Repurchases for Price Impact:

Evidence from Fragile Stocks

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Abstract: We highlight an important but overlooked characteristic of financial fragility: "fragile" stocks command higher liquidity. This reduces their sensitivity to corporate actions with price impact and affects the firms' incentives to engage in such actions. We show that fragile firms have lower share repurchases, issue more equity, and invest more. We establish causality by relating changes in corporate actions to exogenous changes in fragility induced by mergers of asset managers. Our results suggest that financial fragility has direct but unexpected real implications for corporate actions.

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I. Introduction

Following the global financial crisis, the concept of "financial fragility" has attracted considerable attention because it recognizes how the ownership structure of stocks and the liquidity needs of its owners can create non-fundamental price impact, which, in turn, exacerbates financial crises. A stock is considered "fragile" if it is sensitive to non-fundamental liquidity shocks by its owners (Greenwood and Thesmar 2011). This sensitivity is attributed to ownership by investors subject to volatile and correlated liquidity needs and is exacerbated by the ownership of open-ended funds that can be subject to strategic complementarities (Chen, Goldstein, and Jiang 2010; Goldstein, Jiang, and Ng 2017). The key *asset pricing implication* of this is that fragile stocks exhibit higher return volatility because their prices are more sensitive to non-fundamental demand shocks (Greenwood and Thesmar 2011). Moreover, as shown by Massa, Schumacher, and Wang (2021, MSW hereafter), institutional investors take financial fragility into account in their portfolio decisions and their strategic behavior generates a second major asset pricing implication: fragile stocks command higher liquidity precisely because they are subject to many volatile but ultimately non-fundamental (i.e., flow-driven) liquidity shocks.[1] In this paper, we focus on those liquidity characteristics of fragile stocks and investigate the *corporate implications* of stock price fragility, building on the literature that has established the importance of stock liquidity for corporate actions.[2] Specifically, we extend the analysis in MSW

[1] In particular, MSW show how changes in expected financial fragility trigger strategic portfolio rebalancing by institutional investors that lead to changes in the ownership composition of stocks. These changes in ownership composition then impact future asset pricing quantities including realized fragility, volatility and liquidity. For example, an increase in expected fragility leads to a drop in volatile mutual fund ownership but an increase "long-term" institutional ownership (e.g., pension fund or insurance ownership) which results in a realized reduction in financial fragility, volatility, and liquidity precisely because these new owners have different liquidity needs. We give more details and provide supporting evidence in Section IV.A.

[2] Past literature has established links between stock liquidity and corporate actions in the context of innovation (Fang, Tian and Tice 2014), M&A activity (Massa and Xu 2013), share repurchases (Barclay and Smith 1988, Cook, Krigman, and Leach 2004, Hong, Wang, and Yu 2008, Brockman, Howe, and Mortal 2008, Hillert, Maug, and Obernberger 2016), or cash holdings (Nyborg and Wang 2021), among others.

and focus on a set of corporate actions that (a) are known to elicit strong stock price impact and (b) have an established link with stock liquidity: share repurchases. We posit that the high liquidity of fragile stocks discourages share repurchases because high liquidity (i.e., low price impact) attenuates the expected price impact of the repurchase decision. In doing so, we re-visit the long-standing explanation that repurchases are a signaling tool: firms seek to buy back shares to signal undervaluation. If so, firms will repurchase their stock when it is illiquid to amplify the price impact of the repurchase.[3] Therefore, *if the goal of the repurchase is to signal and maximize the stock price,[4] changes in fragility will change the incentives to repurchase shares.* From this hypothesis, we formulate and test five empirical predictions. First, repurchase activity is negatively related to fragility and liquidity. For example, a reduction in fragility (i.e., liquidity) leads to an increase in share repurchases. Second, we expect a pronounced effect for those repurchase activities that are known to elicit larger stock price impact, i.e., tender offers. Third, fragility-driven repurchases are generally associated with increased stock price impact but not with improved stock liquidity. Fourth, if a higher price impact encourages share repurchases, it should also discourage share issuances. Fifth, absent sizeable cash holdings or changes in leverage to finance repurchases, an increase in repurchases is financed at the expense of corporate investment as firms prioritize the positive stock price impact of repurchases over investment. In other words, our argument ultimately predicts a positive relationship between fragility and investment.

