay exceeds 100 times the compensation of the median worker. Similarly, Connecticut's legislature proposed in HB 6747 to disqualify companies with similar pay ratio levels from accessing state subsidies and grants.

[3]See Banyi, Dyl, and Kahle (2008) for an evaluation of the reliability of using proxies from Compustat or CRSP for repurchases.

are likely to be most elevated, to capture the immediate corporate response. A key challenge in studying the pay ratio mandate lies in isolating its effects from other confounding factors. Since the regulation applies to the entire market, identifying counterfactuals is difficult. To address this concern, we leverage our monthly data and the variations in the staggered timing of firms' initial pay ratio disclosures on a monthly basis. Given that firms typically maintain consistent disclosure schedules, disclosure month is the only variable that distinguishes treated (already-disclosed) and control (not-yet-disclosed) groups. This approach also helps mitigate concerns over substantial differences in firm characteristics between the two groups.

Econometric studies highlight that heterogeneous treatments in staggered settings may introduce estimation biases (e.g., Baker, Larcker, and Wang, 2022). To this end, we focus on a short study window, five months before and four months after the disclosure dates, and use a stacked regression as our main specification to mitigate these concerns.[4] Motivated by the intuition that the treatment effects vary across firms with different pay ratio levels (e.g., Boone, Starkweather, and White, 2024; Norman, Rose, Rose, and Suh, 2021), we exploit another source of variation in the model: the relative level of the disclosed ratio compared to peers. We employ this triple-difference framework to identify the effects of the shock.

We find a statistically significant increase in repurchases following a firm's disclosure of a high pay ratio, even after accounting for common firm-level control variables. The impact is also economically meaningful: In our baseline model, an average firm with a high pay ratio increases repurchase value, relative to market values, by about 34% compared to a firm in the same industry with a low pay ratio. Extending the analysis to a dynamic setting reveals no differences in the trends between treated and control groups before disclosure. The treatment effects on repurchases persist in the disclosure month and the two following months, though

[4]In untabulated results, we show that our baseline results are similar if we imposed a study window of three months before to two months after the disclosures.

the effects become stronger over time within the three-month period.

Our identification strategy allows us to rule out most alternative explanations for our results. For example, repurchases might typically follow the release of proxy statements. The relation between repurchases and proxy filing dates might be especially pronounced if vesting periods coincide with proxy filings since CEOs may repurchase stock in an attempt to sell their shares at a premium (Edmans, Fang, and Huang, 2022). Yet, a relation between repurchases and the timing of proxy filings does not explain why firms repurchase more after proxy filings containing pay ratios than after other proxy filings filed prior to the disclosure mandate, or why firms with high pay ratios repurchase more than those with low pay ratios after proxy filings. Nevertheless, to further rule out the possibility that unobservable factors drive both high pay ratios and repurchases after proxy filings, we conduct placebo tests replicating our empirical strategy for disclosure events in 2017, one year before the mandate. These tests yield no significant differences across treated and control groups; we thus point to the unique impact of the 2018 disclosure. We also perform a battery of robustness tests to address econometric concerns and rule out the possibility that our results are confounded by the contemporaneous Tax Cuts and Jobs Act (TCJA). Lastly, we find little evidence of changes in other major financial decisions, including debt, dividends, cash holdings, or investments.

Furthermore, we offer direct evidence of changes in the investor base. We focus on short-term investors since long-term investors are more reluctant to trade their shares on the back of transient price volatility following disclosure (Derrien, Kecskés, and Thesmar, 2013; Yan and Zhang, 2009). Specifically, short-term investors who are sensitive to social issues and averse to pay inequality are more likely to react to high pay ratios (Pan et al., 2022). While capturing investor preferences is practically challenging, we infer their strategies based on past holdings. Among firms disclosing high pay ratios, we observe a significant reduction in the

holdings of short-term investors with high pre-disclosure KLD social scores in the quarters following disclosure. Such decreases are consistent with socially conscious investors exiting high-pay-ratio firms. We note that these changes do not occur within firms disclosing low pay ratios. In contrast, neither short-term investors in general nor short-term investors sensitive to environmental or governance factors display similar exit behaviors. Taken as a whole, these findings indicate that many short-term, socially conscious investors sell off share in response to high pay ratios.

Our results demonstrate that the group of shareholders least likely to tolerate pay inequality tends to exit when firms disclose high CEO-to-worker pay ratios. As a result, it may become unnecessary for managers to reduce pay ratios since misalignment between managers and the remaining shareholders on pay inequality may decrease as a result of this shift in investor base. We examine whether repurchasing firms that disclose high pay ratios reduce pay inequality in the future and whether they receive more shareholder proposals, the "voice" mechanism typically used by activist shareholders to raise governance concerns (e.g., Ertimur et al., 2011; McCahery et al., 2016). Firms that repurchase after disclosing high pay ratios do not significantly change their pay ratios the following year and face fewer proposals related to CEO compensation or from socially responsible investment (SRI) funds at the next annual meeting. These results are consistent with repurchases serving as an effective tool to quell dissenting shareholders and avoid changing problematic corporate policies.

A related question is whether managers repurchase around high pay ratio disclosures to *stabilize* prices, which may benefit all shareholders, or rather to *manipulate* prices, which may benefit themselves at the expense of long-term shareholders. Our cross-sectional tests evaluate this question. We construct buy-and-hold abnormal returns (BHARs) starting from the disclosure dates across several post-disclosure periods and show that the previously observed

baseline finding is most prominent in firms with relatively lower BHARs. Firms facing more negative market assessments could engage in repurchases to support prices and front run possible waves of shareholder exits. Indeed, our results suggest that post-disclosure repurchases are not insensitive to stock price. We also examine whether entrenched CEOs or those with performance-linked compensation are more likely to drive our results, but we find no significant evidence. There is also little indication of other commonly used manipulative practices; our tests reveal no significant post-disclosure changes in accruals management or earnings surprises in firms with high pay ratios. Overall, our findings suggest that managers primarily use repurchases not to manipulate short-term firm performance but instead to counter shareholder exits by stabilizing prices, which aligns with the arguments of Hong et al. (2008) and Busch and Obernberger (2017) that repurchases enhance price efficiency during downturns.

Our study provides several key contributions to the literature. While much prior work seeks to understand the investor's decision between voice and exit (e.g., Admati and Pfleiderer, 2009; Broccardo, Hart, and Zingales, 2022; Edmans, 2009; McCahery et al., 2016), our study adds to this literature by focusing on how this decision is influenced by the firm. We show that firms increase repurchases when investors express disagreement and, in turn, they rid themselves of the investors who disagree the most with current corporate practices. These results offer a novel explanation for an apparent puzzle in the literature on SRI. That is, despite the recent substantial growth in SRI (e.g., Heath, Macciocchi, Michaely, and Ringgenberg, 2023), and the SEC's belief that pay ratio disclosure is relevant to shareholders (SEC, 2015), empirical studies show inconclusive results on its actual effects. On average, post-disclosure CEO compensation levels do not significantly change (Alam, Ghosh, Ryan, and Wang, 2025; Chang et al., 2023), and pay ratio distributions remain sticky over time (Boone et al., 2024). Our study extends this literature by showing that firms use repurchases to facilitate shareholder

exits, thus increasing manager-shareholder alignment and obviating the need to revise corporate compensation policies. In this way, our findings provide empirical support for the theory proposed by Huang and Thakor (2013), which highlights the role of repurchases in improving agreement between managers and shareholders.

Our study also intersects with recent research highlighting that SRI funds select firms based on existing sustainable performance—but do not actively drive it (Heath et al., 2023). Broccardo et al. (2022) notes that excluding firms that miss SRI targets from portfolios may be an ineffective means to achieve sustainability goals. We provide empirical evidence consistent with this view: When shareholders who prioritize employee welfare issues move away from firms with large pay ratios, they lose their ability to use "voice" to influence management.

We also contribute to the ongoing debate pertaining to the recent surge in repurchases in the U.S. (e.g., Kahle and Stulz, 2021). While some evidence points to managers myopically using repurchases to temporarily boost stock prices at the expense of long-term benefits (Almeida et al., 2016; Edmans et al., 2022), our findings are more consistent with managers repurchasing to stabilize prices and protect shareholder value (Busch and Obernberger, 2017; De Cesari et al., 2011). In this regard, our findings align with those of Bourveau, Li, Macciocchi, and Sun (2023), who find that firms repurchase shares to counter regulation-induced undervaluation.

Finally, our study can inform policymakers. We show that, following the disclosure of pay ratios, firms strategically increase repurchases to concentrate ownership among shareholders who are less concerned about pay inequality. This outcome results in a "win-win" situation for managers and shareholders but essentially circumvents regulators' intentions, namely, narrowing pay divides. In sum, our results raise doubts about the effectiveness of promoting within-firm equality through a disclosure mandate.

2 Research Design

2.1 Identification

Pay ratio disclosures became mandatory in 2018. We focus on the one-year timeframe around this first reporting year, when the effects of disclosure are likely most pronounced. Using a difference-in-differences (DiD) framework similar to Carter, LaViers, Sandvik, and Xu (2023) and Chang et al. (2023), we exploit multiple sources of variation in the impact of pay ratio disclosures to construct a triple-differences framework. First, we exploit time series variation within each firm by comparing the pre-disclosure and post-disclosure periods. This comparison nets out time invariant firm characteristics. Second, using monthly repurchase data, we exploit the staggered initial disclosure of pay ratios. This setting allows us to compare repurchase activity of firms that have disclosed pay ratios with repurchases by firms that have not yet disclosed pay ratios within the same month. This second dimension accounts for macroeconomic factors that could influence repurchases for all firms at the same time.[5] Finally, we expect firms with relatively high pay ratios to experience larger treatment effects. Evidence indicates that firms with higher reported ratios behave differently from those with lower ratios (e.g., Boone et al., 2024); in fact, firms with low ratios face minimal regulatory costs, as they experience little change in investor attention or voting outcomes (Knust and Oesch, 2020).[6] Introducing pay ratios into the model strengthens identification by minimizing concerns about omitted variables driving differences in disclosure month since the variation

[5] One concern is that, to some extent, managers decide when to release the proxy statement, but we find that delayed disclosures are rare. While firms can flexibly complete their disclosure within the SEC's regulated deadline (120 days after the fiscal year-end), the majority of disclosures tend to cluster within a consistent timeframe and could be anticipated. Among the 1,122 sample firms in 2018, 71 postponed their disclosures by one calendar month compared to 2017. On closer inspection, only 14 firms deferred their disclosure by more than 30 days.

