

Do Repurchases Facilitate Shareholder Exit?

Evidence from CEO Pay Ratio Disclosure*

Alice Bonaimé[†] Amedeo De Cesari[‡] Yue Feng[§] Konstantinos Stathopoulos[¶]

This draft: September 2026

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 polarized shareholders by exposing 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 intensively after disclosing high pay ratios neither substantially adjust pay ratios nor face more future proposals related to CEO compensation, and they do not suffer worse performance over a longer horizon. 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 doubt upon the effectiveness of the mandate in promoting pay equity within firms.

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

*We thank Jie Chen; Lora Dimitrova; Dušan Isakov; conference participants at the Annual Corporate Finance Conference, Financial Management Association European Conference, Frontiers in Finance Conference (Alberta), and Massey Sustainable Finance Conference; and seminar participants at Florida International University, University of Essex, University of Science and Technology of China, and University of South Carolina for their helpful comments.

[†]Eller College of Management, University of Arizona; alicebonaime@arizona.edu.

[‡]Alliance Manchester Business School, University of Manchester; amedeo.decesari@manchester.ac.uk.

[§]Faculty of Business for Science and Technology, School of Management, University of Science and Technology of China; yuefeng@ustc.edu.cn.

[¶]Alliance Manchester Business School, University of Manchester; K.Stathopoulos@manchester.ac.uk.

1 Introduction

When shareholders disagree with managers, they can 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, all else equal. The primary downside to shareholder exit is that these sales can 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). Absent price drops, however, managers likely prefer that dissenting shareholders exit rather than voice their disagreement.

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 enable future shareholder exit because they 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; 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 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 (Babenko, Bennett, and Wang, 2025). 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 managers and shareholders often disagree: pay inequality (e.g., Mueller, Ouimet, and Simintzi, 2017; Pan, Pikulina, Siegel, and Wang, 2022). Fueled by corporate pay practices, pay inequality has been rising in the U.S. for decades (Piketty and Saez, 2003; Song, Price, Guvenen, Bloom, and Von Wachter, 2019). 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 (Bank and Georgiev, 2019).^{1,2} Although companies were already required to disclose their executive compensation prior to the mandate, the pay ratio

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

²Momentum behind legislative efforts to narrow compensation divides has also been growing. For 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.

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 (Chang, Dambra, Schonberger, and Suk, 2023; Pan et al., 2022), and used pay ratio information as a screening criterion to divest from certain companies.³

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. Shareholder exit is not always immediate; it may take time due to market frictions and incentives to avoid realizing losses. We therefore predict that high-pay-inequality firms will increase share repurchases shortly 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 timely and 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 SEC filings, we test whether firms increase repurchases to facilitate the exit of 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.⁴ We focus on the first reporting year, when public scrutiny and investor attention are likely to be most elevated, to capture the immediate corporate response. A key challenge in studying the pay ratio mandate lies in

³For example, see [the Triodos Investment Management interview discussing their criticism of excessive pay ratios](#) and Danske Bank’s use of pay ratio metrics in their [screening model](#). Since 2024, MSCI has incorporated this data into [the methodology used to construct their Inclusive Growth Select Indexes](#).

⁴See Banyl, Dyl, and Kahle (2008) for an evaluation of the reliability of using proxies from Compustat or CRSP for repurchases.

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 to distinguish between treated (already-disclosed) and control (not-yet-disclosed) groups. Using this approach also helps mitigate concerns over substantial differences in firm characteristics between the two groups, especially given that firms typically maintain consistent disclosure schedules across time.

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. 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 disclosure mandate.

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. A dynamic setting reveals no differences in the trends between treated and control groups before disclosure. The treatment effects on repurchases peak in the second month following the disclosure month, immediately after the end of the nearest blackout 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 statements 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; the 2018 disclosure thus appears unique. We also perform a battery of robustness tests to address econometric concerns and rule out the possibility that our results are confounded by endogenous disclosure timing or contemporaneous Tax Cuts and Jobs Act (TCJA). Lastly, we find little evidence of changes in other forms of payout, that is, dividends.

We next examine the effects arising from the incremental repurchases induced by the pay ratio mandate. Because repurchase policies can be persistent (Almeida, Huang, and Xuan, 2026; Andriosopoulos, De Cesari, and Stathopoulos, 2021), we examine post-disclosure deviations from predicted repurchase levels estimated from pre-disclosure information. We identify a set of firms that repurchase at levels exceeding expectations and compare them with other firms that do not repurchase or repurchase below expected levels. We start by offering direct evidence of changes in the investor base. Our focus is 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 repurchasing more than expected and disclosing high pay ratios, we observe a significant reduction in the holdings of short-term investors with high pre-disclosure

KLD social scores. We find no comparable changes after disclosures from other high-ratio firms. 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 repurchases facilitate the exit of many short-term, socially conscious investors, who sell their shares quickly 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 pay ratios. As a result, misalignment between managers and the remaining shareholders on pay inequality may decrease due to this shift in investor base, and thus it may become unnecessary for managers to reduce pay ratios. We examine whether firms that repurchase more than expected and disclose high pay ratios reduce pay inequality or receive fewer shareholder proposals, the “voice” mechanism often used by activist shareholders to raise governance concerns (e.g., [Ertimur et al., 2011](#); [McCahery et al., 2016](#)). Firms with above-expected repurchases after disclosing high pay ratios do not significantly change their pay ratios the following year and face fewer proposals related to CEO compensation at the next annual meeting. These results are consistent with repurchases serving as an effective tool to quell dissenting shareholders and avoid costly changes to contentious 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 early repurchases to support prices and front run possible waves of shareholder exits. In the long run, high-pay-ratio firms that repurchase more than expected experience less underperformance relative to peers, inconsistent with price manipulation and managerial myopia.

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 increases 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 smooth future shareholder exits by stabilizing prices.

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., [Edmans and Manso, 2011](#); [McCa-hery 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 socially responsible investing (SRI). That is, despite the recent substantial growth in SRI and the SEC’s belief that pay ratio disclosure is relevant to shareholders ([SEC, 2015](#)), empirical studies show inconclusive results on the disclosure’s effects. On average, post-disclosure CEO compensation levels do not significantly change ([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 extends a related literature examining share repurchase as a mechanism through which firms reshape their shareholder base, most prominently [Bagwell \(1991\)](#), who shows that repurchases can deter takeovers by buying out the shareholders with the lowest reservation values. We also show that repurchases can skew shareholder composition to benefit incumbent management,

but our mechanism and setting differ. [Bagwell \(1991\)](#) exploits variation in investors’ reservation values whereas we focus on a specific and observable disclosure event likely to polarize shareholders along a specific dimension (social preferences regarding pay inequality). Further, we provide evidence on which investors exit (short-term institutions with high pre-disclosure social preferences) and on the downstream governance consequences of this shift, including reduced shareholder proposals on CEO compensation and limited pressure to adjust pay ratios.

Our study also intersects with recent research showing SRI funds’ limited success in improving portfolio firms’ sustainable performance (e.g., [Heath, Macciocchi, Michaely, and Ringgenberg, 2023](#)). [Berk and van Binsbergen \(2025\)](#) and [Broccardo, Hart, and Zingales \(2022\)](#) note that excluding firms that fail to meet SRI targets from portfolios may be an ineffective means to achieve sustainability goals. We complement this view by highlighting the role of the firm in promoting or hindering change: When high-pay-ratio firms use share repurchases to induce the exit of shareholders who prioritize social welfare issues, these investors weaken their ability to exercise “voice” and 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](#)). In particular, earlier studies examine the relationship between share repurchases and compensation policies ([Babenko, 2009](#); [Bonaimé, Kahle, Moore, and Nemani, 2024](#); [Kahle, 2002](#)). We find that managers respond to compensation-disclosure-related concerns through repurchases, thereby reducing the need to adjust actual CEO or worker pay. While some evidence points to managers myopically using repurchases to temporarily boost stock prices at the expense of long-term benefits ([Almeida, Ersahin, Fos, Irani, and Kronlund, 2026](#); [Edmans et al., 2022](#)), our findings are more consistent with managers repurchasing to stabilize prices and protect shareholder value.

Finally, our study can inform policymakers. We show that, following the disclosure of pay ratios, firms strategically increase repurchases to concentrate ownership among shareholders less

concerned about pay inequality. This outcome results in a “win-win” situation for managers and shareholders but essentially circumvents regulators’ intentions of narrowing pay divides. In sum, our results raise doubts about the effectiveness of promoting pay 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 \(2026\)](#) and [Chang et al. \(2023\)](#), we exploit multiple sources of variation in the impact of pay ratio disclosures to construct a triple-differences framework.⁵ 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. Finally, we expect firms with relatively high pay ratios to experience larger treatment effects. 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](#)

⁵We acknowledge that our specification is not fully saturated because not-yet-disclosed firms do not have contemporaneous pay ratios and a fully saturated specification may reduce statistical precision. In [Section OA.1](#), however, we discuss this issue in more detail and show that our results are robust to alternative specifications with fully saturated terms.

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 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 cohorts by pooling firms that disclose in the same year-month with not-yet-treated firms (i.e., firms that have not yet disclosed their pay ratios) in their pre-disclosure periods. We omit the last disclosure month to ensure that only not-yet-treated observations function as controls. This procedure guarantees a clean comparison between treated and control firms within each event period. We control for firm and time fixed effects specific to each cohort. As a result, the sample size varies considerably across cohorts, but our results remain consistent across different cohorts (see Figure OA.3).

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, Ghosh, Ryan, and Wang, 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

⁶Knust 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.

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:

$$\begin{aligned} Repurchases_{i,m,c} = & \alpha + \beta \cdot Post\ Disclosure_{i,m,c} \cdot High\ Ratio_i + \theta \cdot Post\ Disclosure_{i,m,c} \\ & + \gamma \cdot \mathbf{X} + \mu_{i,c} + \rho_{m,c} + \varepsilon_{i,m,c} \end{aligned} \quad (1)$$

where i , m , and c denote firm, month, and cohort, respectively. The dependent variable in the baseline model 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 et al., 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 (2025) 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 definitions of control variables are provided in 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. (2026), who report that 77% of a total of 1,362 firms (including financial and utility firms) file disclosures by April 2018. In comparison, our placebo event sample includes 1,084 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 167 (88) in 2018 and varies widely, with a standard deviation of 311, similar to that reported in Carter et al. (2026) and Pan et al. (2022). On average, firms repurchase 0.12% 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 repeatedly drawing not-yet-treated firms, 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 mean monthly repurchases, this estimate translates to a substantial increase of 34%. This magnitude is comparable to the magnitude of other significant shocks to repurchases. 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., 2025](#)).

Models (3) and (4) replicate our regressions using 2017 as a placebo year. Although companies were not required to disclose pay ratios in 2017, we argue that the underlying distribution should resemble that of the initial disclosure year since pay ratio distributions are persistent across consecutive years even after disclosure ([Boone et al., 2024](#)). Thus, we proxy firms' 2017 pay ratio status using their 2018 reported values and assign the same *High Ratio* indicator across both years. If repurchases are simply a regular response to proxy statement disclosures and such a response 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

We extend the baseline analysis by investigating repurchase dynamics surrounding the initial pay ratio disclosure period. To this end, we replace the variable *Post Disclosure* with a continuous set of indicator variables denoting the disclosure month and bifurcating the time periods preceding and following the disclosure month. We do not bin or truncate any relative-time indicators in the event-study specifications (Baker et al., 2022; Sun and Abraham, 2021). We omit the indicator for the month immediately preceding the disclosure and use this month as the reference period. We report the coefficients of interest, the interactions between the time dummies and *High Ratio*, over a ten-month window.

Figure 2 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 one month prior. These results are consistent with parallel pre-trends across the treatment and control groups. Starting in the disclosure month, however, high-pay-ratio firms begin to repurchase more than low-pay-ratio firms, although the differences are not statistically significant. High-pay-ratio firms repurchase significantly more, however, two months after the disclosure. The delayed increase in repurchase intensity is likely due to blackout periods, during which firms are restricted from repurchasing shares. The median disclosure day is the 19th day of the disclosure month, or about 24 days after the start of a typical blackout period and 30 days before it ends. In other words, the repurchase gap widens two months following disclosure once the blackout period ends. It then tapers off. These findings suggest that firms respond to high pay ratios sharply and quickly.

3.3 Robustness tests

We carry out a series of tests in Table 3 to evaluate the robustness of our inferences. Panel A addresses the concern that firms' proxy filing timing is endogenous. Although firms have some

flexibility in disclosure timing within the SEC’s deadline (120 days after the fiscal year-end), the majority of disclosures cluster within a consistent timeframe.⁷ Nevertheless, we explore the validity of this disclosure timing concern. Model (1) replaces the actual disclosure month with the firm’s disclosure month in the prior year (2017) and re-estimates the stacked regression. Models (2), (3), and (4) restrict the sample to firms who have filed proxy statements in the same calendar month since 2017, 2016, or 2015, respectively. Our results remain robust across these specifications, implying that endogenous changes in proxy filing timing are unlikely to explain our findings.⁸

Panel B 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, 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. \(2025\)](#) 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 C of Table 3 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 3-digit SIC. We also construct a proxy that reflects staggered comparisons, similar to [Bozos, Chen,](#)

⁷Only 14 (24) firms delayed (advanced) their disclosures by more than 30 days from 2017 to 2018.