[3] See Dann (1981), Vermaelen (1981), Comment and Jarrell (1991), Lee, Mikkelson, and Partch (1992), Grullon and Ikenberry (2000), Louis and White (2007) and others. Ofer and Thakor (1987) argue that the higher the cost for the manager to execute the repurchase, the stronger is the credibility of the signal. Brennan and Thakor (1990) claim that share repurchases have a cost as "the investor bears an information cost or else loses the ownership to a better-informed shareholder."

[4] There could be many reasons why firms seek to maximize stock price impact, including incentives to increase executive compensation, ability to raise cheaper equity financing, increasing bargaining power with lenders, marketing strategies geared to increase awareness with customers through a higher stock price, existence of ESOPs, desire to use equity to engage in an equity-based M&A, and signaling power to the competitors, etc.

We contrast our reasoning against the competing hypothesis that firms repurchase shares to "time the market": firms buy back shares when they are perceived to be undervalued not to signal such undervaluation but to benefit from it by repurchasing shares at a discount. In contrast to the signaling alternative, this motive will induce firms to repurchase when the stock is liquid as liquidity minimizes the market impact and transaction costs of the repurchase. Furthermore, market-timing leads to a preference for open market repurchases, no or even lower price impact of repurchase announcements but potentially a positive impact on stock liquidity.

The key challenge to testing these predictions is to establish the direction of causality because the involved quantities – stock price fragility, stock liquidity, and share repurchases – are jointly determined. We directly extend the analysis in MSW and rely on the natural experiments of mergers between asset management firms that lead to exogenous *reductions* in stock price fragility because they induce changes in the ownership base of affected stocks: away from existing owners with volatile liquidity needs and towards new "stable" owners. These changes lower both stock price fragility and liquidity.[5] We focus on the firms most heavily affected by these mergers and match these "treated" firms with "control" firms from the lowest quintile of merger-affected firms.

We start by documenting that for the global equity universe, stock price fragility in the sense of Greenwood and Thesmar (2011) is associated with higher stock liquidity and fewer share repurchases: the quintile of most fragile (and liquid) stocks execute 56% fewer share repurchases compared to the average stock in the sample. Complementing these full-sample results with an event-based difference-in-difference analysis around mergers of asset management firms, we show that treated firms increase their overall payout by 22% relative to control firms in the two

[5] We explain the mechanism behind this established result in more detail in Section VI.A.

years following the mergers. The increase in payout is (i) detectable only after the merger events but not before, suggesting that the parallel trend assumption is unlikely to be violated, and (ii) driven exclusively by increases in stock repurchases with no effect on dividends.

Next, we test our argument that this increase in "fragility-induced" repurchases is driven by the motive to seek stock price impact. First, conditioning on firms that make a repurchase announcement, we find that treated firms are almost twice as likely to execute their repurchases via the most signaling-sensitive type (i.e., tender offers) relative to the unconditional sample average, consistent with a signaling explanation (e.g., Louis and White 2007) but difficult to reconcile with the market timing alternative (e.g., Brockman, Howe, and Mortal 2008). Second, we find no evidence that these repurchases improve stock liquidity across different measures and horizons but instead generate elevated price impact. In fact, we document that the repurchase announcements of treated firms register both more positive short- and long-run valuation effects compared to control firms: short-run (long-run) CARs are between 0.5% and 1.0% (4.4% to 7.0%) higher around the repurchase announcements for treated versus control firms, again favoring a price-impact over a market-timing motive.

The same motive also affects other financing policies beyond repurchases: treated firms experience a stronger reduction in equity issuances relative to control firms but otherwise register no changes in other financing activities, including changes in leverage and debt issuances. There is also no evidence of changes in cash holdings, making it unlikely that firms repurchase shares to return excess cash to shareholders. Instead, we find that treated firms reduce CAPEX by 6% relative to control firms, consistent with the substitution between cash spent on repurchases and cash used for investment documented in a different context by Almeida, Fos, and Kronlund (2016).

We provide extensive additional discussion and address alternative explanations. For example, we provide complementary evidence from another natural experiment: the 2016 Tick Size Pilot Program. We also investigate why firms seek to maximize their stock price via repurchases and find results consistent with executive compensation-related motives: the increase in repurchase activities is pronounced for firms with growth options and high issuance of equity-based executive compensation. Furthermore, CEOs of treated firms are more likely to sell shares and exercise some of their vested options relative to CEOs of control firms in the post-event periods. We further examine cross-sectional heterogeneity across jurisdictions with different degrees of price and volume constraints in corporate repurchase programs and find that, outside the US, price regulations on repurchase programs attenuate our results. For US firms, characteristics such as firm size and return volatility also attenuate or magnify our results. We also discuss several alternative explanations for our findings, including changes in the firm's cost of capital, changes in the level of institutional ownership, or changes in firm governance that could follow our natural experiments but find no support for those.