[6] Knust and Oesch (2020) report that pay ratios for firms with public floats just above the regulatory threshold are generally modest, averaging 35. Many of these firms voluntarily continue to report the ratios even if later exempted, suggesting that disclosing a small ratio does not impose significant costs on them.

we capture is across high and low pay ratio firms but within groups of firms disclosing in the same month.

A common concern in staggered settings is that treated firms may contaminate the control group (e.g., Baker et al., 2022). This issue requires particular attention in our design since disclosure is mandatory and universal. That is, all firms in the sample eventually complete disclosures, leaving no never-treated firms to serve as clean controls. To address this issue, we follow Baker et al. (2022) and adopt a stacked regression as our main specification. Specifically, we first set a stringent window from 5 months prior to a firm's initial disclosure month to 4 months after the disclosure (a total of 10 months surrounding and comprising the disclosure). Next, we build our cohorts by matching (with replacement) firms with disclosures in the same year-month to not-yet-treated firms (i.e., firms that have not yet disclosed their pay ratios) in their pre-disclosure periods. This procedure guarantees that only not-yet-treated observations function as the control group. We control for firm and time fixed effects specific to each cohort.

To assess the effects of disclosure, we create an indicator variable that equals one if a firm's pay ratio exceeds its industry's median pay ratio. Focusing on the sharp distinction between high and low pay ratios mitigates concerns about measurement errors due to managerial discretion in handling reported values (Alam et al., 2025) and relaxes the assumption that pay ratios affect corporate policies monotonically. Further, industry benchmark comparisons account for industry variation in pay practices and the common practice of benchmarking pay against peers from similar product markets (Norman et al., 2021; Pan et al., 2022). Throughout our baseline tests, we opt for a broad industry classification, the Fama French 12 industries, as managers are less likely to manipulate the specific ranking of their pay ratio relative to a very broad peer group, making the relative level presumably exogenous. The results remain robust and comparable when using different thresholds, as detailed later.

We estimate the following specification:

$$Repo_{i,m,c} = \alpha + \beta \cdot Post\ Disclosure_{i,m,c} \cdot High\ Ratio + \theta \cdot Post\ Disclosure_{i,m,c} + \gamma \cdot \mathbf{X} + \mu_{i,c} + \rho_{m,c} + \varepsilon_{i,m,c} \quad (1)$$

where i, m, and c denote firm, month, and cohort, respectively. The dependent variable is measured as the monthly repurchase value over the market capitalization of the end of the last quarter, following Dittmar and Field (2015) and Edmans et al. (2022). The variable of interest is the product of *Post Disclosure* and *High Ratio*. *Post Disclosure* is a binary variable that equals one if a firm has published its first-time pay ratio in the current or previous months, and zero otherwise. *High Ratio* equals one if the firm's pay ratio exceeds its industry median pay ratio and is absorbed by the firm fixed effects in each cohort, $\mu_{i,c}$. Calendar year-month fixed effects in each cohort are denoted as $\rho_{m,c}$.

We include a set of standard control variables following previous studies (e.g., Andriosopoulos, De Cesari, and Stathopoulos, 2021), including *Ln(Assets)*, *Mkt-to-Book*, *Cash*, *ROA*, *Leverage*, *Dividend Payer*, and *Return*, all lagged to mitigate reverse causality. Except for *Return*, which is measured as the monthly stock return, all other variables are measured quarterly due to data availability. Moreover, to account for corporate calendars effects, we follow Dittmann, Li, Obernberger, and Zheng (2023) and include the proportion of blackout days in a given month, denoted as *Blackout*, when managers are temporarily prohibited from engaging in insider trading, typically during sensitive periods such as days around earnings announcements. Controlling for the blackout period is important in our monthly-frequency specification because the initial disclosure date typically falls within this period (for example, see Figure 1). Detailed variable definitions are provided in the Table A1. Robust standard errors are clustered at the firm level.

2.2 Data

We begin by searching SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system for all proxy statements filed since January 1, 2017, one year before the mandated disclosure year 2018. Because our interest lies in initial mandatory disclosures, we drop from our sample firms that voluntarily disclose their CEO pay ratios before the mandate. We keep the U.S.-headquartered Russell 3000 companies whose common stocks are listed on NYSE, NASDAQ, or AMEX. We condition on firms with available pay ratios and positive total assets but exclude firms in the financial and utility industries. We restrict the sample to firms with first-time reported ratios greater than one, as CEOs in firms with ratios below this threshold are likely adopting zero or marginal compensation policies that are not comparable. The first disclosures in our sample were reported in February 2018, and we include all first-ever disclosures until January 2019. In our setting, the sample period differs from the event period: The 2018 regression sample extends from five months before the first pay ratio disclosure (September 2017) to four months after the last disclosure (May 2019). For placebo tests, we create a separate dataset with a 2017 placebo event period spanning from February 2017 to January 2018 and a sample period covering September 2016 to May 2018.

We hand collect monthly open market repurchase data, including the number of shares repurchased and the average price paid each month, from 10-K or 10-Q filings on EDGAR. We further cross-reference the aggregated quarterly amounts with values reported in Compustat and manually verify any significant discrepancies. Monthly data enable us to concentrate on a short window around proxy statement disclosures and better understand the dynamics of buyback transactions.

We merge the sample with other financial information, primarily from Compustat and

CRSP. All continuous variables are winsorized at the 1st and 99th percentiles. Our final disclosure sample comprises 1,122 firms. Table A2 summarizes the distribution of disclosure year-months. The distribution aligns with that of Carter et al. (2023), who report that 77% of a total of 1,362 firms (including financial and utility firms) had filed their disclosures by April 2018. In comparison, our placebo event sample includes 1,085 firms.

Table 1 reports descriptive statistics for the main variables used in the baseline models for the two disclosure (main and placebo) events. In our sample, the mean (median) CEO-to-median-employee pay ratios equals 181 (88) in 2018 and varies widely, with a standard deviation of 339, similar to that reported in Carter et al. (2023) and Pan et al. (2022). On average, firms repurchase 0.1% of their market value each month. The distributions of the variables are similar across the two panels and are comparable to those in recent studies. Because the stacked regression framework requires the construction of control groups by drawing not-yet-treated firms with replacement, the size of regression samples is larger than the count of unique observations reported here.

We collect information about shareholders and their ownership data from the Thomson/Refinitiv Institutional (13F) Holdings database. KLD ESG statistics are gathered from the MSCI database, and shareholder proposal data from the ISS ESG. To match the available frequency of the outcome variables, we adjust the frequency of all other variables to be monthly, quarterly, or annual in different specifications, which results in varying numbers of observations.

3 Main results

3.1 Baseline results

Table 2 contains estimates for the baseline model of repurchases presented in Eq. (1). Model (1) shows that high-pay-ratio firms repurchase significantly more than low-pay-ratio firms after disclosing pay ratios. The coefficient on the interaction of *Post Disclosure* and *High Ratio* is positive and significant at the 1% level. In contrast, the coefficient on *Post Disclosure* alone does not achieve statistical significance, suggesting that disclosure itself does not impact repurchase. Rather, repurchases increase only after firms disclose *high* pay ratios. The observed effects persist after controlling for time-varying firm characteristics in Model (2).

Our findings are consistent with a reaction to pay ratio disclosure that is economically meaningful. On average, following pay ratio disclosures, firms with above-median pay inequality increase monthly repurchases 0.04% more than firms with below-median pay inequality. As a percentage of the mean monthly repurchases in our regression sample, this estimate translates to a substantial increase of 34%. This magnitude is comparable to the magnitude of other significant shocks to repurchases documented in prior studies. For example, Dang, De Cesari, and Phan (2021) estimate that repurchases increase 26% in states that pass wrongful discharge laws. The effects of other variables align with previous studies; firms with lower growth opportunities, more cash, lower previous returns, and fewer blackout days repurchase more (e.g., Andriosopoulos et al., 2021; Dittmann et al., 2023).

Models (3) and (4) replicate our regressions using 2017 as a placebo year. Although companies are not required to disclose pay ratios in 2017, we assume their ratios were consistent with those reported in 2018. Thus, we assign the same *High Ratio* values across both years. If repurchases are simply a regular response to proxy statement disclosures and such a re-

sponse differs between high- and low-pay-ratio firms for reasons unrelated to pay inequality, then we would expect similar results in 2017. However, high-pay-ratio firms do not repurchase significantly more than low-pay-ratio firms after 2017 disclosures, suggesting that firms with high pay inequality do not systematically increase repurchases more than firms with low pay inequality after proxy disclosures. The differential response is unique to 2018.

3.2 Dynamic effects

Table 3 extends the baseline analysis by investigating the repurchase dynamics over our interval surrounding the initial pay ratio disclosure. To this end, we replace the independent variable *Post Disclosure* with five indicator variables denoting the disclosure month and bifurcating the time periods preceding and following the disclosure month. These variables are set to one: (i) three or four months prior to the disclosure ($Post\ Disclosure^{-3,-4}$); (ii) one or two months prior to the disclosure ($Post\ Disclosure^{-2,-1}$); (iii) the month of the disclosure ($Post\ Disclosure^{0}$); (iv) one or two months after the disclosure ($Post\ Disclosure^{+1,+2}$); and (v) three or four months after the disclosure ($Post\ Disclosure\ ^{+3,+4}$). To avoid multicollinearity, we omit the indicator for the fifth month before the disclosure, using this month as a reference.

Model (1) of Table 3 shows no evidence of significant differences in repurchase activity across high-pay-ratio and low-pay-ratio firms during the four months leading up to the disclosure relative to five months prior. These results are consistent with parallel pre-trends across our treatment and control samples. Beginning the month of the disclosure, however, high-pay-ratio firms repurchase more than low-pay ratio firms. The repurchase gap widens during the two months after disclosures, then tapers off by three months after disclosure. The slight delay in the intensity of firms' response may be due to the constraining effects of the blackout period during which firms are restricted from repurchasing shares. Results are similar when we control

for firm characteristics in Model (2). These findings suggest that firms respond sharply and in a timely manner to the disclosure of high pay ratios.