⁸A related concern is that managers, who know the disclosure content and expected filing date in advance, may adjust repurchase policies before the proxy statement filing. In this case, control firms that have not yet disclosed may anticipate the upcoming disclosure by increasing repurchases. Yet, anticipatory repurchases within control firms would only bias us against identifying a positive and significant treatment effect.

Gao, Tian, and Yuan (2025). 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 firms that disclosed 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 D 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, four census tract regions, and firm age. Our results are robust to the inclusion of these additional fixed effects individually and simultaneously.

Panel E reports the estimations from alternative methodologies. First, as a benchmark for the bias-corrected estimates, we use the original sample in a fixed time period from September 2017 to December 2018 and estimate the model using conventional two-way fixed effects (TWFE) regressions. Second, since repurchases often exhibit seasonality related to corporate calendar effects (Dittmann et al., 2025), we extend the window to 36 months prior to the disclosure and include firm-level fiscal month fixed effects in the stacked regression. The estimated coefficients remain comparable to those in the baseline regression.

To conclude our robustness tests, Panel F confirms that our findings are not driven by the concurrence of pay ratio disclosure with the Tax Cuts and Jobs Act (TCJA). 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, 2025; 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 high-dimensional state-year-month fixed effects in the model to account 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 findings suggest that the introduction of the TCJA is unlikely to drive our findings.

3.4 Dividends

We document higher repurchases by high-pay-ratio firms relative to their low-pay-ratio counterparts following pay ratio disclosure. In this section, we examine whether disclosing a high pay ratio affects other payout, namely, dividends. If omitted variables related to firm fundamentals drive our findings, we might also observe significant changes in dividend policies. Alternatively, if repurchase increases are motivated by facilitating shareholder exits, dividends should not increase.

Since dividend data is at the quarterly level, we adjust the frequency of the variables in Eq.

(1) as follows:

$$Dividends_{i,q,c} = \alpha + \beta \cdot Post\ Disclosure_{i,q,c} \cdot High\ Ratio_i + \theta \cdot Post\ Disclosure_{i,q,c} + \gamma \cdot \mathbf{Z} + \mu_{i,c} + \rho_{q,c} + \varepsilon_{i,q,c} \quad (2)$$

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. We adjust several control variables to better accommodate the dependent variables.

Using a range of dividend proxies, including quarterly dividends scaled by market capitalization, total assets, or sales revenue, as well as a dividend dummy, Models (1)–(4) in Table 4 reveal no significant differences between high- and low-pay-ratio firms following disclosure. Model (5) examines whether high-pay-ratio firms that were dividend payers prior to disclosure increased their dividends within the post-disclosure test window. Again, we find no significant evidence. If unobserved firm characteristics related to pay ratios drive the differences in repurchase activity, we may see corresponding differences in other shareholder payouts. Yet, we find that the results are concentrated within share repurchases alone, not dividends. These results provide credence to our hypothesis that firms use repurchases to facilitate shareholder exits.

4 Investor responses to post-mandate repurchases

Thus far, we have established robust evidence that high-ratio firms increase share repurchases following the mandate. We next examine how investors respond to changes in repurchase activity. We focus on the incremental component of repurchases, as they tend to be persistent and reflect

past behavior (Almeida et al., 2026). Because the relationship between investor behavior and repurchases is endogenous, our approach is to isolate the portion of repurchases plausibly driven by the disclosure. To this end, we identify “above-expected repurchasers”—firms whose repurchases exceed predictions based on pre-disclosure coefficient estimates within the short post-disclosure window—and compare them with other firms that either do not repurchase or repurchase less than expected. The predicted levels represent counterfactual repurchases expected in the absence of changes in disclosure mandate. Deviations from these predicted levels therefore capture repurchase activity that departs from firms’ historical policies and is more likely attributable to the disclosure. This comparison allows us to examine how investors react to the mandate-induced component of repurchases. More details are provided in Section OA.5.

4.1 Investor exit

Pay ratio disclosure places CEOs and firms with high reported pay inequality under public scrutiny. Investors’ reactions to high pay ratios depends on their time horizons and preferences, however. Trading strategies differ systematically due to investment horizons (e.g., McCahery et al., 2016; Starks, Venkat, and Zhu, 2026). 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 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 quartile of 2017Q4 average churn rate as short-term.⁹ Table [OA4](#) shows that our results remain similar when defining short-term investors as transient institutions according to Brian Bushee’s classification.

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. 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 2017.¹⁰

Next, we construct ESG profiles at the *firm* level. Specifically, we calculate the weighted average

⁹Investors in the top quartile of the churn rate distribution are classified as short-term investors and have an average churn rate of 12.60%. 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 quartiles 1 through 3 are 0.86%, 2.56%, and 4.81%, respectively.

¹⁰Panels A and B of Table [OA4](#) present summary statistics on KLD ESG scores and investor-level preferences, respectively. Among the short-term investors we identify based on either churn rate or Bushee’s framework, we observe several prominent market participants with stated commitments to environmental and social considerations, such as Alberta Investment, Algebris Investments, and MEAG (Munich Ergo).

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, and *Governance Holdings* for corporate governance.

Because our focus is now on the net effects arising from post-disclosure repurchase strategies, we switch to a cross-sectional test by examining changes in social investor holdings from the disclosure quarter $t - 1$ to quarter $t + 2$, recognizing that investors may adjust their positions gradually over time. Given that above-expected repurchasers are identified within the five-month post-disclosure window, using quarter $t + 2$ helps us capture changes in investor holdings occurring after the repurchase window. We construct an indicator variable, *Drop in Soc Holdings*, which equals one if social holdings at $t + 2$ are lower than at $t - 1$, and zero otherwise. Our main variable of interest is *High Ratio*. All control variables follow those used in Section 3.4 but are measured using pre-disclosure values. Because institutional investors are required to disclose their holdings only at the end of each quarter, the adjustment period mechanically varies with the timing of pay ratio disclosures. To account for industry heterogeneity and differences in exposure duration, we include both industry and disclosure-month fixed effects.

Model (1) of Table 5 shows that, among firms with above-expectations repurchases, high-ratio firms experience a significant increase in the likelihood of a drop in social holdings around the disclosure relative to low-ratio firms. In contrast, Model (2) shows that, among firms that repurchase zero or less than expected, we do not find evidence of an increase in the likelihood of reducing social investors’ holdings in high-ratio firms during the short window surrounding the disclosure. These results suggest that post-disclosure repurchases accelerate the adjustment of socially conscious investor holdings.

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, Models (3) to (6) examine the holdings of investors who are sensitive to environmental and governance issues. We use *Drop in Env Holdings* and *Drop in Gov Holdings* to indicate whether environmentally or governance-conscious investors, respectively, reduce their holdings around the disclosure window. We do not observe a differential reduction in these holdings between high-pay-ratio and low-pay-ratio firms. These results suggest that only investors attentive to social concerns sell off some of their holdings in high-pay-ratio stocks. Additionally, in Models (7) and (8), we use a dependent variable, *Drop in ST Holding*, measuring the drop in proportion of shares held by short-term investors relative to the total shares held by all institutional investors. We again do not find a significant change in overall short-term holdings following the disclosure across high-pay-ratio and low-pay-ratio firms. Taken together, these findings help mitigate the concern that investors are responding to other information contained in the disclosure.

4.2 Alignment between investors and managers

Table 6 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 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 of Table 6 examines changes in pay ratios as a function of initial pay ratio levels and pre-mandate firm characteristics. Because nominal pay ratio figures can be vulnerable to estimation methods (e.g., [Alam et al., 2025](#)), we focus on changes in firms' relative positions within the pay ratio distribution, which may better reflect deliberate managerial actions and efforts aimed at

reducing pay inequality. We estimate models in which the dependent variable is an indicator capturing whether a firm’s CEO pay ratio drops from 2018 to 2019, and the main 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. We first standardize pay ratios within each industry-year and then examine whether a firm’s standardized pay ratio declines from 2018 to 2019. We denote firms as *High Ratio* if their 2018 pay ratio falls in the upper tail of the distribution, for example, above the median, in the top tercile, top quartile, or top quintile, relative to industry peers. High-pay-ratio firms that repurchase more than expected may face less pressure to adjust their pay ratios, whereas other high-pay-ratio firms may remain subject to investor pressure to reduce pay inequality.

In Panel A, high-pay-ratio firms that repurchase more than expected have only limited reductions in pay ratios between the initial disclosure in 2018 and the subsequent disclosure in 2019, especially among firms in the upper tail of the pay ratio distribution. These findings stand in contrast to the more intense likelihood of reducing pay ratio for other firms with high pay ratios. Our findings suggest that, for firms with zero or below-expectation repurchases, investors dissatisfied with high pay ratios are less likely to exit and instead may pressure managers to adjust compensation policies. In contrast, such adjustments are less common for among firms that repurchase more than expected, indicating that managers and investors are more aligned on pay inequality issues.

Panel B 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 Huang and Thakor (2013) and Huang, Maharjan, and Thakor (2020), we argue 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 proxy season (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 shareholder-management alignment (Huang and Thakor, 2013).¹¹ Model (1) examines all shareholder proposals. Since the ISS ESG database classifies proxy proposals into governance-related (GOV) and social responsibility-related (SRI), we further split the sample by these two categories in Models (2) and (3). We then differentiate governance-related proposals based on their specific focus. Model (4) focuses on pay-related proposals pertaining to CEO compensation, because shareholder proposals do not necessarily explicitly reference pay ratios. In contrast, Model (5) examines governance proposals unrelated to pay topics.

In Panel B, we find that high-pay-ratio firms that repurchase more than expected experience a decline in shareholder proposals related to CEO compensation, but not in other categories of proposals. In particular, we do not find significant changes in total proposals, governance-related or SRI-related proposals, or governance proposals unrelated to CEO pay. These results complement and reinforce our findings in Section 4.1 that investors specifically averse to pay inequality issues tend to exit firms that repurchase more than expected. As a result, repurchases following the disclosure of high pay ratios appear to shield managers from pay-related proposals by shareholders.

5 Price support or price manipulation?

5.1 Price pressure

Shareholder exit likely takes time due to frictions such as transaction costs and price impact considerations, which render immediate and simultaneous portfolio adjustment unwise. Moreover, selling at a prevailing depressed market price compromises fund performance. Even sustainable

¹¹Because shareholder proposals are relatively rare, few firms experience a change in proposal receipt between the two years. Partitioning the sample would therefore leave limited variation in the outcome variable in some subsamples. To preserve sufficient variation for estimation, we use an interaction specification in this test.

fund managers have incentives to avoid such losses (Edmans, Gosling, and Jenter, 2025) and may, for example, strategically time their trades around disclosure dates of their portfolio holdings (Parise and Rubin, 2025). Reporting high pay inequality may affect investors’ expectations through various channels and trigger instant adverse market reactions (Bozos et al., 2025; Chang et al., 2023; Pan et al., 2022). Consequently, following high pay ratio disclosures, ongoing price pressure arising prior to shareholder exit may motivate managers to counteract short-term downward price revisions by using share repurchases to stabilize prices (Busch and Obernberger, 2017; De Cesari et al., 2011), so as to reduce exit frictions and encourage subsequent exit.

To investigate if price stabilization motivates firms to repurchase following the disclosure of high pay ratios, we calculate Buy-and-Hold Abnormal Returns (BHARs) over 21-, 32-, and 42-trading-days after the proxy statement disclosure. Price pressures leading up to actual shareholder exits can persist during the months following the disclosure, when managers may simultaneously face blackout restrictions. We use the value-weighted market index as the benchmark since managers likely regard it as the most relevant reference when assessing their firm’s stock performance. For each BHAR measure, we bifurcate our sample 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 firms with low BHARs are those experiencing stronger price pressure and therefore are more likely to repurchase to accommodate or preempt (future) selling by 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 itself, we exclude firm-month observations corresponding to disclosing firms 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 $BHAR[0,32]$ or $BHAR[0,42]$.¹²

Results in Panel A of Table 7 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 pay 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 $Post\ Disclosure \times High\ Ratio$ across low returns and high returns subsamples are statistically different. These results suggest that firms use repurchases to counter the effects of disclosing high pay ratios when price support appears to be a concern.

Since negative price pressure may affect not only the magnitude but also the decision to repurchase, Panel B of Table 7 models the extensive margin. 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.

5.2 Long-run returns

The previous section suggests that firms repurchase to stabilize prices and counteract transient price pressure, which facilitates shareholder exit following the disclosure of high CEO pay ratios. One interpretation of these results is that managers act 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 would expect the stock returns of high-ratio repurchasers to be higher or at least

¹²Since 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 $BHAR[0,21]$, and exclude months from t to $t + 2$ when using $BHAR[0,32]$ or $BHAR[0,42]$. In general, we find that the results remain largely significant in the subsample with low BHARs.

indistinguishable from those of low-ratio firms in the short run, but to mean revert over the long run (Bargeron and Bonaimé, 2020).

We examine BHARs over three post-disclosure periods: the first five months (0–105 trading days), the subsequent five months (106–210 trading days), or the remainder of the year (106–252 trading days).¹³ The final window may capture some information from the second pay ratio disclosure if firms advance their subsequent disclosure dates. Unlike the previous section, where stock performance is evaluated from a managerial perspective using market benchmarks, we follow Bargeron, Bonaimé, and Thomas (2017) and Chen and Wang (2012) to construct matched control portfolios based on similar financial characteristics. Specifically, each sample firm is matched with candidate control firms in the same market capitalization decile, book-to-market quintile, and momentum quintile, all measured prior to the disclosure. From these candidates, we select the five firms with the closest book-to-market ratios. The BHAR for each firm is then computed as the difference between its buy-and-hold return and the average return of its five matched control firms over the same horizon. In Models (1), (3), and (5) of Table 8, we find no evidence that high-pay-ratio firms that repurchase more than expected underperform their low-pay-ratio counterparts in either the short or long run; in some cases, they perform significantly better. These results suggest that repurchase activity is unlikely to be myopic.