Finally, we consider alternative empirical designs, an alternative definition of our treatment variable, or an instrumental variable approach. All these alternative approaches support our hypothesis. Extensive robustness tests also show that our results are not driven by biases that can arise in difference-in-difference estimations with staggered events and possibly heterogeneous treatment effects (e.g., Callaway and Sant'Anna 2021; Sun and Abraham 2021) or by multiple testing biases because we reuse MSW's experimental setting (Heath, Ringgenberg, Samadi, and Werner 2023).

We make several contributions. First, we provide direct evidence that financial fragility has real implications by driving corporate actions including share repurchases, equity issuances, and

investment. Key to this contribution is the observation that while "fragile" stocks are known to be more volatile (Greenwood and Thesmar 2011), the same forces that increase return volatility also improve stock liquidity which is known to affect corporate policies. Our paper is the first to document that fragility may encourage corporate investment because it reduces firms' incentives to repurchase their own stock. This is a strong and surprising result because the high volatility of fragile stocks can be expected to lead to a negative relationship between fragility and investment (e.g., Panousi and Papanikolaou 2012). Our natural experiments of mergers between asset management firms that induce exogenous reductions in *realized* fragility allow us to examine the competing effects of both volatility and liquidity on investment and payout and to examine the causal link between fragility and corporate behavior. As such, we relate to a contemporary paper by Friberg, Goldstein, and Hankins (2024) examines precautionary short-term changes in cash holdings for US firms that are driven by *expected* (but yet *unrealized*) changes in fragility.

Second, we contribute to the literature on share repurchases. Of the many explanations that have been put forward to explain share repurchases, "signaling" and "market timing" stand out (but are certainly not the only ones).[6] Our experimental setting structured around exogenous changes of fragile stocks allows us to both provide a fresh look at this broader debate and more specifically to connect to extant literature that examines the link between repurchases and stock liquidity. Several studies argue for a positive link between repurchases and stock liquidity either because firms seek to exploit liquidity to time the market or, at times, repurchase shares to create or positively affect liquidity.[7] Recently, Nyborg and Wang (2021) identify a repurchase motive behind the empirical observation that improved liquidity increases corporate cash holdings.

[6] We refer to literature reviews of Allen and Michaely (2003), DeAngelo, DeAngelo and Skinner (2008), or Fare-Mensa (2014) for more comprehensive discussions.

[7] See Cook, Krigman, and Leach (2004), Chung, Isakov, and Perignon (2007), Hong, Wang, and Yu (2008), De Cesari, Espenlaub, and Khurshed (2011), McNally and Smith (2011), Hillert, Maug, and Obernberger (2016).

Other studies find that share repurchases negatively affect liquidity (e.g., Barclay and Smith 1988, Ginglinger and Hamon 2007, Brockman and Chung 2011) because of adverse selection. In contrast, we consider a different motivation (i.e., the desire to increase the stock price) and show a new result: firms may repurchase for price impact, leading to a negative relationship between repurchases and liquidity. Such repurchases are more prominent for firms with growth options and high equity-linked executive compensation and firms show a higher propensity to conduct tender offers to maximize the stock price impact of repurchases. This is consistent with past literature documenting positive relationships between repurchases and insider selling (Bonaime and Ryngaert 2013), executive or employee stock options (Fenn and Liang 2001; Kahle 2002), or employee equity stakes (Babenko 2009).[8]

These results raise the question whether our findings imply that fragility-driven repurchase lead to overvaluation or deviations between stock prices and fundamental value. In that respect, Busch and Obernberger (2017) document that repurchases improve stock price efficiency and do not lead to misvaluation. Our results are in line with theirs: we document elevated price impact of fragility-driven repurchases not only in the short-run but up to a 2-year horizon following the repurchase announcement. In other words, the lack of evidence of price reversals suggests that fragility-driven repurchases are consistent with long-run value maximization and our signaling hypothesis. In any case, our focus is different as we focus on whether fragility drives repurchases, not vice versa.