We also repeat this analysis for the 2017 placebo disclosure period in Models (3) and (4). We find no significant differences across high- and low-pay-ratio firms' repurchases during any time period. Overall, the changes in repurchases do not appear driven by other firm characteristics correlated with the pay ratios that were already present in 2017.

3.3 Robustness tests

We carry out a series of tests to evaluate the robustness of our inferences. Panel A of Table 4 evaluates the robustness of our repurchase metric, which equals repurchases scaled by market capitalization. One potential criticism is that market prices can be volatile and may be influenced by the disclosure event (Andriosopoulos et al., 2021), especially for firms with high ratios. A drop in a firm's stock price possibly inflates our main proxy, making the results appear related to repurchases when they may not be. Thus, we use several alternative measures of repurchases. To begin, instead of scaling repurchases by time-varying market capitalization, we fix the denominator to equal market capitalization one month prior to the disclosure event. We also scale repurchases by quarterly total assets or sales revenue to generate alternative repurchase outcome variables. In addition, we follow Dittmann et al. (2023) and calculate repurchase intensity, defined as the number of shares repurchased in the open market during the month, divided by the number of shares outstanding at the end of the previous quarter. Lastly, we construct a dummy variable indicating whether a firm repurchases in a given month. Our results are robust across these alternative repurchase proxies.

Panel B of Table 4 examines the consistency of our results by using different benchmarks to determine *High Ratio*. We first consider two narrower industry classifications: Fama French 30

and SIC 3-digits. We also construct a proxy that reflects staggered comparisons. For instance, if a firm discloses its pay ratio in April, we compare it not only to firms disclosing in the same month but also to those that disclosed it in February or March. This approach results in an uneven distribution of the dummy variable values, but it recognizes the possibility that investors' perceptions of high pay ratios develop gradually. Next, similar to Pan et al. (2022), we alternatively classify firms with pay ratios above an absolute level of 100 as high ratio firms. Finally, we compare disclosed ratios to those of other firms headquartered in the same state. Our results are not sensitive to alternative definitions of *High Ratio*.

In Panel C we include additional fixed effects to alleviate concerns about other sources of omitted variables that relate to repurchases. Specifically, we consider fixed effects to capture the time-varying heterogeneity across industries, states of incorporation, firm age, and fiscal months. Our results are robust to the inclusion of these additional fixed effects individually and simultaneously.

To conclude our robustness tests, Panel D confirms that our finding are not driven by the concurrence of pay ratio disclosure with the Tax Cuts and Jobs Act (TCJA), which led to historically significant changes in U.S. tax law. Previous studies have established that the TCJA resulted in tax windfalls through repatriation and income tax cuts, which contributed to a surge in repurchases (Albuquerque, Bennett, Lisowsky, and Wang, 2024; Kahle and Stulz, 2021). We note that the TCJA was applied nationwide and became effective at the start of 2018 for all the firms, whereas pay ratio disclosures are staggered over time depending on the disclosure month of proxy statements. Even so, we formally address potential confounding factors related to tax policy in our results. Specifically, we start by focusing on two subsamples less likely to be affected by the TCJA. First, since the TCJA reduced the corporate income tax rate from 35% to 21%, a firm with an effective tax rate lower than 21% before the TCJA

cannot further benefit from the tax cut. We therefore restrict our sample to the subsample of firms with an effective tax rate ($ETR = \frac{\text{Income tax paid}}{\text{Pre-tax income}-\text{Special items}}$) already below 21% in 2017, the year before the TCJA was implemented. Second, because the TCJA's repatriation tax cut benefits multinational firms with foreign cash savings to repatriate, we examine the subsample of firms with no foreign pre-tax income before the TCJA. Additionally, the TCJA affects both federal and state-level taxes. While some states align their tax regulations with federal standards, others do not. These variations in state-level tax regulations may also influence repurchases. As a final test, we, therefore, include state-year-month fixed effects in the model. The incorporation of these high-dimensional fixed effects accounts for changes in state-level tax regulations over time. Our results are robust to restricting our sample to firms whose repurchases are less likely to be caused by the TCJA and to controlling for state-time fixed effects. These finding suggest that the introduction of the TCJA is unlikely to drive our findings.

3.4 Other financial decisions

So far, we have shown that firms with high pay ratios repurchase more than other firms following the pay ratio disclosure. In this section, we examine the impact of disclosing a high pay ratio on a range of other major financial decisions, including debt financing, dividend payouts, cash holdings, and investment decisions such as capital expenditures, acquisitions, and fixed asset investment. If firms increase repurchases to facilitate shareholder exit, then we do not expect firms to rapidly change other corporate policies post-disclosure. Alternatively, if omitted variables related to firm fundamentals drive our findings, then we may also observe significant changes in other corporate policies.

Since the variables used in the following tests are only available at the quarterly level, we

adjust the frequency of the variables in Eq. (1) as follows:

$$Y_{i,q,c} = \alpha + \beta \cdot Post\ Disclosure_{i,q,c} \cdot High\ Ratio + \theta \cdot Post\ Disclosure_{i,q,c} + \gamma \cdot \mathbf{Z} + \mu_{i,c} + \rho_{q,c} + \varepsilon_{i,q,c} \tag{2}$$

where i, q, and c denote firm, quarter, and cohort, respectively. We redefine *Post Disclosure* at the quarterly level; it equals one in all quarters during and after the pay ratio disclosure, and zero otherwise. Consistent with the baseline specification, we set a fixed window around the disclosure, extending it to two quarters pre-disclosure and two quarters post-disclosure, and then estimate the model using a stacked regression.

Table 5 does not reveal any significant differences in other corporate policies across high-pay-ratio and low-pay-ratio firms following pay ratio disclosure. These findings are in line with our expectations and provide credence to our hypothesis that firms use repurchases to facilite shareholder exit. If omitted variables related to pay ratios drive our observed differences in repurchase activity, then we would likely also observe differences in other firm policies, especially other shareholder payouts like dividends. Yet, we find that the results are concentrated within share repurchases alone.

4 Investors' responses to the pay ratio mandate

4.1 Investor exit

Mandatory pay ratio disclosure places CEOs and firms with high reported pay inequality under public scrutiny. Investors' reactions to high pay ratios will depend on their time horizons and preferences, however. Trading strategies differ systematically due to investment horizons (e.g., McCahery et al., 2016; Yan and Zhang, 2009). While long-term shareholders adjust their

portfolios less frequently and are less willing to trade in response to transient shocks, short-term investors actively buy and sell to attempt to exploit informational advantages (Yan and Zhang, 2009). Further, share repurchases appeal to short-term investors (Derrien et al., 2013; Gaspar, Massa, Matos, Patgiri, and Rehman, 2013), for example, by boosting stock prices ahead of their sales. Therefore, we consider the effects of the pay ratio disclosure on short-term investor holdings. Furthermore, investors who prioritize social equality and employee welfare may reduce their holdings in companies with high ratios, either because they view high pay inequality as a predictor of underperformance or on ethical grounds.

We examine whether firms disclosing high pay ratios experience a shift in investor base by analyzing changes in the trading patterns of institutional investors. We obtain institutional investors' portfolio holdings at the end of each quarter from the Thomson/Refinitiv Institutional (13F) Holdings database. To identify short-term investors, we follow Yan and Zhang (2009) to calculate churn rate. Specifically, we first exclude observations with aggregated institutional ownership for each firm-year-quarter exceeding 100%. Next, we calculate the aggregate purchases and sales for each institution each quarter and then use the minimum of these values to compute the churn rate for a given institutional investor. Lastly, we average each institution's churn rate over the past four quarters. We classify institutional investors in the top quintile of 2017Q4 average churn rate as short-term (Starks, Venkat, and Zhu, 2023).[7]

We infer investors' preferences by combining their portfolio holdings with KLD ESG scores for each company in 2017. KLD evaluates a firm's ESG profile based on a set of strengths and concerns, with each category receiving a score of either 0 or 1. The net ESG score is the sum of relevant strengths ratings minus the sum of the relevant concerns ratings. In addition

[7]Investors in the top quintile of the churn rate distribution are classified as short-term investors and have an average churn rate of 21.14%. Several well-known short-term investors are identified in this category, such as George Soros (Soros Fund Management) and Paul Tudor Jones (Tudor Investment Corporation). The average churn rates for quintiles 1 through 4 are 1.24%, 2.89%, 4.49%, and 7.36%, respectively.

to building an overall KLD ESG score, we follow Ng and Rezaee (2015) to calculate separate net scores for each of the three dimensions of ESG (environmental, social, and governance). Environmental scores consider environmental issues; social sustainability scores consider community, diversity, human rights, and employee relations; and governance scores account for product quality and corporate governance. Similar to Pan et al. (2022), we measure an investor's appetite for each ESG aspect as the value-weighted average of the net KLD score across all the stocks the investor holds, where weights correspond to the market values of the holdings in the investor's portfolio at the end of 2017.[8]

Next, we construct ESG profiles at the *firm* level. Specifically, we calculate the weighted average investor preference score for all short-term investors in the firm, where weights are determined by the institutional investors' holdings. Since investors' ESG preferences are held constant at historical 2017 levels, changes in the firms' ESG profiles are driven by shifts in investor holdings over time. We end up with the following firm-level ESG profiles: *Social Holdings* for social issues, *Environmental Holdings* for environmental issues, *Governance Holdings* for corporate governance, and the combined variable *ESG Holdings*.