We also examine the stock performance of high-ratio firms that repurchase none or less than expected to illustrate why repurchases are likely associated with shareholder exit. If the decision to increase repurchases is a mechanical response to short-term price pressure but irrelevant to changes in the shareholder base, then, in the absence of or with only limited repurchases, high-ratio firms should initially underperform low-ratio firms but eventually converge as the market fully reprices and transitory selling pressure dissipates. However, in Models (2), (4), and (6), we find that their

¹³While our primary focus is on the firm-level analysis of long-run performance, we alternatively use a calendar-time portfolio approach to assess the robustness of our findings. The results are reported in Table OA3 and remain qualitatively similar.

underperformance emerges in the long run. These results are inconsistent with a temporary price effect but suggest that repurchases help alleviate the longer-term valuation pressures due to the costs of retaining dissatisfied investors.

5.3 Do managers manipulate short-term performance?

We next provide more direct evidence to evaluate whether post-disclosure repurchases are driven by managers' self-interested motives to manipulate stock prices. 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. Additionally, if the release of the firm's proxy statement is greeted negatively by the market and executive equity awards vest shortly after such release, managers may 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 whether 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 industry median as having high managerial incentives.

Panel A of Table 9 presents 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.

Panel C in Table 9 examines 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 us-

ing two proxies: *DACC_MJ* is the absolute value of the residuals from regressions of the modified Jones model in each quarter (Dechow, Sloan, and Sweeney, 1995), and *DACC_FW* is the absolute value of the residuals from the Dechow, Richardson, and Tuna (2003) model, which incorporates forward-looking components in estimating expected accruals. We find no evidence of increased 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 the fraction of misses, or *FOM*, defined as $\frac{(K-M)}{N}$, where N is the total number of forecasts, $K(M)$ is the number of forecasts below (above) realized 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* $\times$ *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, 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 following initial disclosures. Because repurchase behavior can be persistent and may reflect past patterns (Almeida et al., 2026; Andriosopoulos

et al., 2021), we study firms whose post-disclosure repurchases exceed expectations based on pre-disclosure behavior. These deviations capture firms' responses to the disclosure mandate.

On the other side of transaction appears to be short-term institutional investors sensitive to social issues. They exit high-pay-ratio firms that repurchase more than expected, even though other short-horizon investors continue to hold shares in these firms. Following disclosures, high-pay-ratio firms that repurchase more than expected show smaller reductions in pay ratios than high-pay-ratio firms that repurchase none or less than expected, especially among firms in the upper tail of the pay ratio distribution. Yet, they receive fewer shareholder proposals related to CEO pay. 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 when firms face polarizing events. 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.

References

- Admati, A. R. and P. Pfleiderer (2009). The “Wall Street Walk” and shareholder activism: Exit as a form of voice. *Review of Financial Studies* 22(7), 2645–2685.
- Alam, Z., C. Ghosh, H. E. Ryan, and L. Wang (2025). Discretion in pay ratio estimation. *Journal of Banking and Finance* 173, 107416.
- Albuquerque, A. M., B. Bennett, P. Lisowsky, and Z. Wang (2025). The effect of the Tax Cuts and Jobs Act on payout policies. Working Paper.
- Almeida, H., N. Ersahin, V. Fos, R. M. Irani, and M. Kronlund (2026). How do short-term incentives affect long-term productivity? *Review of Financial Studies* 39(1), 114–157.
- Almeida, H., V. Fos, and M. Kronlund (2016). The real effects of share repurchases. *Journal of Financial Economics* 119(1), 168–185.
- Almeida, H., R. Huang, and Y. Xuan (2026). Are share repurchases really flexible? *Journal of Financial and Quantitative Analysis* 61(2), 176–205.
- Andriosopoulos, D., A. De Cesari, and K. Stathopoulos (2021). Excess corporate payouts and financial distress risk. *European Financial Management* 27(5), 865–898.
- Babenko, I. (2009). Share repurchases and pay-performance sensitivity of employee compensation contracts. *Journal of Finance* 64(1), 117–150.
- Babenko, I., B. Bennett, and Z. Wang (2025). Does better access to disclosure curb CEO pay? Evidence from a modern information technology improvement. *Journal of Finance* forthcoming.
- Bagwell, L. S. (1991). Share repurchase and takeover deterrence. *RAND Journal of Economics* 22(1), 72–88.
- Baker, A. C., D. F. Larcker, and C. C. Wang (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics* 144(2), 370–395.
- Bank, S. A. and G. S. Georgiev (2019). Securities disclosure as soundbite: The case of CEO pay ratios. *BCL Rev.* 60, 1123.
- Banyi, M. L., E. A. Dyl, and K. M. Kahle (2008). Errors in estimating share repurchases. *Journal of Corporate Finance* 14(4), 460–474.
- Bargeron, L. and A. Bonaimé (2020). Why do firms disagree with short sellers? Managerial myopia versus private information. *Journal of Financial and Quantitative Analysis* 55(8), 2431–2465.
- Bargeron, L., A. Bonaimé, and S. Thomas (2017). The timing and source of long-run returns following repurchases. *Journal of Financial and Quantitative Analysis* 52(2), 491–517.
- Berk, J. B. and J. H. van Binsbergen (2025). The impact of impact investing. *Journal of Financial Economics* 164, 103972.
- Bonaimé, A., K. Hankins, and B. Jordan (2016). The cost of financial flexibility: Evidence from share repurchases. *Journal of Corporate Finance* 38, 345–362.
- Bonaimé, A., K. Kahle, D. Moore, and A. Nemani (2024). Employee compensation still impacts payout policy. *Critical Finance Review* 13(3-4), 419–463.
- Bonaimé, A., Ö. Öztekin, and R. Warr (2014). Capital structure, equity mispricing, and stock repurchases. *Journal of Corporate Finance* 26, 182–200.

- Boone, A., A. Starkweather, and J. T. White (2024). The saliency of the CEO pay ratio. *Review of Finance* 28(3), 1059–1104.
- Bozos, K., J. Chen, Y. Gao, X. Tian, and J. Yuan (2025). Do consumers care about pay inequality? Evidence from household purchase data. Working Paper.
- Brav, A., J. R. Graham, C. R. Harvey, and R. Michaely (2005). Payout policy in the 21st century. *Journal of Financial Economics* 77(3), 483–527.
- Broccardo, E., O. Hart, and L. Zingales (2022). Exit versus voice. *Journal of Political Economy* 130(12), 3101–3145.
- Busch, P. and S. Obernberger (2017). Actual share repurchases, price efficiency, and the information content of stock prices. *Review of Financial Studies* 30(1), 324–362.
- Carter, M. E., L. LaViers, J. J. Sandvik, and D. Xu (2026). CEO pay ratio disclosures and changes in employee pay satisfaction. *Management Science* 72(8), 6559–6579.
- Chang, W., M. Dambra, B. Schonberger, and I. Suk (2023). Does sensationalism affect executive compensation? Evidence from pay ratio disclosure reform. *Journal of Accounting Research* 61(1), 187–242.
- Chen, S.-S. and Y. Wang (2012). Financial constraints and share repurchases. *Journal of Financial Economics* 105(2), 311–331.
- Chen, W., P. Hribar, and S. Melessa (2023). Standard error biases when using generated regressors in accounting research. *Journal of Accounting Research* 61(2), 531–569.
- Chiang, C., W. Dai, J. Fan, H. Hong, and J. Tu (2019). Robust measures of earnings surprises. *Journal of Finance* 74(2), 943–983.
- Dang, V. A., A. De Cesari, and H. V. Phan (2021). Employment protection and share repurchases: Evidence from wrongful discharge laws. *Journal of Corporate Finance* 69, 102036.
- De Cesari, A., S. Espenlaub, and A. Khurshed (2011). Stock repurchases and treasury share sales: Do they stabilize price and enhance liquidity? *Journal of Corporate Finance* 17(5), 1558–1579.
- Dechow, P. M., S. A. Richardson, and I. Tuna (2003). Why are earnings kinky? An examination of the earnings management explanation. *Review of Accounting Studies* 8, 355–384.
- Dechow, P. M., R. G. Sloan, and A. P. Sweeney (1995). Detecting earnings management. *The Accounting Review* 70(2), 193–225.
- Derrien, F., A. Kecskés, and D. Thesmar (2013). Investor horizons and corporate policies. *Journal of Financial and Quantitative Analysis* 48(6), 1755–1780.
- Dittmann, I., A. Y. Li, S. Obernberger, and J. J. Zheng (2025). Equity-based compensation and the timing of share repurchases: The role of the corporate calendar. *Journal of Accounting and Economics* 80(1), 101798.
- Dittmar, A. and L. C. Field (2015). Can managers time the market? Evidence using repurchase price data. *Journal of Financial Economics* 115(2), 261–282.
- Donelson, D. C. and J. J. Hopkins (2016). Large market declines and securities litigation: Implications for disclosing adverse earnings news. *Management Science* 62(11), 3183–3198.
- Edmans, A. (2009). Blockholder trading, market efficiency, and managerial myopia. *Journal of*

- Finance* 64(6), 2481–2513.
- Edmans, A., V. W. Fang, and A. H. Huang (2022). The long-term consequences of short-term incentives. *Journal of Accounting Research* 60(3), 1007–1046.
- Edmans, A., T. Gosling, and D. Jenter (2025). Sustainable investing in practice: Objectives, constraints, and limits to impact. Working Paper.
- Edmans, A. and G. Manso (2011). Governance through trading and intervention: A theory of multiple blockholders. *Review of Financial Studies* 24(7), 2395–2428.
- Ertimur, Y., F. Ferri, and V. Muslu (2011). Shareholder activism and CEO pay. *Review of Financial Studies* 24(2), 535–592.
- Fama, E. F. (1998). Market efficiency, long-term returns, and behavioral finance. *Journal of financial economics* 49(3), 283–306.
- Fenn, G. W. and N. Liang (2001). Corporate payout policy and managerial stock incentives. *Journal of Financial Economics* 60(1), 45–72.
- Gaspar, J.-M., M. Massa, P. Matos, R. Patgiri, and Z. Rehman (2013). Payout policy choices and shareholder investment horizons. *Review of Finance* 17(1), 261–320.
- Guernsey, S., S. M. Sepe, and M. Serfling (2022). Blood in the water: The value of antitakeover provisions during market shocks. *Journal of Financial Economics* 143(3), 1070–1096.
- Heath, D., D. Macciocchi, R. Michaely, and M. Ringgenberg (2023). Does socially responsible investing change firm behavior? *Review of Finance* 27(6), 2057–2083.
- Hillert, A., E. Maug, and S. Obernberger (2016). Stock repurchases and liquidity. *Journal of Financial Economics* 119(1), 186–209.
- Hirschman, A. O. (1970). *Exit, voice, and loyalty: Responses to decline in firms, organizations, and states*. Cambridge: Harvard University Press.
- Hong, H., J. Wang, and J. Yu (2008). Firms as buyers of last resort. *Journal of Financial Economics* 88(1), 119–145.
- Hribar, P., N. T. Jenkins, and W. B. Johnson (2006). Stock repurchases as an earnings management device. *Journal of Accounting and Economics* 41(1-2), 3–27.
- Huang, S., J. Maharjan, and A. V. Thakor (2020). Disagreement-induced CEO turnover. *Journal of Financial Intermediation* 43, 100819.
- Huang, S. and A. V. Thakor (2013). Investor heterogeneity, investor-management disagreement and share repurchases. *Review of Financial Studies* 26(10), 2453–2491.
- Jenter, D. and K. Lewellen (2021). Performance-induced CEO turnover. *Review of Financial Studies* 34(2), 569–617.
- Kahle, K. and R. M. Stulz (2021). Why are corporate payouts so high in the 2000s? *Journal of Financial Economics* 142(3), 1359–1380.
- Kahle, K. M. (2002). When a buyback isn’t a buyback: Open market repurchases and employee options. *Journal of Financial Economics* 63(2), 235–261.
- Knust, L. and D. Oesch (2020). On the consequences of mandatory CEO pay ratio disclosure. Working Paper.
- McCahery, J. A., Z. Sautner, and L. T. Starks (2016). Behind the scenes: The corporate governance

- preferences of institutional investors. *Journal of Finance* 71(6), 2905–2932.
- Mueller, H. M., P. P. Ouimet, and E. Simintzi (2017). Within-firm pay inequality. *Review of Financial Studies* 30(10), 3605–3635.
- Ng, A. C. and Z. Rezaee (2015). Business sustainability performance and cost of equity capital. *Journal of Corporate Finance* 34, 128–149.
- Norman, C. S., A. M. Rose, J. M. Rose, and I. Suh (2021). An investigation of corporate directors’ responses to CEO pay ratio disclosures and say-on-pay votes. *Human Resource Management* 60(3), 455–474.
- Pan, Y., E. S. Pikulina, S. Siegel, and T. Y. Wang (2022). Do equity markets care about income inequality? Evidence from pay ratio disclosure. *Journal of Finance* 77(2), 1371–1411.
- Parise, G. and M. Rubin (2025). Green window dressing. *Journal of Finance* forthcoming.
- Piketty, T. and E. Saez (2003). Income inequality in the United States, 1913–1998. *Quarterly Journal of Economics* 118(1), 1–41.
- SEC (2015). Pay ratio disclosure. Final rule 33-9877, approved August 5. Available from: <https://www.sec.gov/rules/final/2015/33-9877.pdf>.
- Song, J., D. J. Price, F. Guvenen, N. Bloom, and T. Von Wachter (2019). Firming up inequality. *Quarterly Journal of Economics* 134(1), 1–50.
- Starks, L. T., P. Venkat, and Q. Zhu (2026). Corporate ESG profiles and investor horizons. *Journal of Finance* 81(2), 603–642.
- Sun, L. and S. Abraham (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics* 225(2), 175–199.
- Wang, Z., Q. E. Yin, and L. Yu (2021). Real effects of share repurchases legalization on corporate behaviors. *Journal of Financial Economics* 140(1), 197–219.
- Yan, X. and Z. Zhang (2009). Institutional investors and equity returns: Are short-term institutions better informed? *Review of Financial Studies* 22(2), 893–924.