The potential tension between our and established results lies in the broader question of how liquidity affects repurchases. Studies on repurchases and liquidity frequently restrict themselves to open market repurchases and/or samples of repurchasing firms and focus on the

[8] It also resonates with past literature showing that CEOs strategically time corporate news releases (Edmans, Goncalves-Pinto, Groen-Xu, and Wang 2018) or firm advertising (Lou 2014) to positively affect stock prices.

implementation of repurchase programs to infer the relationships between repurchase activity and stock liquidity (sometimes from a microstructure perspective, as in Hillert, Maug, and Obernberger, 2016). Our analysis is broader: we use the global universe of stocks and include both repurchasing and non-repurchasing firms as well as all repurchase types, allowing us to investigate how liquidity affects the decision to repurchase (and if so, under what form).[9] Third, we contribute to the literature on how opportunistic behavior in the stock market shapes corporate policies by showing how changes in financial fragility shift firms' incentives to repurchase shares which then has ramifications for corporate investment and equity issuances. These findings are consistent with theories of capital budgeting in which managers maximize the stock price (e.g., Stein 1996; Baker, Stein, and Wurgler 2003) and related empirical evidence (e.g., Almeida, Fos, and Kronlund 2016; Wang, Yin, and Yu 2021).

II. Data & Main Variables

Our main data set relies on two primary data sources. First, we collect firm and stock information for the global universe of listed firms from the Worldscope and Datastream databases. From Worldscope, we collect firm-level accounting information on share repurchases, dividend policy, capital expenditure as well as several other balance sheet and income statement items (e.g., total assets, book equity, leverage, cash holdings, and others). From Datastream, we collect information on stock prices (including bid and ask prices), returns, and trading volume for this global sample of firms. To compute stock return volatility and Amihud's (2002) measure of price impact, we rely on daily volume, price, and return data but we also collect those items at the monthly frequency for some control variables. As in MSW, we apply the filters suggested by

[9] Brockman, Howe, and Mortal (2008) examine how changes in market liquidity affect repurchase decisions in the cross-section of US firms (but not the time series) and do not address the endogeneity between these quantities.

Ince and Porter (2006) to the data collected from Worldscope and Datastream. We convert all accounting, price, and return information into US$ terms.

The second main data source is the FactSet Ownership Database from which we collect data on institutional ownership. For each firm, we download stock holding information to construct annual measures of institutional ownership for all institutions and fund types, including open-end funds, insurance funds, closed-end funds, and other types.

Most variables we employ are standard measures of corporate payout, investment, and other firm characteristics and policies, including firm size, book-to-market, cash flow, total institutional ownership, age, cash holdings, leverage, and others. For brevity, we present a complete list of variables and their definitions in Appendix A.

To capture the key concept of "financial fragility," we estimate the stock-level measure of fragility developed by Greenwood and Thesmar (2011) for the global universe of firms using the holdings and flows of all fund types in FactSet. While Greenwood and Thesmar (2011) construct the measure of stock price fragility from holdings and flow information of open-end funds only, we follow MSW who emphasize that changes in ownership composition can have important effects on stock price fragility. For example, MSW show that the rebalancing of open-end funds leads to changes in the composition of ownership that cannot be captured by relying exclusively on mutual fund holdings and flow information (thereby excluding all the other fund types).

To be included in the sample, we require firms to have non-missing information for stock-level fragility, liquidity, and volatility as well as the main corporate outcome variables for share repurchases, capital expenditures and the control variables. This delivers a final firm-year panel with 61,123 observations attributable to 12,722 individual firms for the sample period from 2002

to 2012. Our sample period is dictated by the global sample of asset management mergers that we describe in detail in Section IV.

Table 1 presents the summary statistics for this global sample. In the average firm-year, actual share repurchases amount to 1.1% of book assets and capital expenditures amount to almost 6% of book assets. The average firm has institutional ownership of almost 33% and is a growth firm with a book-to-market ratio smaller than 1, consistent with other corporate finance studies that use a global universe of listed firms (e.g., Pinkowitz, Stulz, and Williamson 2015).

Table 1 here

III. Fragility, Liquidity, and Repurchases: Motivating Evidence

We begin our analysis with simple motivating panel regressions to highlight key empirical associations that are implied by our argument on repurchases for price impact. We start with the relationships between fragility, volatility, and liquidity and estimate the following specification for our firm-year panel:

$$(1) \qquad Y_{ft+1} = \beta\sqrt{Fragility_{ft}} + \gamma Controls_{ft} + \alpha_c + \alpha_i + \alpha_t(+\alpha_f) + \epsilon_{ft+1},$$

where Y_{ft+1} measures outcome variables (including return volatility, liquidity, repurchases, or investment), $\sqrt{Fragility_{ft}}$ is the lagged square root of stock price fragility as in Greenwood and Thesmar (2011) but computed using holdings and return information from all funds holding firm *f* in Factset in year *t* and the vector $Controls_{ft}$ includes additional firm-level controls. We present specifications with fixed-effect effects for the primary listing country *c* of firm *f* (denoted by α_c), the industry affiliation *i* of firm *f* based on the Datastream global industry classification (denoted by α_i), year *t* (denoted by α_t), and in some specifications firm fixed effects (denoted by α_f), and cluster standard errors at the firm level.