Panel A in Table 6 focuses on holdings of short-term investors that are sensitive to social issues. We estimate Eq. (2) using *Social Holdings* as the dependent variable. The coefficient of interest is on the interaction term *Post Disclosure* $\times$ *High Ratio*. Its estimates are negative and statistically significant. Results from Model (1) imply that after the disclosure, our measure of holdings by socially conscious investors drops 0.83% more around disclosure in high-pay-ratio firms than low-pay-ratio firms. This difference is approximately 7% of the standard deviation of the dependent variable. The magnitude of the difference increases slightly to 0.89% in Model (2) when we control for firm characteristics. We interpret these findings as evidence

[8]Panels A and B of Table A3 present summary statistics on KLD ESG scores and investor-level preferences, respectively.

that investors sensitive to social issues exit firms when they disclose high pay inequality.[9]

It is possible that socially oriented investors are responding not to pay inequality but to information on worker pay. For example, some investors may be averse to particularly low wages for workers. In our sample, the mean (median) value of median worker pay used in the ratio calculation is $73,390 ($58,588). Like Boone et al. (2024), we construct two additional variables *Low Worker Pay (Med)* and *Low Worker Pay (Q1)*, which respectively indicate whether the disclosed worker pay falls in the bottom half or bottom quartile within a particular industry. We interact these two terms with *Post Disclosure* and include them in Models (3) and (4). Our results remain robust, suggesting that investors are reacting specifically to the disclosed pay ratio.

To investigate if the above result is merely part of a broad post-disclosure decrease in short-term holdings among firms with high pay ratios, in Model (1) of Panel B we consider a dependent variable that reflects the proportion of shares held by short-term investors relative to the total shares held by all institutional investors. We do not observe a significant change in short-term holdings relative to all institutional investor holdings after the disclosure across high-pay-ratio and low-pay-ratio firms. Additionally, in Models (2), (3), and (4), we examine holdings of investors who are sensitive to environmental, governance, and all ESG issues, respectively. Although we do not find significant differences in post-disclosure changes in holdings of environmental and governance conscious investors across high-pay-ratio and low-pay-ratio firms, we do see a differential drop in the holding of investors mindful of ESG in general. These results indicate that investors attentive to ESG concerns sell off some of their holdings in high-pay-ratio stocks but that these sells off are driven by socially conscious

[9]In untabulated results, we further examine employee welfare issues specifically, rather than broader social issues, and find similar results. This suggests that shareholders who prioritize employee welfare are likely reacting to the high pay ratios.

investors. These findings help to mitigate the concern that ESG-motivated investors respond to other information included in the disclosure.

4.2 Alignment between investors and managers

Table 7 analyzes post-disclosure changes in pay ratio and in shareholder proposals. We argue that, following the disclosure of high pay ratios, managers use share repurchases to smooth adverse price fluctuations and facilitate the exit of shareholders who disapprove of pay inequality. Once dissenting shareholders sell their shares, their voting rights cease and they are no longer able to affect future corporate decisions. We predict that the remaining investors will thus be better aligned with the managers and note that improved manager-investor alignment may eliminate the need for change in controversial compensation policies altogether.

Panel A examines changes in pay ratio as a function of pay ratio level and changes in firm characteristics. We assume that changes in reported figures reflect deliberate managerial actions, either efforts to lower actual pay inequality or adjustments in estimation methods (Alam et al., 2025). Similar to Cheng and Zhang (2024), we estimate models in which the dependent variable is the relative change in a firm's CEO pay ratio from 2018 to 2019 and the variable of interest is an indicator for high-pay-ratio firms. We collect CEO pay ratio data from the 2019 proxy statements of firms used in our main event sample. The mean (median) pay ratio disclosed in 2019 is 174 (90), similar to the 2018 value. We denote firms as *High Ratio* if their pay ratio falls above the median, in the top tercile, or in the top quartile, as noted, compared to industry peers. We estimate models separately for sub-samples of firms that repurchase during the post-disclosure period (from disclosure month t to $t+4$) and firms that do not repurchase during this period. High-pay-ratio firms that engage in repurchases may face less pressure to adjust their ratios while high-pay-ratio firms that do not repurchase

shares may still face pressure from investors to reduce pay inequality. All control variables are measured as annual changes.

We find that high-pay-ratio firms that do not repurchase after the initial pay ratio disclosure tend to significantly decrease pay ratios between initial disclosures in 2018 and subsequent disclosures in 2019 while repurchasers tend to maintain the status quo. Although repurchasing firms with above-median pay ratios slightly decrease pay ratios (by approximately 0.18, significant at the 10% level), repurchasing firms with top-tercile or top-quartile pay ratios do not significantly alter pay ratios. These findings stand in stark contrast to the intense response of non-repurchasing firms with high pay ratios; they significantly decrease pay ratios by 0.36% to 0.41%. Our findings suggest that, in non-repurchasing firms, investors dissatisfied with high pay ratios are less likely to exit and may pressure managers to make adjustments. In repurchasing firms, however, managers and the remaining investors appear more aligned on pay inequality issues, precluding the need for change.

Panel B of Table 7 examines changes in shareholder proposals. Shareholders can express their concerns to managers by submitting proxy proposals, one example of "voice" (Ertimur et al., 2011; McCahery et al., 2016). Building on the work of Huang and Thakor (2013) and Huang, Maharjan, and Thakor (2020), we exploit the idea that investors are more likely to be in opposition to management when they submit proxy proposals than when they do not. Since shareholder proposals are submitted before the publication of the proxy statement, any effects of repurchases following the 2018 proxy statement would not become apparent until the subsequent year (2019). Therefore, we focus on changes in shareholder proposals between 2017 and 2019. Specifically, the dependent variable in our models is an indicator variable that equals one if a firm receives one or more proxy proposals in 2017 but none in 2019, representing an improvement in alignment (Huang and Thakor, 2013). We also segment proposals by

type. Although the ISS ESG database classifies proxy proposals into governance- and social responsibility-related proposals, we further differentiate proposals based on their specific focus and sponsor. Model (1) focuses on changes in pay-related proposals pertaining to all CEO compensation matters within governance-related proposals, and Model (2) examines proposals sponsored by SRI funds (SRI funds can submit both governance- and social responsibility-related proposals).[10] As placebo tests, Model (3) studies governance proposals other than those related to CEO pay, and Model (4) explores SRI proposals submitted by other stakeholders, such as unions, public pension funds, or religious groups. As before, we identity firms as *Repurchasers* if they repurchase stock within the 5-month window following the disclosure and as *High Ratio* firms if their pay ratio exceeds the median.

We find that high-pay-ratio firms that repurchase experience drops in shareholder proposals both related to CEO compensation and from SRI funds, indicating an increase in manager-investor alignment. We also find an unconditional marginal decrease in SRI-sponsored proposals for high-pay-ratio firms, indicating that the SRI funds decrease engagement with those firms in general. The drop is significantly greater for repurchasing firms, however. We do not find significant drops in governance-related proposals that are unrelated to CEO pay or on changes in SRI proposals initiated from other stakeholders who are less likely to be affected by corporate repurchases. These results complement and reinforce our findings from Section 4.1 that investors most averse to pay inequality exit repurchasing firms. Furthermore, repurchases made after the disclosure of high pay ratios appear to shield managers from future confrontations with shareholders.

[10]We caution that the SRI funds identified here may not align precisely with the investors defined as sensitive to KLD ESG scores in Section 4.1. Due to the absence of standardized methods for identifying SRI funds, we are unable to directly match the list of funds as provided in the ISS database.

5 Price support or price manipulation?

5.1 Effects of price pressure

Why would firms disclosing a high CEO pay ratio increase repurchase volumes post-disclosure? One explanation is that repurchases offset the negative stock price impact of such disclosure. In particular, reporting high pay inequality may lead to negative reactions from inequality-averse shareholders (Chang et al., 2023; Pan et al., 2022). If enough investors sell their shares following the disclosure, stock prices may fall. Shareholder exit likely takes time, however, due to frictions such as transaction costs and price impact considerations, which render immediate and simultaneous portfolio adjustment unwise. Consequently, ongoing price pressure following high pay ratio disclosures might motivate managers to counteract short-term downward price revisions using share repurchases to stabilize prices (Busch and Obernberger, 2017; De Cesari et al., 2011).

To investigate if price stabilization motivates firms to repurchase following the disclosure of high pay ratios, we calculate Buy-and-Hold Abnormal Returns (BHAR) following the proxy statement disclosure over 21, 32, or 42 trading days.[11] For each BHAR measure, we bifurcate our sample into subsamples based on whether the firm's abnormal return is below or above the median abnormal return of industry peers in the same disclosure month. We assume that firms with low BHARs face the most price pressure from selling investors. We estimate our baseline repurchase regression by subsample. The coefficient of interest again corresponds to the interaction term *Post Disclosure* $\times$ *High Ratio*; we expect larger coefficients in the low abnormal returns subsamples. Since repurchases during disclosure months may occur prior to the disclosure, we exclude firm-month observations corresponding to disclosing firms

[11] BHARs over 21, 32, and 42 days average -0.77%, -1.37% and -2.00%, respectively.

in the disclosure month. Further, to account for future repurchase decisions being potentially confounded by the contemporaneous stock price movements, we also exclude observations from the first month following disclosure when dividing the sample by *BHAR[0,31]* or *BHAR[0,41]*.[12]

For the most part, results in Panel A of Table 8 are in line with our expectations. For firms with below-median abnormal returns following the disclosure of pay ratios, firms disclosing high pay inequality repurchase significantly more than firms disclosing low equality. In contrast, for firms with high post-disclosure abnormal returns, we observe no significant difference across low- and high-pay-ratio firms. These findings hold for all three BHAR measures, and the coefficients on *Post Disclosure* × *High Ratio* across low returns and high returns subsamples are statistically significant at the 5% level for the 31-day measure and at the 10% level for the 41-day measure. These results suggest that firms use repurchases to counter the effects of disclosing high pay ratios when price support appears to be a concern.

Panel B of Table 8 models the extensive margin: the decision whether or not to repurchase. Negative price pressure may affect not only the magnitude but also the likelihood of repurchases. Consistent with our previous findings, the difference between low- and high-ratio firms' repurchases is larger in economic magnitude for low-abnormal-returns firms than high-abnormal-returns firms in all cases and statistically different for two of the three measures. These results suggest that firms are more likely to repurchase following high-pay-ratio disclosure if returns are abnormally low and price support therefore more necessary.

[12]Since firms may not disclose pay ratios at the start of the month, we alternatively exclude months t and $t+1$ when splitting the sample based on BHAR[0,20], and exclude months from t to $t+2$ when using *BHAR[0,31]* or *BHAR[0,41]*. In general, we find that the results remain largely significant in the subsample with low BHARs.