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 refer to blackout periods, the definition of which follows [Dittmann et al. \(2025\)](#).

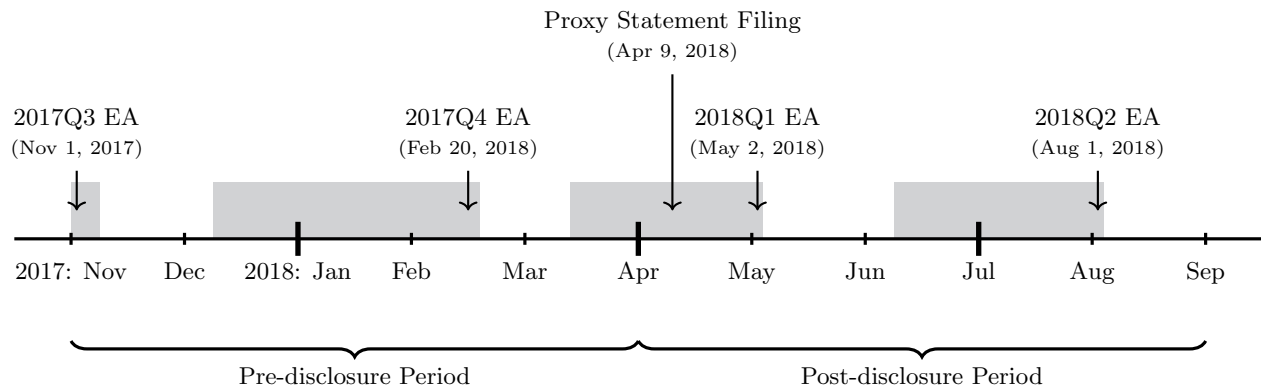


Figure 2: Dynamic effects

This figure reports dynamic effects from a triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. We report the lags and leads of a binary variable that captures the timing of the pay ratio disclosure over a 10-month window around the event. The y -axis plots the coefficients for the interaction terms between *High Ratio* and such lags and leads, where *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. The x -axis indicates months relative to the disclosure. The dashed vertical line indicates the month immediately preceding the disclosure. Following [Baker et al. \(2022\)](#), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from before their own disclosures (not-yet-treated sample). The model includes firm-cohort and year-month-cohort fixed effects. The 95% confidence intervals calculated from standard errors clustered at the firm level are reported as vertical bars. The solid vertical line indicates the median disclosure day (the 19th) in the disclosure month, and the shaded areas refer to the most recent blackout period surrounding the disclosure. The definition of blackout periods follows [Dittmann et al. \(2025\)](#).

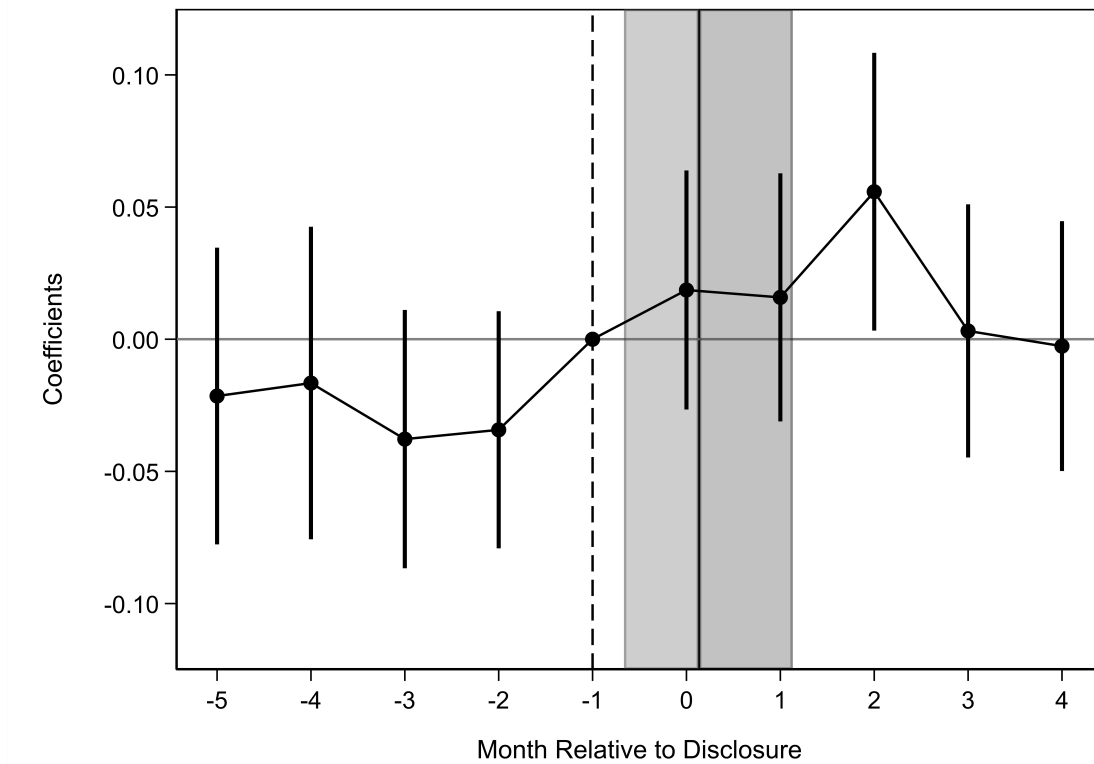


Table 1: Summary statistics

This table reports descriptive statistics for the variables used in our baseline regression model. Panel A presents summary statistics for the main sample around the 2018 disclosure events and for the 2018 pay ratio variables. Panel B 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).

	Obs.	Mean	Lower Quartile	Median	Upper Quartile	Standard Deviation
Panel A: Sample for the 2018 disclosure events						
<i>Monthly data</i>						
Repurchases/MV (%)	12,944	0.1158	0	0	0.0150	0.3050
Post Disclosure	12,944	0.3936	0	0	1	0.4886
Return	12,944	0.0148	-0.0412	0.011	0.0666	0.0979
Blackout	12,944	0.6572	0.3929	0.7000	1	0.3335
<i>Quarterly data</i>						
Ln(Assets)	5,450	7.5370	6.3828	7.4576	8.6074	1.6378
Mkt-to-Book	5,450	2.5029	1.3845	1.8827	2.8376	1.8249
Cash Holding	5,450	0.1860	0.0360	0.0981	0.2538	0.2115
Dividend Payer	5,450	0.4760	0	0	1	0.4995
ROA	5,450	0.0062	-0.0005	0.0119	0.0235	0.0379
Leverage	5,450	0.2733	0.0328	0.152	0.3461	0.3667
<i>Annual data</i>						
Pay Ratio	1,122	167.2820	41.1	87.7	168	310.7245
High Ratio	1,122	0.4973	0	0	1	0.5002
High Ratio (Top Tercile)	1,122	0.3280	0	0	1	0.4697
High Ratio (Top Quartile)	1,122	0.2460	0	0	0	0.4309
High Ratio (Top Quintile)	1,122	0.1943	0	0	0	0.3958
Panel B: Sample for the 2017 disclosure events						
<i>Monthly data</i>						
Repurchases/MV (%)	12,551	0.1040	0	0	0	0.2781
Post Disclosure	12,551	0.3907	0	0	1	0.4879
Return	12,551	0.0153	-0.0389	0.0103	0.0625	0.0969
Blackout	12,551	0.6589	0.4194	0.7000	1	0.3353
<i>Quarterly data</i>						
Ln(Assets)	5,277	7.3787	6.1528	7.2778	8.4198	1.6367
Mkt-to-Book	5,277	2.3726	1.3773	1.8281	2.6927	1.6112
Cash Holding	5,277	0.1934	0.0395	0.1097	0.2629	0.2142
Dividend Payer	5,277	0.4618	0	0	1	0.4986
ROA	5,277	0.0032	-0.0004	0.0105	0.0210	0.0400
Leverage	5,277	0.2714	0.0283	0.1492	0.3440	0.3634

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

This table reports estimates of 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 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. The dependent variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. *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 definitions of control variables can be found in Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from 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: Repurchases/MV (%)			
	2018 Disclosure		2017 Disclosure (Placebo)	
	(1)	(2)	(3)	(4)
Post Disclosure $\times$ High Ratio	0.0329*** (0.012)	0.0401*** (0.012)	0.0112 (0.012)	0.0143 (0.012)
Post Disclosure	0.0072 (0.014)	-0.0050 (0.014)	-0.0091 (0.013)	-0.0165 (0.013)
Ln(Assets)		0.0217 (0.035)		0.0747** (0.030)
Mkt-to-Book		-0.0129* (0.007)		-0.0097** (0.005)
Cash Holding		0.2998*** (0.082)		0.3874*** (0.099)
Dividend Payer		0.0262 (0.022)		0.0112 (0.014)
ROA		0.0365 (0.157)		-0.0070 (0.138)
Leverage		-0.0211 (0.040)		-0.0816** (0.035)
Return		-0.0857*** (0.028)		-0.1022*** (0.027)
Blackout		-0.1322*** (0.020)		-0.0940*** (0.016)
Observations	30,094	30,094	29,439	29,439
R-squared	0.410	0.422	0.414	0.424
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes

Table 3: Robustness tests

This table examines the robustness of the results in Table 2, which estimates triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. All models examine the 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 Panels A, C, D, E and F, the dependent variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. In Panel A, Model (1) re-estimates Model (2) from Table 2 using the disclosure month from 2017; Models (2), (3), and (4) re-estimate the same model by restricting the sample to firms that filed their proxy statements in the same calendar month across earlier years. In Panel B, the dependent variables reflect different measurements of actual share repurchases. In Panel C, *High Ratio* reflects different ways to compare a firm’s disclosed pay ratio to its peers. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from before their own disclosures (not-yet-treated sample). All models include firm-cohort and year-month-cohort fixed effects. In Panel D, all models re-estimate Model (2) from Table 2 using different fixed effects. In Panel E, Models (1) and (2) re-estimate Model (2) from Table 2 using a staggered specification with conventional two-way fixed effects (TWFE); Models (3) and (4) extend the test window to cover 36 months prior to a firm’s proxy filing and up to 4 months after, and estimate with controlling for firm-by-fiscal-month-cohort fixed effects. In Panel F, 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. Detailed definitions of control variables can be found in Table A1. 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: Variations in disclosure month

	Dependent variable: Repurchases/MV (%)			
	2017 disclosure month (1)	Same disclosure month since 2017 (2)	Same disclosure month since 2016 (3)	Same disclosure month since 2015 (4)
Post Disclosure $\times$ High Ratio	0.0346*** (0.012)	0.0346*** (0.013)	0.0412*** (0.014)	0.0439*** (0.013)
Post Disclosure	-0.0004 (0.014)	0.0103 (0.015)	0.0027 (0.016)	0.0139 (0.014)
Observations	30,261	25,655	22,964	20,943
R-squared	0.425	0.427	0.426	0.415
Controls from Table 2	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes

Table 3 (Continued)

Panel B: Alternative measures of repurchases

	Repurchases/ MV Alt. (%) (1)	Repurchases/ Assets (%) (2)	Repurchases/ Sales (%) (3)	Repurchases/ Shares (%) (4)	Repurchase Dummy (5)
Post Disclosure $\times$ High Ratio	0.0429*** (0.013)	0.0515*** (0.019)	0.4111*** (0.116)	0.0400*** (0.012)	0.0572*** (0.015)
Post Disclosure	-0.0013 (0.016)	-0.0065 (0.023)	0.0457 (0.129)	-0.0027 (0.015)	0.0116 (0.019)
Observations	30,094	30,094	30,094	30,094	30,094
R-squared	0.422	0.458	0.496	0.418	0.617
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: Alternative measures of *High Ratio*

	Dependent variable: Repurchases/MV (%)				
	FF30 Median (1)	SIC3 Median (2)	Staggered Median (3)	Over 100 (4)	State Median (5)
Post Disclosure $\times$ High Ratio	0.0341*** (0.012)	0.0375*** (0.012)	0.0402*** (0.013)	0.0296** (0.013)	0.0320** (0.013)
Post Disclosure	-0.0019 (0.014)	-0.0058 (0.014)	-0.0046 (0.014)	0.0014 (0.014)	-0.0011 (0.014)
Observations	30,094	30,094	30,094	30,094	30,094
R-squared	0.421	0.422	0.422	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