5.2 Do managers repurchase stock to manipulate short-term performance?

The previous section suggests that firms use repurchases to stabilize prices and counteract transient price pressure following the disclosure of high CEO pay ratios. One interpretation of these results is that managers are acting in the best interest of all shareholders—both selling and remaining—when they stabilize prices using repurchase. An alternative interpretation is that self-interested managers repurchase opportunistically to manipulate short-term firm performance. If so, we expect opportunistic behavior to be more pronounced among CEOs who are entrenched or whose compensation is more closely tied to firm performance. Prior evidence indicates that entrenched CEOs are more likely to engage in poorly-timed buybacks (Bonaimé, Hankins, and Jordan, 2016). In our context, powerful CEOs may repurchase at inflated stock prices to facilitate the exit of disgruntled shareholders, reducing the risk of shareholder activism or proxy fights stimulated by pay ratio controversies. Managers who hold large amounts of stock or other equity-related incentives may also be more likely to inflate short-term performance metrics like earnings per share (EPS) through repurchases (e.g., Hribar, Jenkins, and Johnson, 2006), particularly on the back of a controversial pay ratio disclosure.

As an illustrative example, Figure 1 describes the corporate timeline of a firm with a December fiscal year end that publishes its 2018 proxy statement shortly before an earnings announcement. If the release of the firm's proxy statement is greeted negatively by the market and executive equity awards vest shortly after such release, the firm's managers may have incentives to safeguard their personal wealth by supporting stock market valuations through buybacks (Edmans et al., 2022). Buybacks also provide the added benefit of boosting reported

EPS (Almeida et al., 2016; Hribar et al., 2006), reducing the likelihood of further disappointing investors when earnings are subsequently announced.

Using sample splits, we formally evaluate the alternative interpretation of our baseline findings that CEOs use repurchases to manipulate short-term performance. First, we build a measure of CEO entrenchment to test whether the relation between high-pay-ratio disclosure and repurchases is stronger for more entrenched CEOs. We argue that managers are more entrenched in states with stronger antitakeover provisions (ATPs), because they are less susceptible to external pressures of hostile takeovers and shareholder engagement is less effective. Following Guernsey, Sepe, and Serfling (2022), we consider five major plausibly exogenous state-level ATPs: business combination, control share acquisitions, directors' duties, fair price, and poison pill laws. We construct an ATP index, ranging from zero to five, based on a firm's state of incorporation. We categorize firms with an above-median 2017 Q4 ATP index (i.e., greater than one) as having high entrenchment. Second, we examine variation in compensation structure to capture managerial incentives (Fenn and Liang, 2001). If repurchases following pay ratio disclosure are driven by a CEO's sensitivity to changes in stock price levels or volatility, we expect differences between CEOs with high and low compensation delta and vega. We construct these variables using data from Equilar, supplemented with ExecuComp data when missing. We categorize firms with CEOs whose delta (vega) is above the 2017 sample median as having high managerial incentives.

Panel A of Table 9 presents estimations of our repurchase model across sample splits based on managerial entrenchment and incentives. The positive and significant relation between high-pay-ratio disclosure and repurchase holds within all subsamples. Importantly, we find no significant difference in the magnitude of this relation across subsamples based on entrenchment or compensation incentives. Repurchase models in Panel B replace the continuous dependent

repurchase variable with an indicator variable that captures repurchase likelihood and show similar results. The lack of cross-sectional differences indicates that neither CEO entrenchment nor CEO equity-related incentives drive the strong positive relation between high-pay-ratio disclosures and repurchases.

We continue our investigation of possible manipulation in Panel C in Table 9 by examining whether managers in high-pay-ratio firms inflate accounting performance after the pay ratio disclosure. Opportunistic managers may bolster EPS either by using repurchases to reduce the count of shares outstanding (the denominator) and/or by adjusting accounting and accruals policies (e.g., deferring expenses or accelerating revenue recognition) to inflate reported earnings (the numerator). Models (1) and (2) estimate discretionary accruals using two proxies: *DACC_MJ* is the absolute value of the error terms from regressions of the modified Jones model in each quarter (Dechow, Sloan, and Sweeney, 1995), and *DACC_FW* is the absolute value of the error terms from regressions of the Dechow, Richardson, and Tuna's (2003) model. We do not observe a significant difference in accruals for firms that disclose high pay ratios.

Finally, Models (3) and (4) provide more direct evidence on earnings surprises by modeling binary and continuous measures of earnings surprises, respectively. After retrieving analyst forecast data from IBES, we generate *Positive Surprise*, an indicator variable equal to one if a firm meets or beats the consensus estimate of quarterly analyst EPS. Following Chiang, Dai, Fan, Hong, and Tu (2019), we also calculate *FOM*, a measure of the fraction of earnings misses, which equals $\frac{(K-M)}{N}$, where N is the total number of forecasts, $K(M)$ is the number of forecasts that are lower (higher) than the actual announced EPS. FOM thus ranges from -1 to +1, with higher values indicating more positive earnings surprises. If managers repurchase to push their firms' performance metrics above market expectations and generate positive investor reactions, we expect a higher incidence of EPS surprises within firms disclosing high

pay ratios. Instead, the coefficients on the *Post Disclosure* × *High Ratio* interactions are statistically significant but negative, rather than positive, in both models. These results are inconsistent with high-pay-ratio firms manipulating performance metrics through repurchases.

6 Conclusion

Using the Dodd-Frank Act's mandatory disclosure of CEO-to-median-employee pay ratios as our setting, we examine whether firms use share repurchases to facilitate the exit of dissenting shareholders. Our findings suggest that firms reporting high pay inequality increase share repurchases significantly more than firms reporting low pay inequality during the three months following initial disclosures. On the other side of transaction appears to be short-term institutional investors sensitive to social issues. They exit firms with high pay ratios, even though other short-horizon investors continue to hold shares in these firms. Following disclosures, high-ratio firms repurchase do not significantly alter pay ratios. Yet, they receive fewer shareholder proposals related to CEO pay or initiated by SRI funds. Taken as a whole, these findings indicate that firms disclosing potentially controversial information use share repurchases to encourage dissenting shareholders to sell their stakes. The resulting shift in shareholder base renders both corporate compensation policy changes and shareholder activism unnecessary.

Overall, our study offers a novel perspective on the role of share repurchases in corporate governance. In turn, we cast doubt on the effectiveness of mandatory disclosure in promoting social change. Rather than revising corporate policies in response to shareholder concerns, managers use repurchases to facilitate the exit of dissenting investors and influence shareholder composition, thereby increasing alignment with remaining shareholders and reducing shareholder pressure for reform.

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Figures and Tables

Figure 1: Example timeline of corporate calendar

This figure shows the timeline of the corporate calendar for a typical firm with a fiscal year ending in December. Such a firm publishes its proxy statement in April 2018. We focus on the 5 months before (pre-disclosure period, from November 2017 to March 2018) and 4 months after (post-disclosure period, from April 2018 to August 2018) the firm's proxy filing publication. "EA" refers to the earnings announcement date. The shaded areas represent the blackout periods, as defined by Dittmann et al. (2023).

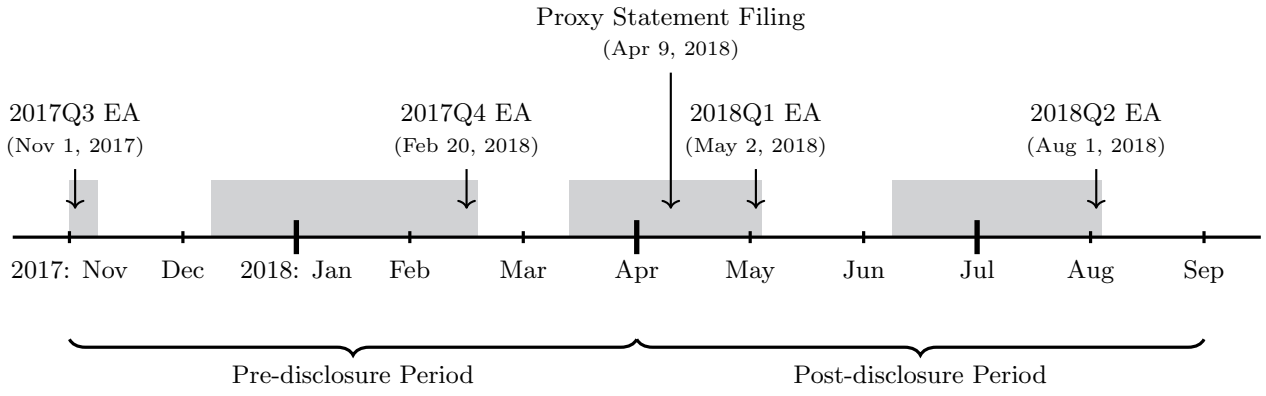


Table 1: Summary statistics

This table reports descriptive statistics for the variables used in our baseline regression model. Panel A presents summary statistics for the 2018 pay ratio variables. Panel B presents summary statistics for the main sample around 2018 disclosure events. Panel C presents summary statistics for the 2017 placebo sample. Samples span from 5 months prior to a firm's publication of proxy filings to 4 months after (a total of 10 months surrounding the event). Detailed variable definitions can be found in the Table A1.

Panel A: Pay ratios in 2018

	Obs.	Mean	Lower Quartile	Median	Upper Quartile	Standard Deviation
Pay Ratio	1,122	180.7896	42	87.85	173	339.4885
High Ratio	1,122	0.4973	0	0	1	0.5002
High Ratio (Ter3)	1,122	0.3289	0	0	1	0.4700
High Ratio (Quar4)	1,122	0.2460	0	0	0	0.4309

Panel B: Sample for the 2018 disclosure events

	Obs.	Mean	Lower Quartile	Median	Upper Quartile	Standard Deviation
Monthly data						
Repo/MV (%)	13,072	0.1159	0	0	0.0166	0.3043
Post Disclosure	13,072	0.3996	0	0	1	0.4898
Blackout	13,072	0.6568	0.3929	0.7000	1	0.3338
Return	13,072	0.0147	-0.0415	0.0110	0.0667	0.0981
Quarterly data						
Ln(Assets)	5,501	7.5391	6.3826	7.4579	8.6070	1.6398
Mkt-to-Book	5,501	2.4984	1.3835	1.8812	2.8334	1.8193
Cash	5,501	0.1855	0.0359	0.0976	0.2528	0.2111
Dividend Payer	5,501	0.4781	0	0	1	0.4996
ROA	5,501	0.0063	-0.0004	0.0119	0.0236	0.0378
Leverage	5,501	0.2729	0.0325	0.1521	0.3460	0.3658

Panel C: Sample for the 2017 disclosure events

	Obs.	Mean	Lower Quartile	Median	Upper Quartile	Standard Deviation
Monthly data						
Repo/MV (%)	12,673	0.1049	0	0	0	0.2799
Post Disclosure	12,673	0.3961	0	0	1	0.4891
Blackout	12,673	0.6583	0.4000	0.7000	1	0.3357
Return	12,673	0.0150	-0.0391	0.0101	0.0623	0.0970
Quarterly data						
Ln(Assets)	5,326	7.3818	6.1542	7.2809	8.4198	1.6388
Mkt-to-Book	5,326	2.3703	1.3778	1.8285	2.6937	1.6065
Cash	5,326	0.1930	0.0394	0.1096	0.2626	0.2137
Dividend Payer	5,326	0.4640	0	0	1	0.4987
ROA	5,326	0.0033	-0.0005	0.0105	0.0211	0.0398
Leverage	5,326	0.2716	0.0283	0.1492	0.3440	0.3647

Table 2: Do high-pay-ratio firms repurchase more than low-pay-ratio firms following disclosure?

This table reports estimates on triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. Models (1) and (2) examine the actual initial pay ratio disclosure events in 2018 and span from 5 months prior to a firm's publication of proxy filings to 4 months after (a total of 10 months surrounding the event). Models (3) and (4) present placebo tests around proxy filings in 2017. *Post Disclosure* is a binary variable set to one if a firm has disclosed its pay ratio in the current or previous months. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. Detailed variable definitions can be found in the Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped and matched (with replacement) to samples before their own disclosures (not-yet-treated sample). All models include firm-cohort and year-month-cohort fixed effects. The standard errors in parentheses are clustered at the firm level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Dependent variable: Repo/MV (%)			
	2018 Disclosure		2017 Disclosure (Placebo)	
	(1)	(2)	(3)	(4)
Post Disclosure × High Ratio	0.0327***	0.0401***	0.0161	0.0190
	(0.012)	(0.012)	(0.012)	(0.012)
Post Disclosure	0.0073	-0.0051	-0.0111	-0.0183
	(0.014)	(0.014)	(0.013)	(0.013)
Ln(Assets)		0.0247		0.0749**
		(0.035)		(0.030)
Mkt-to-Book		-0.0130*		-0.0098**
		(0.007)		(0.005)
Cash		0.3091***		0.3835***
		(0.081)		(0.099)
Dividend Payer		0.0273		0.0114
		(0.022)		(0.014)
ROA		0.0319		-0.0027
		(0.155)		(0.137)
Leverage		-0.0203		-0.0794**
		(0.039)		(0.034)
Return		-0.0847***		-0.1014***
		(0.028)		(0.027)
Blackout		-0.1330***		-0.0943***
		(0.020)		(0.016)
Observations	30,348	30,348	29,727	29,727
R-squared	0.409	0.421	0.415	0.425
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes

Table 3: Dynamic effects

This table reports dynamic effects from a triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. Models (1) and (2) examine the actual initial pay ratio disclosure events in 2018 and span from 5 months prior to a firm's publication of proxy filings to 4 months after (a total of 10 months surrounding the event). Models (3) and (4) present placebo tests around proxy filings in 2017. *Post Disclosure* is replaced with a set of binary variables that identify periods preceding and following a disclosure event. These variables are set to one: (i) three or four months prior to the disclosure (*Post Disclosure*$^{-3,-4}$); (ii) one or two months prior to the disclosure (*Post Disclosure*$^{-2,-1}$); (iii) the month of the disclosure (*Post Disclosure*^{0}); (iv) one or two months after the disclosure (*Post Disclosure*$^{+1,+2}$); and (v) three or four months after the disclosure (*Post Disclosure* $^{-+3,+4}$). To avoid multicollinearity, we omit the indicator for the fifth month before the disclosure, using this month as a reference. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. Detailed variable definitions can be found in the Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped and matched (with replacement) to samples before their own disclosures (not-yet-treated sample). All models include firm-cohort and year-month-cohort fixed effects. The standard errors in parentheses are clustered at the firm level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Dependent variable: Repo/MV (%)			
	2018 Disclosure		2017 Disclosure	
	(1)	(2)	(3)	(4)
Post Disclosure$^{-3,-4}$ × High Ratio	0.0001	-0.0049	0.0146	0.0100
	(0.019)	(0.018)	(0.018)	(0.018)
Post Disclosure$^{-1,-2}$ × High Ratio	0.0278	0.0176	-0.0000	-0.0094
	(0.019)	(0.019)	(0.020)	(0.019)
Post Disclosure^{0} × High Ratio	0.0456*	0.0453*	0.0205	0.0178
	(0.024)	(0.024)	(0.022)	(0.022)
Post Disclosure$^{+1,+2}$ × High Ratio	0.0618***	0.0622***	0.0165	0.0129
	(0.021)	(0.021)	(0.020)	(0.020)
Post Disclosure$^{+3,+4}$ × High Ratio	0.0257	0.0284	0.0272	0.0247
	(0.020)	(0.020)	(0.020)	(0.020)
Post Disclosure$^{-3,-4}$	0.0005	0.0184	-0.0010	0.0110
	(0.015)	(0.015)	(0.015)	(0.015)
Post Disclosure$^{-1,-2}$	-0.0190	0.0010	-0.0097	0.0045
	(0.016)	(0.016)	(0.018)	(0.018)
Post Disclosure^{0}	-0.0051	0.0160	-0.0118	0.0021
	(0.019)	(0.019)	(0.019)	(0.019)
Post Disclosure$^{+1,+2}$	0.0028	-0.0099	-0.0010	-0.0070
	(0.020)	(0.020)	(0.019)	(0.019)
Post Disclosure$^{+3,+4}$	-0.0036	0.0001	-0.0400	-0.0355
	(0.024)	(0.024)	(0.025)	(0.024)
Observations	30,348	30,348	29,727	29,727
R-squared	0.409	0.422	0.416	0.425
Controls from Table 2	No	Yes	No	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes

Table 4: Robustness tests

This table examines the robustness of the results in Table 2, which estimate on triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. All models examine the actual initial pay ratio disclosure events in 2018 and span from 5 months prior to a firm's publication of proxy filings to 4 months after (a total of 10 months surrounding the event). In Panel A, the dependent variables reflect different measurements of actual share repurchases. In Panel B, *High Ratio* reflects different ways to compare a firm's disclosed pay ratio to its peers. All models include firm-cohort and year-month-cohort fixed effects. In Panel C, all models re-estimate Model (2) from Table 2 using different fixed effects. In Panel D, Model (1) re-estimates Model (2) from Table 2 using subsamples of firms with a cash effective tax rate lower than 21% in 2017. Model (2) re-estimates the same model using subsamples of firms without foreign profits in 2017. Model (3) re-estimates the same model with headquarter state-month fixed effects. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped and matched (with replacement) to samples before their own disclosures (not-yet-treated sample). The standard errors in parentheses are clustered at the firm level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Alternative measures of repurchases

Dependent variable:	(1) Repo/MV Alt. (%)	(2) Repo/Asset (%)	(3) Repo/Sales (%)	(4) Repo/Shares (%)	(5) Repo Dummy
Post Disclosure × High Ratio	0.0426***	0.0516***	0.4066***	0.0399***	0.0573***
	(0.013)	(0.019)	(0.116)	(0.012)	(0.015)
Post Disclosure	-0.0013	-0.0068	0.0454	-0.0028	0.0115
	(0.016)	(0.022)	(0.127)	(0.014)	(0.019)
Observations	30,348	30,348	30,348	30,348	30,348
R-squared	0.421	0.458	0.496	0.417	0.616
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes

Table 4 (Continued)

Panel B: Alternative measures of *High Ratio*

	Dependent variable: Repo/MV (%)				
	(1) FF30 Median	(2) SIC3 Median	(3) Staggered Median	(4) Over 100	(5) State Median
Post Disclosure × High Ratio	0.0328***	0.0361***	0.0379***	0.0276**	0.0329***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)
Post Disclosure	-0.0014	-0.0052	-0.0036	0.0022	-0.0016
	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)
Observations	30,348	30,348	30,348	30,348	30,348
R-squared	0.421	0.421	0.421	0.421	0.421
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes

Panel C: Additional fixed effects

	Dependent variable: Repo/MV (%)				
	(1)	(2)	(3)	(4)	(5)
Post Disclosure × High Ratio	0.0383***	0.0389***	0.0389***	0.0397***	0.0449***
	(0.013)	(0.013)	(0.012)	(0.013)	(0.014)
Post Disclosure	-0.0111	-0.0001	-0.0032	0.0098	-0.0021
	(0.014)	(0.015)	(0.017)	(0.016)	(0.020)
Observations	30,206	29,145	30,331	30,245	28,810
R-squared	0.446	0.445	0.426	0.465	0.519
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	No	No	Yes	Yes	No
Industry-Year-Month-Cohort FE	Yes	No	No	No	Yes
Inc. State-Year-Month-Cohort FE	No	Yes	No	No	Yes
Fiscal Month-Cohort FE	No	No	Yes	No	Yes
Firm Age-Cohort FE	No	No	No	Yes	Yes

Table 4 (Continued)

Panel D: The effects of TCJA

	Dependent variable: Repo/MV (%)		
	(1)	(2)	(3)
	Pre-TCJA	Pre-TCJA	Headquarter state-time
	CETR $< 21\%$	No foreign profits	effects
Post Disclosure $\times$ High Ratio	0.0482***	0.0418**	0.0410***
	(0.018)	(0.018)	(0.013)
Post Disclosure	-0.0035	-0.0037	-0.0011
	(0.021)	(0.018)	(0.014)
Observations	15,624	12,512	29,381
R-squared	0.425	0.452	0.480
Controls from Table 2	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	No
State-Year-Month-Cohort FE	No	No	Yes