Table 3 (Continued)

Panel D: Additional fixed effects

	Dependent variable: Repurchases/MV (%)				
	(1)	(2)	(3)	(4)	(5)
Post Disclosure $\times$ High Ratio	0.0392*** (0.013)	0.0414*** (0.013)	0.0415*** (0.012)	0.0393*** (0.013)	0.0466*** (0.014)
Post Disclosure	-0.0114 (0.014)	-0.0018 (0.015)	-0.0053 (0.014)	0.0101 (0.016)	0.0035 (0.017)
Observations	29,990	28,983	30,090	29,991	28,721
R-squared	0.446	0.448	0.427	0.465	0.523
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	No	No	No	Yes	No
Industry-Year-Month-Cohort FE	Yes	No	No	No	Yes
Inc. State-Year-Month-Cohort FE	No	Yes	No	No	Yes
Region-Year-Month-Cohort FE	No	No	Yes	No	Yes
Firm Age-Cohort FE	No	No	No	Yes	Yes

Panel E: Alternative specifications

	Dependent variable: Repurchases/MV (%)			
	TWFE		Stacked with fiscal-month FE	
	(Sep 2017 to Dec 2018)		(Test window -36,+4)	
	(1)	(2)	(3)	(4)
Post Disclosure $\times$ High Ratio	0.0350*** (0.013)	0.0394*** (0.013)	0.0526*** (0.014)	0.0499*** (0.014)
Post Disclosure	-0.0035 (0.014)	-0.0105 (0.013)	-0.0231 (0.018)	-0.0252 (0.017)
Observations	16,619	16,619	136,780	136,780
R-squared	0.345	0.353	0.494	0.497
Controls from Table 2	No	Yes	No	Yes
Firm FE	Yes	Yes	No	No
Year-Month FE	Yes	Yes	No	No
Firm-Fiscal-Month-Cohort FE	No	No	Yes	Yes
Year-Month-Cohort FE	No	No	Yes	Yes

Table 3 (Continued)

Panel F: The effects of TCJA

	Dependent variable: Repurchases/MV (%)		
	Pre-TCJA CETR < 21% (1)	Pre-TCJA No foreign profits (2)	Headquarter state-time effects (3)
Post Disclosure $\times$ High Ratio	0.0513*** (0.018)	0.0418** (0.018)	0.0416*** (0.013)
Post Disclosure	-0.0047 (0.021)	-0.0035 (0.018)	-0.0012 (0.014)
Observations	15,457	12,432	29,226
R-squared	0.424	0.453	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 4: Do high-pay-ratio firms change dividends following disclosure?

This table reports estimates of changes in quarterly dividends around the mandatory disclosure of CEO-to-median-employee pay ratio. The dependent variables reflect different measurements of dividends. Models (1) to (4) use triple difference-in-differences regressions to examine the 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 quarters 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. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-quarter are grouped into cohorts and pooled with samples from 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. Model (5) examines whether high-pay-ratio firms that were dividend payers prior to disclosure increased their dividends during the post-disclosure period. The standard errors in parentheses are clustered at the industry level. Detailed definitions of control variables can be found in Table A1. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Dividends/ MV (%) (1)	Dividends/ Assets (%) (2)	Dividends/ Sales (%) (3)	Dividend Payer (4)	Dividend Increase (5)
Post Disclosure $\times$ High Ratio	0.0056 (0.008)	0.0018 (0.011)	0.0571 (0.057)	0.0159* (0.008)	
High Ratio					0.0464 (0.078)
Post Disclosure	0.0030 (0.010)	-0.0036 (0.015)	0.0773 (0.089)	0.0082 (0.009)	
Ln(Assets)	-0.0276 (0.017)	-0.1039*** (0.032)	-0.0306 (0.143)	0.0115 (0.018)	0.0528** (0.018)
Mkt-to-Book	-0.0128*** (0.004)	0.0072 (0.005)	0.0609 (0.046)	0.0017 (0.002)	0.0121 (0.019)
ROA	0.1339 (0.088)	0.3140** (0.129)	-2.1931 (1.470)	0.0938 (0.092)	1.0731 (0.842)
Leverage	0.0890*** (0.023)	-0.0457** (0.022)	-0.3845** (0.160)	-0.0427** (0.021)	-0.1428 (0.104)
Return	-0.0499*** (0.013)	-0.0108 (0.016)	-0.1563* (0.081)	-0.0091 (0.014)	0.0688 (0.159)
Observations	8,202	8,202	8,202	8,202	500
R-squared	0.891	0.907	0.907	0.944	0.048
Firm-Cohort FE	Yes	Yes	Yes	Yes	No
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes	No

Table 5: Investor exit

This table reports estimates from cross-sectional regressions of changes in quarterly investor holdings around the mandatory disclosure of CEO-to-median-employee pay ratio. The dependent variables reflect the holdings of short-term investors who exhibit social, environmental, or governance preferences, as well as those of investors in general. They are defined as dummy variables indicating whether these investors reduce their holdings from one quarter prior to the initial pay ratio disclosure to two quarters after the disclosure. The regression sample is split based on whether a firm is an above-expected repurchaser, whose cumulative repurchases within the five-month window following the disclosure exceed the predicted level based on pre-disclosure coefficient estimates. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. This table also reports the p -values of F -statistics testing the differences in coefficients on *High Ratio* between the respective partitioned subsamples. Detailed definitions of control variables can be found in Table A1. All models include industry and disclosure-month fixed effects. The standard errors in parentheses are clustered at the disclosure-month level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Drop in Soc Holdings		Drop in Env Holdings		Drop in Gov Holdings		Drop in ST Holding	
	Above-expected Repurchasers	Other Firms	Above-expected Repurchasers	Other Firms	Above-expected Repurchasers	Other Firms	Above-expected Repurchasers	Other Firms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High Ratio	0.1130** (0.048)	-0.0313 (0.031)	0.0529 (0.033)	-0.0443 (0.058)	-0.0268 (0.046)	0.0372 (0.049)	-0.0127 (0.101)	-0.0170 (0.061)
p -value (equal coefficient estimates)	(0.017)		(0.153)		(0.340)		(0.970)	
Observations	294	752	294	752	294	752	294	752
R-squared	0.162	0.168	0.192	0.162	0.123	0.053	0.096	0.062
Controls from Table 4	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Disclosure Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Investor-management alignment

This table reports estimates from cross-sectional regressions of changes in CEO pay ratios and shareholder proposals around the mandatory disclosure of CEO-to-median-employee pay ratio. In Panel A, the dependent variable reflects the change in CEO pay ratios from 2018 to 2019 and is defined as a dummy variable equal to one if the industry-year standardized ratio in 2019 is lower than that in 2018. *High Ratio*, *High Ratio (Top Tercile)*, *High Ratio (Top Quartile)*, and *High Ratio (Top Quintile)* equal one if the firm discloses a pay ratio above the median, in the top tercile, in the top quartile, and in the top quintile, respectively, for the same Fama French 12 industry. The regression sample is split based on whether a firm is an above-expected repurchaser, whose cumulative repurchases within the five-month window following the disclosure exceed the predicted level based on pre-disclosure coefficient estimates. This table also reports the p -values of F -statistics testing the differences in coefficients on the variables of interest between the respective partitioned subsamples. In Panel B, the dependent variables reflect different types of shareholder proposals and are defined as dummy variables indicating whether a firm received one or more proposals in 2017 but none in 2019. *Above-expected Repurchasers* equals one if a firm has cumulative repurchases within the five-month window following the disclosure exceed the predicted level based on pre-disclosure coefficient estimates. Detailed definitions of control variables can be found in 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: Drop in Pay Ratio							
	Above-expected Repurchasers (1)	Other Firms (2)	Above-expected Repurchasers (3)	Other Firms (4)	Above-expected Repurchasers (5)	Other Firms (6)	Above-expected Repurchasers (7)	Other Firms (8)
High Ratio	0.2111*** (0.064)	0.3511*** (0.087)						
High Ratio (Top Tercile)			0.1398 (0.090)	0.4409*** (0.092)				
High Ratio (Top Quartile)					0.1577* (0.081)	0.4090*** (0.092)		
High Ratio (Top Quintile)							0.1417 (0.084)	0.4106*** (0.069)
p -value (equal coefficient estimates)	(0.198)		(0.026)		(0.048)		(0.020)	
Ln(Assets)	0.0396** (0.016)	0.0068 (0.020)	0.0479** (0.018)	-0.0041 (0.018)	0.0490** (0.021)	0.0126 (0.021)	0.0526** (0.022)	0.0208 (0.024)
Mkt-to-Book	-0.0071 (0.020)	0.0038 (0.011)	-0.0050 (0.019)	0.0035 (0.010)	-0.0053 (0.020)	0.0048 (0.010)	-0.0028 (0.021)	0.0069 (0.010)
ROA	0.1316 (0.464)	0.2110** (0.092)	0.0491 (0.582)	0.2943*** (0.088)	0.0821 (0.582)	0.3134*** (0.084)	0.0755 (0.568)	0.3161*** (0.086)
Leverage	0.0679 (0.091)	-0.0335 (0.031)	0.0755 (0.079)	-0.0356 (0.039)	0.0654 (0.078)	-0.0370 (0.043)	0.0682 (0.076)	-0.0333 (0.039)
Return	0.0776 (0.120)	-0.0964 (0.054)	0.0630 (0.131)	-0.1217** (0.054)	0.0528 (0.129)	-0.1058* (0.049)	0.0654 (0.131)	-0.1133** (0.049)
Observations	298	794	298	794	298	794	298	794
R-squared	0.152	0.211	0.137	0.242	0.140	0.213	0.135	0.201
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6 (Continued)

Panel B: Shareholder proposals

Dependent variable:	Drop in All Proposals (1)	Drop in GOV Proposals (2)	Drop in SRI Proposals (3)	Drop in Pay Related Proposals (4)	Drop in Non-Pay Proposals (5)
High Ratio $\times$ Above-expected Repurchasers	-0.0498 (0.049)	-0.0625 (0.049)	0.0381 (0.039)	0.0281** (0.011)	-0.0701 (0.046)
High Ratio	0.0221 (0.015)	0.0209 (0.014)	-0.0027 (0.023)	-0.0049 (0.004)	0.0226 (0.015)
Above-expected Repurchasers	0.0555 (0.042)	0.0528 (0.036)	0.0101 (0.022)	-0.0012 (0.001)	0.0534 (0.035)
Observations	1,097	1,097	1,097	1,097	1,097
R-squared	0.022	0.021	0.092	0.044	0.022
Controls from Panel A	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Table 7: Cross-sectional analyses: Price pressure

This table reports estimates of 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. In Panel A, the dependent variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. In Panel B, the dependent variable, *Repurchase Dummy*, is a binary variable equal to one if a firm repurchases shares during the month. All models examine the 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 firm’s buy-and-hold return (BHAR) relative to value-weighted market index 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 [0,21] (BHAR [0,32] or BHAR [0,42]). This table also reports the *p*-values of *F*-statistics testing the differences in coefficients on *Post Disclosure* $\times$ *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 definitions of control variables can be found in Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from 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: Repurchases/MV (%)					
	BHAR [0,21]		BHAR [0,32]		BHAR [0,42]	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
Post Disclosure $\times$ High Ratio	0.0659*** (0.021)	0.0171 (0.017)	0.0852*** (0.021)	-0.0007 (0.019)	0.0631*** (0.023)	0.0154 (0.018)
<i>p</i> -value (equal coefficient estimates)	(0.067)		(0.002)		(0.098)	
Post Disclosure	-0.0192 (0.025)	-0.0154 (0.024)	-0.0365 (0.029)	0.0083 (0.026)	-0.0104 (0.026)	-0.0087 (0.028)
Observations	13,115	15,658	12,506	15,258	12,624	15,140
R-squared	0.389	0.453	0.439	0.415	0.425	0.424
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 7 (Continued)

Panel B: Repurchase likelihood

	Dependent variable: Repurchase Dummy					
	BHAR [0,21]		BHAR [0,32]		BHAR [0,42]	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)
Post Disclosure $\times$ High Ratio	0.0709*** (0.026)	0.0511** (0.022)	0.1038*** (0.027)	0.0349 (0.023)	0.1047*** (0.026)	0.0289 (0.023)
<i>p</i> -value (equal coefficient estimates)	(0.559)		(0.054)		(0.032)	
Post Disclosure	0.0009 (0.039)	0.0080 (0.029)	-0.0482 (0.036)	0.0537 (0.036)	-0.0390 (0.040)	0.0530 (0.036)
Observations	13,115	15,658	12,506	15,258	12,624	15,140
R-squared	0.605	0.630	0.627	0.612	0.632	0.605
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: Long-run returns

This table reports estimates from cross-sectional regressions of Buy-and-Hold Abnormal Returns (BHARs) following the mandatory disclosure of CEO-to-median-employee pay ratio. The dependent variables reflect each firm's BHAR relative to the average return of matched firms over three post-disclosure horizons: the first five months (0–105 trading days), the subsequent five months (106–210 trading days), or the remainder of the year (106–252 trading days). Five matched firms are selected from the same market capitalization decile, book-to-market quintile, and momentum quintile prior to the disclosure, with the closest book-to-market ratios. The regression sample is split based on whether a firm is an above-expected repurchaser, whose cumulative repurchases within the five-month window following the disclosure exceed the predicted level based on pre-disclosure coefficient estimates. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. This table also reports the p -values of F -statistics testing the differences in coefficients on *High Ratio* between the respective partitioned subsamples. Detailed definitions of control variables can be found in Table A1. All models include industry and disclosure-month fixed effects. The standard errors in parentheses are clustered at the disclosure-month level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

	BHAR[0-105]		BHAR[106-210]		BHAR[106-252]	
	Above- expected Repurchasers	Other Firms	Above- expected Repurchasers	Other Firms	Above- expected Repurchasers	Other Firms
	(1)	(2)	(3)	(4)	(5)	(6)
High Ratio	-0.0488	0.0015	0.0238**	-0.0407*	0.0120	-0.0479**
	(0.033)	(0.009)	(0.009)	(0.021)	(0.018)	(0.021)
p -value (equal coefficient estimates)	(0.139)		(0.008)		(0.034)	
Observations	297	770	297	770	297	770
R-squared	0.146	0.095	0.123	0.122	0.131	0.100
Controls from Table 4	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Disclosure Month FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: Managerial incentives and manipulation

This table reports estimates of triple difference-in-differences regressions of monthly repurchases and quarterly discretionary accruals and earnings surprises around the mandatory disclosure of CEO-to-median-employee pay ratio. Models in Panels A and B examine the 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 variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. In Panel B, the dependent variable, *Repurchase Dummy*, is a binary variable equal to one if a firm repurchases shares during the month. 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* $\times$ *High Ratio* between the respective partitioned subsamples. All models include firm-cohort and year-month-cohort fixed effects. Models in Panel C examine the 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 quarters surrounding the event). The dependent variables reflect different measurements of discretionary accruals and earnings surprises. All models include firm-cohort and year-quarter-cohort fixed effects. *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 definitions of control variables can be found in Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from 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: Repurchase magnitude

	Dependent variable: Repurchases/MV (%)					
	State ATP Index		CEO Pay Delta		CEO Pay Vega	
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
Post Disclosure $\times$ High Ratio	0.0469** (0.022)	0.0382** (0.015)	0.0381** (0.017)	0.0465** (0.022)	0.0318* (0.017)	0.0522** (0.020)
<i>p</i> -value (equal coefficient estimates)	(0.740)		(0.761)		(0.443)	
Post Disclosure	-0.0118 (0.023)	0.0008 (0.018)	-0.0055 (0.025)	-0.0082 (0.021)	0.0006 (0.023)	-0.0161 (0.021)
Observations	9,163	20,908	12,794	13,830	12,885	14,100
R-squared	0.427	0.422	0.430	0.408	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: Repurchase Dummy					
	State ATP Index		CEO Pay Delta		CEO Pay Vega	
	High (1)	Low (2)	High (3)	Low (4)	High (5)	Low (6)
Post Disclosure $\times$ High Ratio	0.0679** (0.032)	0.0505*** (0.018)	0.0444* (0.025)	0.0738*** (0.024)	0.0453** (0.023)	0.0747*** (0.025)
<i>p</i> -value (equal coefficient estimates)	(0.635)		(0.395)		(0.382)	
Post Disclosure	-0.0103 (0.034)	0.0264 (0.023)	0.0307 (0.037)	0.0165 (0.024)	0.0219 (0.032)	0.0047 (0.028)
Observations	9,163	20,908	12,794	13,830	12,885	14,100
R-squared	0.614	0.619	0.612	0.607	0.635	0.591
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: Discretionary accruals and earnings surprises

	DACC_MJ (1)	DACC_FW (2)	Positive Surprise (3)	FOM (4)
Post Disclosure $\times$ High Ratio	-0.0034* (0.002)	-0.0021 (0.002)	-0.0423* (0.025)	-0.0973** (0.042)
Post Disclosure	-0.0035 (0.002)	-0.0019 (0.002)	0.0338 (0.033)	0.1128** (0.056)
Observations	8,056	8,052	7,304	7,206
R-squared	0.461	0.460	0.446	0.421
Controls from Table 4	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Quarter-Cohort FE	Yes	Yes	Yes	Yes

Appendix

Table A1: Definitions and measurement of variables

Variable	Definition
<i>Monthly data</i>	
Repurchases/MV	The product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. (<i>Source</i> : EDGAR, Compustat)
Post Disclosure	Dummy equal to one if a firm has disclosed its pay ratio in the current or previous months. (<i>Source</i> : EDGAR)
Return	Monthly period stock return. (<i>Source</i> : CRSP)
Blackout	Fraction of blackout days within a month. The blackout period is from 20 days before the end of a firm's fiscal quarter until 3 days after the following earnings announcement, following Dittmann et al. (2025) . (<i>Source</i> : Compustat)
Repurchases/MV Alt.	Monthly repurchase value scaled by market capitalization one month prior to the disclosure event. (<i>Source</i> : EDGAR, CRSP)
Repurchases/Assets	Monthly repurchase value scaled by quarterly total assets of the previous quarter. (<i>Source</i> : EDGAR, Compustat)
Repurchases/Sales	Monthly repurchase value scaled by quarterly sales revenue of the previous quarter. (<i>Source</i> : EDGAR, Compustat)
Repurchases/Shares	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. (<i>Source</i> : EDGAR, Compustat, CRSP)
Repurchase Dummy	Dummy equal to one if a firm repurchases shares during the month. (<i>Source</i> : EDGAR)
<i>Quarterly data</i>	
Ln(Assets)	Natural logarithm of total assets. (<i>Source</i> : Compustat)
Mkt-to-Book	Total assets minus book common equity plus market capitalization, scaled by total assets. (<i>Source</i> : Compustat)
Cash Holding	Cash and short-term investments over total assets. (<i>Source</i> : Compustat)
Dividend Payer	Dummy equal to one if the firm has paid dividends in a quarter. (<i>Source</i> : Compustat)
ROA	Income before extraordinary items over total assets. (<i>Source</i> : Compustat)
Leverage	Long-term debt plus long-term debt due in one year over market capitalization. (<i>Source</i> : Compustat)
Post Disclosure	Dummy equal to one if a firm has disclosed its pay ratio in the current or previous quarters. (<i>Source</i> : EDGAR)
Return	Quarterly period stock return. (<i>Source</i> : CRSP)
Dividends/MV	Quarterly dividends scaled by market capitalization of the previous quarter. (<i>Source</i> : Compustat)
Dividends/Assets	Quarterly dividends scaled by total assets of the previous quarter. (<i>Source</i> : Compustat)
Dividends/Sales	Quarterly dividends scaled by sales revenue of the previous quarter. (<i>Source</i> : Compustat)
Dividend Increase	Dummy equal to one if a firm that was a dividend payer prior to disclosure increased its dividends within the post-disclosure test window. (<i>Source</i> : Compustat)
Drop in Soc Holdings	Dummy equal to one if social holdings decrease from one quarter prior to the disclosure to two quarters after the disclosure. (<i>Source</i> : EDGAR, Thomson/Refinitiv Institutional (13F) Holdings, MSCI KLD)

Continued on next page

Continued from previous page

Variable	Definition
Drop in Env Holdings	Dummy equal to one if environmental holdings decrease from one quarter prior to the disclosure to two quarters after the disclosure. (<i>Source</i> : EDGAR, Thomson/Refinitiv Institutional (13F) Holdings, MSCI KLD)
Drop in Gov Holdings	Dummy equal to one if governance holdings decrease from one quarter prior to the disclosure to two quarters after the disclosure. (<i>Source</i> : EDGAR, Thomson/Refinitiv Institutional (13F) Holdings, MSCI KLD)
Drop in ST Holding	Dummy equal to one if the proportion of shares held by short-term investors relative to the total shares held by all institutional investors decrease from one quarter prior to the disclosure to two quarters after the disclosure. (<i>Source</i> : EDGAR, Thomson/Refinitiv Institutional (13F) Holdings)
DACC_MJ	Absolute value of the residuals from regressions of the modified Jones model (Dechow et al., 1995). (<i>Source</i> : Compustat)
DACC_FW	Absolute value of the residuals from the Dechow et al. (2003) model. (<i>Source</i> : Compustat)
Positive Surprise	Dummy equal to one if a firm meets or beats the consensus estimate of quarterly analyst EPS. (<i>Source</i> : IBES)
FOM	Fraction of misses, defined as $(K - M)/N$, where N is the total number of forecasts and K (M) is the number of forecasts below (above) realized EPS, following Chiang et al. (2019). (<i>Source</i> : IBES)
<i>Annual data</i>	
High Ratio	Dummy equal to one if the firm's pay ratio exceeds the median pay ratio of its Fama French 12 industry. (<i>Source</i> : EDGAR)
Drop in Pay Ratio	Dummy equal to one if the industry-year standardized CEO pay ratio decreases from 2018 to 2019. (<i>Source</i> : EDGAR)
Ln(Assets)	Natural logarithm of total assets. (<i>Source</i> : Compustat)
Mkt-to-Book	Total assets minus book common equity plus market capitalization, scaled by total assets. (<i>Source</i> : Compustat)
ROA	Income before extraordinary items over total assets. (<i>Source</i> : Compustat)
Leverage	Long-term debt plus long-term debt due in one year over market capitalization. (<i>Source</i> : Compustat)
Return	Annual stock return. (<i>Source</i> : CRSP)
Drop in All Proposals	Dummy equal to one if a firm receives one or more shareholder proposals in 2017 but none in 2019. (<i>Source</i> : ISS ESG)
Drop in GOV Proposals	Dummy equal to one if a firm receives one or more governance-related proposals in 2017 but none in 2019. (<i>Source</i> : ISS ESG)
Drop in SRI Proposals	Dummy equal to one if a firm receives one or more social-responsibility-related proposals in 2017 but none in 2019. (<i>Source</i> : ISS ESG)
Drop in Pay Related Proposals	Dummy equal to one if a firm receives one or more proposals related to CEO compensation in 2017 but none in 2019. (<i>Source</i> : ISS ESG)
Drop in Non-Pay Proposals	Dummy equal to one if a firm receives one or more governance proposals unrelated to pay in 2017 but none in 2019. (<i>Source</i> : ISS ESG)
<i>Other data</i>	
Above-expected Repurchasers	Dummy equal to one if cumulative repurchases within the five-month window following disclosure exceed the predicted level based on pre-disclosure coefficient estimates. (<i>Source</i> : EDGAR, Compustat, CRSP)

Continued on next page

Continued from previous page

Variable	Definition
BHAR[0, t]	Buy-and-hold abnormal return over t trading days after the proxy statement disclosure, where $t \in \{21, 32, 42\}$, using the value-weighted market index as the benchmark. (<i>Source</i> : EDGAR, CRSP)
BHAR[a, b]	Buy-and-hold abnormal return over trading days a – b , where $(a, b) \in \{(0, 105), (106, 210), (106, 252)\}$, calculated relative to the average return of five matched control firms. Matched firms are selected from the same market capitalization decile, book-to-market quintile, and momentum quintile prior to the disclosure, with the closest book-to-market ratios. (<i>Source</i> : EDGAR, Compustat, 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

Online Appendix

OA.1 Fully saturated triple-difference

In our baseline analysis, we estimate a heterogeneous DiD specification that exploits variation in firms' disclosure timing and allows the disclosure effect to differ between high- and low-pay-ratio firms. This specification provides a transparent economic interpretation of the coefficient on $Post\ Disclosure \times High\ Ratio$, but also imposes a restriction that high- and low-pay-ratio firms serving as not-yet-treated controls do not experience differential changes in repurchase behavior around other cohorts' disclosure dates.

We assess the importance of this restriction using two more flexible specifications. First, we estimate a fully saturated triple-difference specification by adding $Post\ Cohort \times High\ Ratio$. $Post\ Cohort$ is an indicator that equals one if the firms in each cohort (including both disclosed and not-yet-disclosed firms) have reached or passed the cohort-specific event time. The coefficient on $Post\ Cohort \times High\ Ratio$ captures the average post-event change between high- and low-ratio firms in the not-yet-disclosed group. In Model (1) of Table OA1, its estimated coefficient is small, and our coefficient of interest on $Post\ Disclosure \times High\ Ratio$ remains close to that in the baseline specification. The primary drawback to Model (1) is that, by construction, controls firms have not yet disclosed their pay ratio, implying a potential lookahead bias. Second, we further relax this restriction by interacting the $High\ Ratio$ indicator with time-cohort fixed effects. This specification allows unrestricted time paths for high- and low-pay-ratio firms within each cohort. In Model (2), the coefficient on $Post\ Disclosure \times High\ Ratio$ remains positive and statistically significant. Therefore, our findings do not appear to be driven by differential cohort-specific time dynamics between high- and low-ratio firms.

Overall, our inferences are not sensitive to the baseline restriction. However, these flexible specifications come at the cost of statistical precision because they absorb variation that is also

informative about $Post\ Disclosure \times High\ Ratio$. We therefore retain the parsimonious specification as our baseline, while showing that our estimates are robust to this restriction.

OA.2 Do transitory performance shocks drive the high-pay-ratio classification?

Our baseline specification conditions the treatment effect on firms' relative pay-ratio classifications. A potential concern is that the *High Ratio* indicator may capture transitory firm-level shocks that simultaneously affect CEO compensation and repurchase decisions. For example, a positive performance shock may increase both performance-based CEO compensation and a firm's capacity to repurchase shares, so the estimated effect could partly reflect omitted performance-related variables rather than a response to the pay-ratio disclosure itself. We conduct several additional tests to address this possibility.