Table 5: Other firm outcomes

This table reports estimates on triple difference-in-differences regressions of quarterly firm outcomes around the mandatory disclosure of CEO-to-median-employee pay ratio. All models examine the actual initial pay ratio disclosure events in 2018 and span from 2 quarters prior to a firm's publication of proxy filings to 2 quarters after (a total of 5 months surrounding the event). *Post Disclosure* is a binary variable set to one if a firm has disclosed its pay ratio in the current or previous quarters. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. Detailed variable definitions can be found in the Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-quarter are grouped and matched (with replacement) to samples before their own disclosures (not-yet-treated sample). All models include firm-cohort and year-quarter-cohort fixed effects. The standard errors in parentheses are clustered at the firm level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Total Debt	Div Yield (%)	Cash	Capex (%)	Acquisition	Fixed Assets
Post Disclosure × High Ratio	-0.0060	0.0038	-0.0038	0.0027	-0.0012	-0.0027
	(0.005)	(0.008)	(0.003)	(0.035)	(0.002)	(0.002)
Post Disclosure	-0.0044	0.0123	0.0013	-0.0886**	0.0024	-0.0039
	(0.007)	(0.011)	(0.004)	(0.045)	(0.003)	(0.003)
Ln(Assets)	-0.2583***	-0.0398**	0.0059	-0.1260	-0.0557***	-0.0884***
	(0.022)	(0.020)	(0.013)	(0.102)	(0.008)	(0.012)
Mkt-to-Book	0.0156***	-0.0158***	0.0056**	0.0817***	-0.0011	-0.0005
	(0.006)	(0.006)	(0.002)	(0.023)	(0.001)	(0.001)
ROA	-0.0796	0.0846	0.0601	-0.7862*	0.0522***	-0.0503*
	(0.076)	(0.108)	(0.040)	(0.433)	(0.019)	(0.026)
Leverage	0.0558***	0.0916**	-0.0101	-0.4401***	-0.0477***	-0.0246**
	(0.017)	(0.037)	(0.008)	(0.121)	(0.008)	(0.010)
Stk. Return	-0.0275**	-0.0565***	0.0001	-0.1579***	-0.0125***	-0.0136***
	(0.011)	(0.014)	(0.005)	(0.056)	(0.003)	(0.004)
Observations	8,339	8,319	8,339	8,339	8,339	8,339
R-squared	0.900	0.866	0.971	0.854	0.297	0.986
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Investor exit

This table reports estimates on triple difference-in-differences regressions of quarterly investor holdings around the mandatory disclosure of CEO-to-median-employee pay ratio. All models examine the actual initial pay ratio disclosure events in 2018 and span from 2 quarters prior to a firm's publication of proxy filings to 2 quarters after (a total of 5 months surrounding the event). In Panel A, the dependent variable reflects holdings of short-term investors who exhibit preferences for socially conscious companies. In Models (3) and (4), we also augment with *Low Worker Pay (Med/Q1)*, a dummy variable that equals one if a firm has disclosed worker pay below the median level or in the bottom quartile of the same Fama French 12 industry. In Panel B, the dependent variables reflect the holdings of short-term investors who exhibit preferences for environmental, governance, or all ESG issues, as noted. *Post Disclosure* is a binary variable set to one if a firm has disclosed its pay ratio in the current or previous months. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. Detailed variable definitions can be found in the Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-quarter are grouped and matched (with replacement) to samples before their own disclosures (not-yet-treated sample). All models include firm-cohort and year-quarter-cohort fixed effects. The standard errors in parentheses are clustered at the firm level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10% respectively.

Panel A: Social-oriented investors

	Dependent variable: Social Holdings			
	(1)	(2)	(3)	(4)
Post Disclosure × High Ratio	-0.0083**	-0.0089**	-0.0089**	-0.0102**
	(0.004)	(0.004)	(0.004)	(0.004)
Post Disclosure × Low Worker Pay (Med)			-0.0002	
			(0.004)	
Post Disclosure × Low Worker Pay (Q1)				0.0043
				(0.005)
Post Disclosure	0.0062	0.0062	0.0063	0.0058
	(0.005)	(0.005)	(0.005)	(0.005)
Ln(Assets)		0.0197*	0.0197*	0.0198*
		(0.010)	(0.010)	(0.010)
Mkt-to-Book		0.0088***	0.0088***	0.0087***
		(0.002)	(0.002)	(0.002)
ROA		-0.0233	-0.0233	-0.0234
		(0.035)	(0.035)	(0.035)
Leverage		-0.0289*	-0.0289*	-0.0290*
		(0.016)	(0.016)	(0.016)
Stk. Return		-0.0043	-0.0043	-0.0042
		(0.006)	(0.006)	(0.006)
Observations	8,132	8,132	8,132	8,132
R-squared	0.869	0.870	0.870	0.870
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes

Table 6 (Continued)

Panel B: Heterogenous investors

Dependent variable:	(1) ST Holding	(2) Environmental Holdings	(3) Governance Holdings	(4) ESG Holdings
Post Disclosure × High Ratio	-0.0015	-0.0069	0.0028	-0.0139*
	(0.002)	(0.004)	(0.002)	(0.008)
Post Disclosure	-0.0005	0.0069	-0.0041	0.0116
	(0.002)	(0.005)	(0.003)	(0.008)
Observations	8,132	8,132	8,132	8,132
R-squared	0.909	0.880	0.878	0.875
Controls from Panel A	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes

Table 7: Investor-management alignment

This table reports regressions of changes in CEO pay ratios and shareholder proposals around the actual initial pay ratio disclosure events in 2018. In Panel A, the dependent variable reflects the relative changes in CEO pay ratios from 2018 to 2019. *High Ratio*, *High Ratio (T3)*, and *High Ratio (Q4)* equal one if the firm discloses a pay ratio above the median, in the top tercile, and in the top quartile, respectively, for the same Fama French 12 industry. The regression sample is split based on whether a firm repurchases in the five months after the disclosure, and we report p-values of F-statistics testing the differences across sample splits. In Panel B, the dependent variables are binary variables equal to one if a firm received one or more proxy proposals in 2017 but not in 2019. *Repurchaser* equals one if a firm has made actual repurchases in the five-month window following the disclosure. Detailed variable definitions can be found in the Table A1. All models include industry fixed effects. The standard errors in parentheses are clustered at the industry level. The coefficients on constants are not reported. ***, **, * correspond to the significance levels of 1%, 5%, and 10%, respectively.

Panel A: Changes in CEO pay ratios

	Dependent variable: Chg. in CEO Pay Ratio					
	(1)	(2)	(3)	(4)	(5)	(6)
	Repurchaser	Non-repurchaser	Repurchaser	Non-repurchaser	Repurchaser	Non-repurchaser
High Ratio	-0.1838*	-0.4129***				
	(0.090)	(0.107)				
High Ratio (T3)			-0.1103	-0.3720***		
			(0.081)	(0.098)		
High Ratio (Q4)					-0.1321	-0.3652***
					(0.076)	(0.107)
p-value (equal coefficient estimates)	(0.108)		(0.048)		(0.083)	
Chg. in Ln(Assets)	0.1081	0.0400	0.0986	0.0355	0.1083	0.0363
	(0.238)	(0.288)	(0.249)	(0.302)	(0.249)	(0.290)
Chg. in Mkt-to-Book	-0.0188	0.0630**	-0.0222	0.0562	-0.0171	0.0542
	(0.038)	(0.027)	(0.041)	(0.031)	(0.042)	(0.030)
Chg. in Leverage	0.1723*	-0.2456*	0.1754**	-0.2376*	0.1686**	-0.2398*
	(0.079)	(0.121)	(0.072)	(0.122)	(0.072)	(0.121)
Chg. in ROA	1.1483**	-0.1791	1.1158**	-0.1719	1.2252**	-0.1833
	(0.382)	(0.607)	(0.411)	(0.591)	(0.392)	(0.596)
Return	0.3427**	-0.2549	0.3555**	-0.2638	0.3422**	-0.2586
	(0.109)	(0.156)	(0.116)	(0.167)	(0.113)	(0.159)
Observations	480	598	480	598	480	598
R-squared	0.065	0.031	0.055	0.024	0.057	0.021
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7 (Continued)

Panel B: Shareholder proposals

Dependent variable:	(1) Drop in Pay Related Topics	(2) Drop in SRI Fund Initiators	(3) Drop in GOV Other Topics	(4) Drop in SRI Other Initiators
High Ratio × Repurchaser	0.0164**	0.0295**	0.0288	0.0425
	(0.007)	(0.011)	(0.048)	(0.025)
High Ratio	0.0006	0.0197*	0.0028	0.0129
	(0.001)	(0.009)	(0.020)	(0.011)
Repurchaser	-0.0004	-0.0088	0.0247	0.0115
	(0.001)	(0.008)	(0.036)	(0.015)
Observations	1,103	1,103	1,103	1,103
R-squared	0.029	0.029	0.021	0.039
Controls from Panel A	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Table 8: Cross-sectional analyses: Price pressure

This table reports estimates on triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio across sample splits based on stock returns. All models examine the actual initial pay ratio disclosure events in 2018 and span from 5 months prior to a firm's publication of proxy filings to 4 months after (a total of 10 months surrounding the event). The sample is split according to the buy-and-hold return (BHAR) across different horizons (21, 32, or 42 days) following the date of pay ratio disclosure. A firm is classified into the low returns group if its BHAR is lower than the median value of all other industry peers in the same disclosure month. We exclude the treated firm's observations in the first (or first two) disclosure months from the sample when splitting based on BHAR [0,20] (BHAR [0,31] or BHAR [0,41]). This table also reports the p-values of F-statistics testing the differences in coefficients on *Post Disclosure* × *High Ratio* between the respective partitioned subsamples. *Post Disclosure* is a binary variable set to one if a firm has disclosed its pay ratio in the current or previous months. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. Detailed variable definitions can be found in the Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped and matched (with replacement) to samples before their own disclosures (not-yet-treated sample). All models include firm-cohort and year-month-cohort fixed effects. The standard errors in parentheses are clustered at the firm level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Repurchase magnitude