Ideally, one could observe firms' actual CEO-to-median-employee pay ratios before the disclosure mandate and examine whether firms' relative positions in the pay-ratio distribution persist over time. Historical median-employee compensation, however, is not available because firms were not required to disclose it before the mandate. We therefore construct estimated pay ratios by holding each firm's denominator fixed at its 2018 median-employee-pay measure and replacing the numerator with CEO compensation from earlier years. In this construction, all time-series variation in the estimated ratio is attributable to CEO compensation. If transitory shocks to CEO compensation explain firms' 2018 high- or low-pay-ratio classifications, the estimated historical classifications should be sensitive to such shocks.

For each year, CEO compensation is measured consistently with Item 402(c)(2)(x), the compensation measure required by the SEC for the numerator of the disclosed pay ratio. We obtain these data from ExecuComp and Equilar and hand-collect the remaining observations from proxy statements when available. We first calculate the estimated pay ratio for 2017. The estimated pay

ratio has a mean of 163 and a median of 76, close to the distribution of the actual ratios subsequently disclosed in 2018. Moreover, relative to firms' 2018 high- or low-pay-ratio classifications, 976 of 1,117 firms (87.38%) remain in the same group. This high persistence suggests that the *High Ratio* indicator in 2018 is not generally the result of a one-time realization of CEO compensation.

In Model (1) of Table OA2, we replace the actual *High Ratio* used in the placebo test in Table 2 with the *Estimated High Ratio* for 2017. If omitted variables raise CEO compensation and independently induce firms to repurchase more around proxy filings, firms classified as high-pay-ratio using contemporaneous estimated pay ratios should continue to show a differential change in repurchases. We find no such effect. Thus, high CEO compensation or strong firm performance does not by itself lead to the changes in post-proxy repurchase when the pay ratio disclosure mandate was not yet in effect.

In Model (2), we re-estimate the baseline specification after restricting the sample to firms whose *Estimated High Ratio* in 2017 is unchanged from their actual 2018 classification. Because these firms remain on the same side of the industry median despite year-to-year changes in CEO compensation, their 2018 classification is less likely to be driven by a transitory CEO pay shock. The estimated treatment effect remains similar to that in the baseline specification.

We then extend this approach further back in time by estimating historical ratios using proxy statements filed as early as 2011. Relative to their 2018 classifications, 68.44% (62.10%) of firms remain in the same high- or low-pay-ratio group from 2014 (2011) onward. In Models (3) and (4), we restrict the baseline sample to firms whose pay ratio classifications remain unchanged throughout these long pre-disclosure periods. The baseline results remain similar in these stringent samples. Overall, our baseline findings are unlikely to be driven by omitted transitory performance-related shocks that jointly affect CEO compensation and repurchases.

OA.3 Estimations across different cohorts

Most firms disclosed their pay ratios in March or April 2018, so the sample of not-yet-treated firms that can serve as the control group becomes smaller for firms disclosing in later months. To test the robustness of our findings to changes in the composition of the control sample, we gradually expand the regressions by disclosure cohort. Because the first cohort contains only a few treated firms that disclosed in February, we begin by examining the first two cohorts, which comprise the treated firms in February and March together with their corresponding not-yet-treated control firms. We then sequentially add one additional disclosure cohort at a time and re-estimate the effects until the full baseline sample is included. Across all specifications in Figure OA1, the results remain consistent.

OA.4 Alternative classifications of pay ratio

As an alternative specification to evaluate the relative levels of pay ratios, we categorize the distribution of CEO pay ratios into terciles, quartiles, and quintiles in each industry, omitting the first group in each classification as the benchmark. This approach allows us to evaluate whether the results are driven by the most extreme cases of pay disparity or are broadly distributed across firms. As shown in Table OA3, relative to firms in the lowest pay ratio group, the effects are more pronounced among firms in the upper tail of the pay ratio distribution, with the strongest effect concentrated among firms in the highest pay ratio group.

OA.5 Identification of post-mandate above-expected repurchasers

Share repurchases may be persistent over time (e.g., Almeida et al., 2026; Andriosopoulos et al., 2021). Ideally, when studying the effects of repurchase activity following the pay ratio disclosure, one would identify the exact amount of repurchases directly attributable to the disclosure. In prac-

tice, however, managers typically cite only general motivations for repurchase programs without distinguishing their underlying objectives, rendering the mandate-induced component of repurchases unobservable. We therefore focus on repurchases above expectations in the post-disclosure period. A sudden increase in repurchases within the short window relative to expected levels likely reflects managerial responses to the disclosure mandate.

We construct a sample spanning from 36 months prior to the disclosure month to 4 months after disclosure. As defined in the main text, the test window runs from 5 months before to 4 months after the disclosure. The estimation window is set from 36 to 13 months prior to the disclosure. We exclude the 12 months immediately preceding the mandate to reserve an out-of-sample validation period and to avoid potential contamination as the disclosure approaches, although the results are similar when the full 36-month pre-disclosure period is used for estimation. Within the estimation window, we regress repurchases on the same set of control variables used in the baseline specification, along with firm fixed effects. The inclusion of firm fixed effects removes time-invariant firm characteristics when later computing residual repurchases. Because the coefficients estimated in the pre-disclosure window are subsequently used to generate out-of-sample predictions for the post-disclosure period, time fixed effects cannot be included directly. Instead, we construct a monthly average repurchase measure (excluding the focal firm), *Average Repurchases*, to capture common time trends and include it as an additional control. The coefficients estimated from the pre-disclosure window are as follows:

$$\begin{aligned}
Repurchases/MV(\%) = & 0.0375 \cdot Ln(Assets) - 0.0154 \cdot Mkt-to-Book + 0.1130 \cdot Cash Holding \\
& + 0.0040 \cdot Dividend Payer + 0.0255 \cdot ROA - 0.0532 \cdot Leverage \\
& - 0.0755 \cdot Return - 0.0660 \cdot Blackout + 0.7216 \cdot Average Repurchases \\
& + Firm Fixed Effects
\end{aligned}$$

Using the estimated coefficients, we calculate expected repurchase levels for the full sample and define abnormal repurchases as the difference between actual and predicted values (i.e., the regression residuals). To assess potential bias in the model, we use the period from 12 months prior to the disclosure up to the beginning of the test window as a validation window. During this period, regressing abnormal repurchases on a constant yields an insignificant coefficient (untabulated), suggesting that the model does not produce systematic prediction errors.

Finally, in the post-disclosure window $[0,+4]$, we aggregate abnormal repurchases and classify repurchasing firms with positive cumulative abnormal repurchases as “above-expected repurchasers.”¹⁴ We rely on a dummy indicator to mitigate potential concerns about measurement error in the magnitude of abnormal repurchases estimated from the first-step model (e.g., [Chen, Hribar, and Melessa, 2023](#)). In total, 299 out of 1,122 firms (26.65%) are identified as above-expected repurchasers. The remaining firms, including those either do not repurchase or repurchase below expected levels, are grouped as “other firms.” As an additional robustness check, we restrict the test window to the first three months following the disclosure. This restriction is motivated by the observation that repurchase activity, on average, spikes in the second month after the disclosure month. Under this alternative window, 285 firms (25.40%) are classified as above-expected repurchasers, and our main results remain robust.

OA.6 Summary statistics and Bushee’s investor classification

In the main text, we provide evidence that shareholders with short-term investment horizons, which we identify using the churn rate following [Yan and Zhang \(2009\)](#), exit high pay ratio firms. Although there is no unified method for classifying short-term investors, we alternatively adopt Brian Bushee’s institutional investor classification, which categorizes institutions as transient, ded-

¹⁴Monthly repurchases can be lumpy, so focusing on cumulative abnormal repurchases over the post-disclosure window may mask firms that experience a spike in repurchases in certain months but negative abnormal repurchases in others. However, such potential misclassification would likely bias against finding our predicted effects and thus underestimate our results.

icated, or quasi-indexer. Our focus is on transient institutions, which are characterized by high portfolio turnover and broad diversification and are more likely to trade on news. For comparison, we report summary statistics for KLD ESG scores, investor preferences, and firm-level institutional holdings in Table OA4, where Panel A presents ESG scores and Panels B and C report the latter variables under the two classification schemes. The distributions of short-term and transient investors appear similar across the two approaches. When we replicate the regression analyses from Table 5 using Bushee’s classification, the results in Panel D are broadly consistent.

OA.7 Calendar-time portfolio returns

Because disclosure events are highly concentrated during the 2018 proxy season, post-disclosure holding periods can overlap substantially across firms, which may induce cross-sectional dependence in long-horizon abnormal returns (Fama, 1998). To address this concern, we use a calendar-time portfolio approach to evaluate the robustness of our long-run return results.

For each post-disclosure window ($[0, 105]$, $[106, 210]$, and $[106, 252]$ trading days), we construct four daily portfolios by independently sorting firms on pay ratio (high- versus low-pay-ratio) and repurchase activity (above-expected repurchasers versus other firms). On each calendar trading day, each portfolio includes all firms whose event time relative to the disclosure date falls within the corresponding post-disclosure window. Firms are equally weighted within each portfolio. We then calculate return spreads between high-pay-ratio and low-pay-ratio firms separately for above-expected repurchasers and other firms and regress the zero-investment daily portfolio returns on the Carhart four factors. For ease of interpretation, Table OA5 reports monthly alphas, calculated by multiplying the estimated daily alphas by 21. The calendar-time results are qualitatively consistent with those reported in Table 8.

OA.8 Repurchase history

We examine whether prior announcements of repurchase programs affect repurchase activities following the pay ratio disclosure. Managers retain considerable flexibility in adjusting repurchase activity. In particular, they may have preauthorized repurchase programs that they can act upon; for example, existing repurchase programs may remain in effect for as long as three years (e.g., [Bargeron et al., 2017](#)). Otherwise, managers may be able to obtain a new authorization quickly. Anecdotal evidence suggests that a board can authorize a repurchase program on the same day that management recommends it, allowing the firm to begin repurchases shortly thereafter.¹⁵

We use historical repurchase activity to identify whether a firm has a repurchase program in place. Using the disclosure month as the endpoint, we examine whether firms repurchased shares in the preceding periods. We find that 51.52% and 44.20% of sample firms have never repurchased shares within the 12 or 24 months prior to the disclosure, respectively. Similarly, 41.16% of firms did not engage in any repurchases between the start of 2016 and the disclosure month. We bifurcate the baseline sample accordingly and report the results in Table [OA6](#). The findings show no significant differences across these subsamples in either the magnitude or the likelihood of repurchases. Therefore, the pay ratio disclosure, particularly for firms with high pay ratios, appears salient enough to trigger repurchases even among firms without a recent history of repurchasing activity.

¹⁵For example, [PNC Bank Corp.](#) disclosed that management recommended and the board authorized a new 24-million-share repurchase program at the same board meeting on January 5, 1995.

Figure OA1: Estimations across different cohorts

This figure reports estimates of triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. The dependent variable is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from before their own disclosures (not-yet-treated sample). The model includes firm-cohort and year-month-cohort fixed effects. The y -axis plots the coefficients on the interaction terms between *Post Disclosure* and *High Ratio*. *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. The x -axis indicates the sequential inclusion of disclosure-month cohorts into the regression, leading up to the baseline results (reported in Model (2) from Table 2). The 95% confidence intervals calculated from standard errors clustered at the firm level are reported as vertical bars.

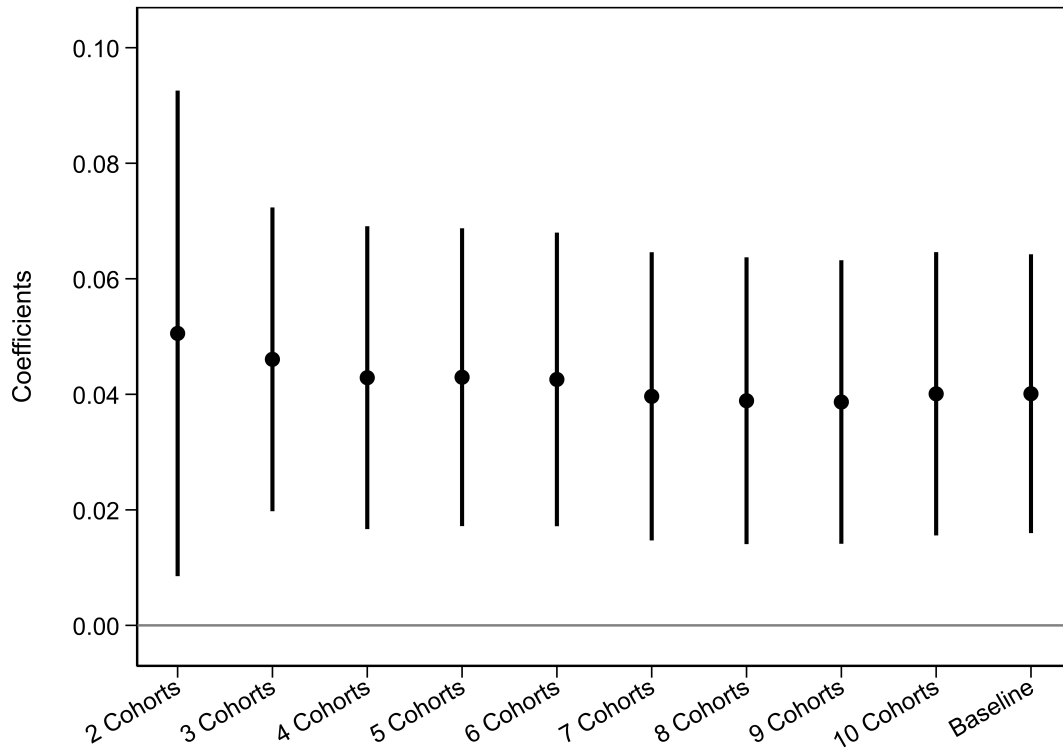


Table OA1: Fully saturated triple-difference

This table reports estimates of triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. The dependent variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. *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. This model includes firm-cohort and year-month-cohort fixed effects. Model (1) re-estimates Model (2) from Table 2 by adding an interaction term between *Post Cohort* and *High Ratio*. *Post Cohort* is a binary variable set to one if a firm's cohort-specific event time falls in the current or previous months. Model (2) re-estimates the same model with the *High Ratio* indicator interacted with year-month-cohort fixed effects. Detailed definitions of control variables can be found in Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from 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.

	Dependent variable: Repurchases/MV (%)	
	(1)	(2)
Post Disclosure $\times$ High Ratio	0.0369* (0.020)	0.0527** (0.026)
Post Cohort $\times$ High Ratio	0.0032 (0.016)	
Post Disclosure	-0.0035 (0.015)	-0.0130 (0.017)
Observations	30,094	30,094
R-squared	0.422	0.425
Controls from Table 2	Yes	Yes
Firm-Cohort FE	Yes	Yes
Year-Month-Cohort FE	Yes	No
High Ratio-Year-Month-Cohort FE	No	Yes

Table OA2: Variations in high-pay-ratio classification

This table reports estimates of triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. The dependent variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. *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. Model (1) re-estimates Model (4) from Table 2 (placebo tests) by replacing *High Ratio* with *Estimated High Ratio*, calculated using 2017 CEO compensation as the numerator while holding median-employee-pay fixed at its 2018 value as the denominator. Models (2), (3), and (4) re-estimate the same model by restricting the sample to firms whose estimated high-pay-ratio classifications in earlier years match their actual 2018 *High Ratio* classification. Detailed definitions of control variables can be found in Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from 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: Repurchases/MV (%)			
	Estimated pay ratio in 2017 (1)	Same group since 2017 (2)	Same group since 2014 (3)	Same group since 2011 (4)
Post Disclosure $\times$ Estimated High Ratio	0.0051 (0.012)			
Post Disclosure $\times$ High Ratio		0.0371*** (0.013)	0.0457*** (0.015)	0.0456*** (0.016)
Post Disclosure	-0.0121 (0.013)	-0.0139 (0.015)	-0.0225 (0.017)	-0.0142 (0.019)
Observations	29,439	26,087	19,807	16,209
R-squared	0.424	0.437	0.455	0.458
Controls from Table 2	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes

Table OA3: Alternative classifications of pay ratio

This table reports estimates of triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio. The dependent variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. All models re-estimate Model (2) from Table 2 using alternative classifications of the pay ratio, including tercile, quartile, and quintile groupings. The lowest group in each classification is omitted. *Post Disclosure* is a binary variable set to one if a firm has disclosed its pay ratio in the current or previous months. Detailed definitions of control variables can be found in Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from 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: Repurchases/MV (%)		
	(1)	(2)	(3)
Post Disclosure $\times$ Ratio Tercile 2	0.0267* (0.014)		
Post Disclosure $\times$ Ratio Tercile 3	0.0439*** (0.015)		
Post Disclosure $\times$ Ratio Quartile 2		-0.0058 (0.016)	
Post Disclosure $\times$ Ratio Quartile 3		0.0317* (0.016)	
Post Disclosure $\times$ Ratio Quartile 4		0.0428** (0.017)	
Post Disclosure $\times$ Ratio Quintile 2			-0.0116 (0.018)
Post Disclosure $\times$ Ratio Quintile 3			0.0189 (0.018)
Post Disclosure $\times$ Ratio Quintile 4			0.0164 (0.018)
Post Disclosure $\times$ Ratio Quintile 5			0.0502** (0.019)
Post Disclosure	-0.0085 (0.015)	-0.0022 (0.016)	0.0002 (0.016)
Observations	30,094	30,094	30,094
R-squared	0.422	0.422	0.422
Controls from Table 2	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes

Table OA4: Investor exit

This table reports descriptive statistics and estimates from cross-sectional regressions of changes in quarterly investor holdings around the mandatory disclosure of the CEO-to-median-employee pay ratio. Panels A, B, and C present summary statistics for KLD ESG scores, investor-level ESG preferences measured in 2017, and firm-level time-varying investor preferences, respectively. These variables are defined in Section 4.1 and are used to measure the change in investor holdings from one quarter prior to the initial pay ratio disclosure to two quarters after the disclosure. Panels B and C use alternative classifications of short-term investors: they are defined as those in the top quartile of the average portfolio churn rate in 2017Q4 in Panel B, and as transient institutions based on Bushee's classification in Panel C. In Panel D, the dependent variables reflect the holdings of transient investors who exhibit social, environmental, or governance preferences, as well as those of investors in general. They are defined as dummy variables indicating whether these investors reduce their holdings from one quarter prior to the initial pay ratio disclosure to two quarters after the disclosure. The regression sample is split based on whether a firm is an above-expected repurchaser, whose cumulative repurchases within the five-month window following the disclosure exceed the predicted level based on pre-disclosure coefficient estimates. *High Ratio* equals one if a firm discloses a pay ratio above the median for the same Fama French 12 industry. This table also reports the *p*-values of *F*-statistics testing the differences in coefficients on *High Ratio* between the respective partitioned subsamples. Detailed definitions of control variables can be found in Table A1. All models include industry and disclosure-month fixed effects. The standard errors in parentheses are clustered at the disclosure-month level. The coefficients on constants are not reported. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Summary statistics for KLD ESG scores

	Obs.	Mean	Lower Quartile	Median	Upper Quartile	Standard Deviation
KLD Social Score	2,665	0.5242	0	0	1	1.1919
KLD Environmental Score	2,665	0.3692	0	0	1	0.7152
KLD Governance Score	2,665	0.1223	0	0	0	0.7275

Panel B: Summary statistics (Churn rate classification)

	Obs.	Mean	Lower Quartile	Median	Upper Quartile	Standard Deviation
<i>Investor preferences</i>						
Social Preference	956	0.4672	0.1754	0.4402	0.7164	0.3713
Environmental Preference	956	0.5437	0.2040	0.4823	0.8280	0.4024
Governance Preference	956	-0.1141	-0.2366	-0.0806	0.0092	0.2246
<i>Firm-level, time-varying investor holdings</i>						
Social Holdings	2,092	0.6194	0.5548	0.6390	0.7020	0.1199
Environmental Holdings	2,092	0.7296	0.6597	0.7470	0.8226	0.1352
Governance Holdings	2,092	-0.1507	-0.1983	-0.1531	-0.1068	0.0665

Panel C: Summary statistics (Bushee's classification)

	Obs.	Mean	Lower Quartile	Median	Upper Quartile	Standard Deviation
<i>Investor preferences</i>						
Social Preference	813	0.4562	0.1457	0.4327	0.7013	0.3765
Environmental Preference	813	0.5312	0.1593	0.4702	0.8035	0.4205
Governance Preference	813	-0.0993	-0.2171	-0.0710	0.0083	0.2218
<i>Firm-level, time-varying investor holdings</i>						
Social Holdings	2,092	0.5746	0.5083	0.5862	0.6500	0.1138
Environmental Holdings	2,092	0.6735	0.6014	0.6861	0.7559	0.1272
Governance Holdings	2,092	-0.1032	-0.1424	-0.1041	-0.0662	0.0569

Table OA2 (Continued)

Panel D: Investor exit (Bushee's classification)

	Drop in Soc Holdings		Drop in Env Holdings		Drop in Gov Holdings		Drop in ST Holding	
	Above-expected Repurchasers	Other Firms	Above-expected Repurchasers	Other Firms	Above-expected Repurchasers	Other Firms	Above-expected Repurchasers	Other Firms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High Ratio	0.0647*	-0.0073	0.0311	-0.0178	0.0493	0.0316	0.0464	-0.0411
	(0.032)	(0.027)	(0.022)	(0.027)	(0.075)	(0.061)	(0.098)	(0.037)
<i>p</i> -value (equal coefficient estimates)	(0.087)		(0.165)		(0.852)		(0.394)	
Observations	294	752	294	752	294	752	294	752
R-squared	0.133	0.082	0.162	0.076	0.158	0.039	0.126	0.127
Controls from Table 4	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Disclosure Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA5: Calendar-time portfolio returns

This table reports monthly calendar-time alphas following the mandatory disclosure of CEO-to-median-employee pay ratios. For each post-disclosure horizon, including the first five months (0–105 trading days), the subsequent five months (106–210 trading days), and the remainder of the year (106–252 trading days), we form four daily equal-weighted portfolios by sorting firms based on their pay ratios (high- versus low-pay-ratio) and repurchase activity (above-expected repurchasers versus other firms). High-pay-ratio firms are those that disclose a pay ratio above the median for the same Fama French 12 industry. Above-expected repurchasers are firms whose cumulative repurchases within the five-month window following disclosure exceed the predicted level based on pre-disclosure coefficient estimates. On each calendar trading day, a portfolio includes all firms whose disclosure-relative trading day falls within the relevant horizon. We calculate the return spread between high- and low-pay-ratio firms separately for above-expected repurchasers and other firms. The reported monthly alpha equals 21 times the daily intercept from a Carhart four-factor regression of the corresponding zero-investment portfolio return on the market excess return, size, book-to-market, and momentum factors. The standard errors in parentheses are 21 times the Newey-West standard errors with five lags. The table also reports the p -values for tests of equality in calendar-time alphas between above-expected repurchasers and other firms. ***, **, and * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Monthly Alpha [0-105]		Monthly Alpha [106-210]		Monthly Alpha [106-252]	
	Above-expected Repurchasers (1)	Others (2)	Above-expected Repurchasers (3)	Others (4)	Above-expected Repurchasers (5)	Others (6)
High Ratio	0.0017 (0.008)	-0.0050 (0.007)	0.0070 (0.008)	-0.0114** (0.005)	0.0226** (0.011)	-0.0018 (0.005)
p -value (equal coefficient estimates)	(0.533)		(0.066)		(0.017)	
Observations	327	327	327	327	369	369

Table OA6: Cross-sectional analyses: Repurchase history

This table reports estimates of triple difference-in-differences regressions of monthly repurchases around the mandatory disclosure of CEO-to-median-employee pay ratio across sample splits based on repurchase history. In Panel A, the dependent variable, *Repurchases/MV*, is measured monthly as the product of the average price paid for the shares repurchased and the number of shares repurchased during the month, scaled by the market capitalization of the previous quarter. In Panel B, the dependent variable, *Repurchase Dummy*, is a binary variable equal to one if a firm repurchases shares during the month. All models examine the 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 if the firm has a recent repurchase history within the 12 months preceding the disclosure (*Repurchased since t-12*), within the 24 months preceding the disclosure (*Repurchased since t-24*), or during the period from the beginning of 2016 up to the disclosure (*Repurchased since 2016*). This table also reports the *p*-values of *F*-statistics testing the differences in coefficients on *Post Disclosure* $\times$ *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 definitions of control variables can be found in Table A1. Following Baker et al. (2022), we adopt a stacked regression approach. Specifically, firms with disclosures in the same year-month are grouped into cohorts and pooled with samples from 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: Repurchases/MV (%)					
	Repurchased since <i>t-12</i>		Repurchased since <i>t-24</i>		Repurchased since 2016	
	Yes (1)	No (2)	Yes (3)	No (4)	Yes (5)	No (6)
Post Disclosure $\times$ High Ratio	0.0456* (0.025)	0.0270** (0.011)	0.0410* (0.022)	0.0259** (0.012)	0.0406* (0.021)	0.0206* (0.012)
<i>p</i> -value (equal coefficient estimates)	(0.503)		(0.543)		(0.402)	
Post Disclosure	-0.0168 (0.030)	0.0157*** (0.006)	-0.0075 (0.026)	0.0114*** (0.004)	-0.0051 (0.024)	0.0104*** (0.004)
Observations	14,924	15,163	17,266	12,821	18,255	11,832
R-squared	0.357	0.236	0.375	0.234	0.381	0.237
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 OA3 (Continued)

Panel B: Repurchase likelihood

	Dependent variable: Repurchase Dummy					
	Repurchased since $t-12$		Repurchased since $t-24$		Repurchased since 2016	
	Yes (1)	No (2)	Yes (3)	No (4)	Yes (5)	No (6)
Post Disclosure $\times$ High Ratio	0.0583* (0.030)	0.0495*** (0.015)	0.0558** (0.026)	0.0413*** (0.015)	0.0537** (0.025)	0.0374** (0.015)
<i>p</i> -value (equal coefficient estimates)	(0.795)		(0.631)		(0.577)	
Post Disclosure	0.0066 (0.038)	0.0252*** (0.007)	0.0171 (0.033)	0.0186*** (0.006)	0.0201 (0.031)	0.0153*** (0.005)
Observations	14,924	15,163	17,266	12,821	18,255	11,832
R-squared	0.446	0.321	0.504	0.351	0.522	0.339
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