	Dependent variable: Repo/MV (%)					
	BHAR [0,20]		BHAR [0,31]		BHAR [0,41]	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disclosure × High Ratio	0.0558***	0.0250	0.0738***	0.0084	0.0628***	0.0129
	(0.020)	(0.017)	(0.021)	(0.018)	(0.023)	(0.018)
p-value (equal coefficient estimates)	(0.242)		(0.020)		(0.082)	
Post Disclosure	0.0011	-0.0305	-0.0236	-0.0008	-0.0032	-0.0153
	(0.021)	(0.025)	(0.030)	(0.026)	(0.026)	(0.027)
Observations	13,046	15,955	12,555	15,411	12,591	15,375
R-squared	0.410	0.431	0.430	0.422	0.461	0.397
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 8 (Continued)

Panel B: Repurchase likelihood

	Dependent variable: Repo Dummy					
	BHAR [0,20]		BHAR [0,31]		BHAR [0,41]	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disclosure × High Ratio	0.0636**	0.0574***	0.0973***	0.0326	0.0992***	0.0295
	(0.025)	(0.022)	(0.027)	(0.023)	(0.026)	(0.023)
p-value (equal coefficient estimates)	(0.853)		(0.067)		(0.046)	
Post Disclosure	0.0136	-0.0024	-0.0336	0.0459	-0.0035	0.0293
	(0.038)	(0.030)	(0.044)	(0.034)	(0.039)	(0.036)
Observations	13,046	15,955	12,555	15,411	12,591	15,375
R-squared	0.615	0.616	0.626	0.610	0.638	0.599
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: Managerial incentives and manipulation

This table reports estimates on triple difference-in-differences regressions of monthly repurchases and quarterly earnings surprises around the mandatory disclosure of CEO-to-median-employee pay ratio. Models in Panels A and B examine the actual initial pay ratio disclosure events in 2018 and span from 5 months prior to a firm's publication of proxy filings to 4 months after (a total of 10 months surrounding the event). The sample is split according to the state-level antitakeover provisions (ATP) index, CEO pay delta, or CEO pay vega. A firm is classified in the high state ATP index group if its incorporation state has an index greater than one as of 2017Q4. A firm is classified in the high CEO Pay Delta/Vega group if the CEO's delta (vega) is greater than the sample median in 2017. This table also reports the p-values of F-stats testing the differences in coefficients on *Post Disclosure* × *High Ratio* between the respective partitioned subsamples. Models in Panel C examine the actual initial pay ratio disclosure events in 2018 and span from 2 quarters prior to a firm's publication of proxy filings to 2 quarters after (a total of 5 months surrounding the event). *Post Disclosure* is a binary variable set to one if a firm has disclosed its pay ratio in the current or previous months. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. Detailed variable definitions can be found in the Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped and matched (with replacement) to samples before their own disclosures (not-yet-treated sample). All models include firm-cohort and year-month-cohort fixed effects. The standard errors in parentheses are clustered at the firm level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Repurchase magnitude

	Dependent variable: Repo/MV (%)					
	State ATP Index		CEO Pay Delta		CEO Pay Vega	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disclosure × High Ratio	0.0480**	0.0375**	0.0503***	0.0413*	0.0414**	0.0461**
	(0.021)	(0.015)	(0.017)	(0.021)	(0.017)	(0.020)
p-value (equal coefficient estimates)	(0.683)		(0.744)		(0.859)	
Post Disclosure	-0.0124	0.0010	-0.0149	-0.0061	-0.0102	-0.0067
	(0.023)	(0.018)	(0.025)	(0.021)	(0.023)	(0.021)
Observations	9,276	21,049	12,896	13,952	13,315	13,904
R-squared	0.426	0.423	0.435	0.405	0.440	0.403
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 9 (Continued)

Panel B: Repurchase likelihood

	Dependent variable: Repo Dummy					
	State ATP Index		CEO Pay Delta		CEO Pay Vega	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Post Disclosure × High Ratio	0.0574*	0.0528***	0.0402*	0.0802***	0.0520**	0.0747***
	(0.031)	(0.018)	(0.024)	(0.025)	(0.022)	(0.025)
p-value (equal coefficient estimates)	(0.899)		(0.247)		(0.497)	
Post Disclosure	-0.0053	0.0255	0.0237	0.0192	0.0191	0.0067
	(0.033)	(0.023)	(0.037)	(0.024)	(0.031)	(0.028)
Observations	9,276	21,049	12,896	13,952	13,315	13,904
R-squared	0.612	0.619	0.613	0.606	0.638	0.586
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Earnings surprises

	(1)	(2)	(3)	(4)
Dependent variable:	DACC_MJ	DACC_FW	Positive Surprise	FOM
Post Disclosure × High Ratio	-0.0010	-0.0022	-0.0441*	-0.0980**
	(0.001)	(0.002)	(0.025)	(0.042)
Post Disclosure	0.0027	0.0020	0.0348	0.1132**
	(0.002)	(0.002)	(0.033)	(0.056)
Ln(Assets)	-0.0118**	-0.0044	-0.0463	-0.0574
	(0.005)	(0.005)	(0.059)	(0.095)
Mkt-to-Book	0.0029**	0.0023	-0.0307**	-0.0413**
	(0.001)	(0.002)	(0.015)	(0.021)
ROA	-0.0185	-0.0455**	-0.4245	-0.7616
	(0.022)	(0.023)	(0.308)	(0.510)
Leverage	-0.0086	-0.0072	0.0664	0.0786
	(0.005)	(0.005)	(0.075)	(0.119)
Stk. Return	-0.0035	-0.0005	0.0187	0.0133
	(0.003)	(0.003)	(0.041)	(0.071)
Observations	8,182	8,177	7,418	7,316
R-squared	0.467	0.488	0.448	0.426
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes

Appendix

Table A1: Variable definitions and measurement

Variable	Definition
Monthly data	
Rep/MV	Average price paid for the shares repurchased times the number of shares repurchased divided by the market capitalization of the previous quarter. (*Source*: 10-K/10-Q filings, Compustat)
Post Disclosure (m)	A binary variable that is set to one if a firm that has disclosed its pay ratio over the current or previous months.
Blackout	Fraction of blackout days within a month. The blackout period is from 20 days before the end of a firm's fiscal quarter (apdedateq) until 3 days after the following earnings announcement (rdq), following Dittmann et al. (2023). (*Source*: Compustat)
Return	Monthly period stock return. (*Source*: CRSP)
Quarterly data	
Post Disclosure (q)	A binary variable that is set to one if a firm that has disclosed its pay ratio over the current or previous quarters.
Ln(Assets)	Natural logarithm of total assets (atq). (*Source*: Compustat)
Mkt-to-Book	Total assets (atq) minus book common equity (ceqq) plus market capitalization (prccq $\times$ cshoq), scaled by total assets. (*Source*: Compustat)
Cash	Cash and short-term investments (cheq) over total assets (atq). (*Source*: Compustat)
Dividend Payer	Dummy that equals one if the firm has paid dividends in a quarter, zero otherwise. (*Source*: Compustat)
ROA	Income before extraordinary items (ibq) over total assets (atq). (*Source*: Compustat)
Leverage	Long-term debt (dlttq) plus long-term debt due in one year (dd1q) over market capitalization. (*Source*: Compustat)
Return	Quarterly period stock return. (*Source*: CRSP)
Annual data	
Chg. in Ln(Assets)	The difference between *Ln (Asset)* at year t and $t-1$. (*Source*: Compustat)
Chg. in Mkt-to-Book	The difference between *Mkt-to-Book* at year t and $t-1$. (*Source*: Compustat)
Chg. in ROA	The difference between *ROA* at year t and $t-1$. (*Source*: Compustat)
Chg. in Leverage	The difference between *Leverage* at year t and $t-1$. (*Source*: Compustat)
Return	Annually period stock return. (*Source*: CRSP)

Table A2: Disclosure year-months of the initial pay ratio

This table presents the timing distribution of event year-months (including the frequency, percentage, and cumulative percentage) for the initial pay ratio disclosures of 1,122 firms in the main sample, spanning from February 2018 to January 2019.

	Frequency	Percentage	Cumulative Percentage
February 2018	3	0.27	0.27
March 2018	375	33.42	33.69
April 2018	500	44.56	78.25
May 2018	40	3.57	81.82
June 2018	40	3.57	85.38
July 2018	29	2.58	87.97
August 2018	30	2.67	90.64
September 2018	26	2.32	92.96
October 2018	22	1.96	94.92
November 2018	11	0.98	95.90
December 2018	19	1.69	97.59
January 2019	27	2.41	100.00

Table A3: ESG Scores, Investor Preferences, and Holdings

This table presents summary statistics for KLD ESG score (Panel A), static investor-level ESG preferences from 2017 (Panel B), and firm-level, time-varying investor preferences (Panel C).

Panel A: KLD ESG Scores

	Mean	Min	p25	p50	p75	Max	SD
KLD Social score	0.50	-2	0	0	1	7	1.17
KLD Environmental score	0.67	-2	0	0	1	5	1.03
KLD Governance score	0.11	-5	0	0	0	3	0.71
KLD ESG score	1.21	-3	0	1	2	11	1.85

Panel B: Investor preferences

	N	Mean	p25	p50	p75	SD
Social Preference	509	0.477	0.192	0.452	0.708	0.365
Environmental Preference	509	0.545	0.229	0.503	0.817	0.382
Governance Preference	509	-0.136	-0.257	-0.096	0.000	0.249
ESG Preference	509	0.955	0.385	0.884	1.420	0.674

Panel C: Firm-level, time-varying investor holdings

	N	Mean	p25	p50	p75	SD
Social Holdings	6,019	0.660	0.608	0.677	0.734	0.117
Environmental Holdings	6,019	0.761	0.698	0.776	0.850	0.131
Governance Holdings	6,019	-0.169	-0.219	-0.170	-0.119	0.074
ESG Holdings	6,019	1.341	1.243	1.373	1.485	0.223