Table 2 presents these first estimates. To relate to the original study of Greenwood and Thesmar (2011), we first replicate the already-established effect of fragility on return volatility. Columns

Table 2 here

1 to 3 document that fragile stocks exhibit more volatile returns, the effect robust across specifications. In the remaining columns, we replace the dependent variable and use Amihud's (2002) measure of price impact in columns 4 to 6 or average daily bid-ask spreads in columns 7 to 9. The first element in our argument posits that fragile stocks, while more volatile, should also exhibit lower price impact (i.e., "higher liquidity"). Therefore, we expect fragile stocks to have a lower measure of price impact and lower bid-ask spreads. The results in columns 4 to 9 of the same table support this reasoning. Across specifications, we find fragile stocks to be associated with both a lower level of Amihud's (2002) price impact and lower bid-ask spreads. In terms of economic impact, we find that a 1 standard deviation (STD) increase in fragility is both related to a 3.3% of a STD increase in volatility (column 2), a 6.5% of a STD decrease in price impact (column 5), and a 3.1% of a STD decrease in bid-ask spreads (column 8).[10]

Table 3 here

In Table 3, we turn to our argument that firms are not only aware of the consequences that financial fragility has on their stock prices but also that it in fact influences corporate policies. Columns 1 to 3 of Table 3 present the same specifications as in Table 2 but now replace the dependent variable with the first corporate policy of interest: share repurchases. We predict that firms with fragile stocks should engage in fewer share repurchases because the expected positive effect of share repurchases is muted due to their higher fragility. Columns 1 to 3 confirm that this is indeed the case, even after controlling for a large number of corporate characteristics that are expected to affect repurchases (including growth opportunities, cash flows, dividend policy, or institutional ownership). The estimate in column 2 suggests that a 1 STD increase in fragility is associated with a 4.2% of a STD reduction in share repurchases. Specifications in the remaining

[10] We present additional robustness tests to these first motivating regressions in the Internet Appendix, Table IA.1. For example, when we transform the fragility measure into quintiles, we find that the difference in volatility (Amihud, spreads) between stocks in the highest and lowest fragility quintile amounts to 12% (32%, 16%) of a STD of these variables.

columns 4 to 6 of Table 3 go one step further and show that fragile firms also invest more. Using capital expenditures as the dependent variable, we find in column 4 that a 1 STD increase in fragility is associated with a 4.0% of a STD increase in capital expenditures.[11]

IV. Fragility, Liquidity, and Repurchases: Causal Evidence

A. Empirical Design

The central challenge in our argument is establishing the direction of causality. We posit a causal relationship between changes in fragility and corporate policies – in particular, share repurchases and later share issuances and capital expenditure/investment – because changes in fragility directly imply changes in stock liquidity that affect the expected price impact of corporate actions and hence shift corporate incentives to spend cash where the expected stock price impact is largest. Clearly, this argument is centered on quantities that are all jointly determined. It is for this reason that we have labeled the results presented in Section III as "motivating evidence" – all the tests presented in Tables 2 and 3 are subject to reverse causality concerns.

To address this issue, we rely on established natural experiments that lead to exogenous changes in stock price fragility. We build on prior work in MSW who use mergers between asset management firms as experiments that lead to exogenous changes in financial fragility at the firm level. MSW establish that asset management mergers happen for reasons that are exogenous to the portfolio holdings of the affiliated buyer and target funds. This is a key observation as it validates the exclusion restriction that these mergers do not happen in anticipation of future changes in corporate policies. MSW show that these natural experiments lead to significant

[11] We again present robustness tests of these motivating regressions in the Internet Appendix, Table IA.1. For example, when we transform the fragility measure into quintiles, we find that the difference in repurchases (CAPEX) between stocks in the highest versus the lowest fragility quintile amounts to 19% (18%) of a STD of these variables. Put differently, share repurchases (CAPEX) of stock in the highest fragility quintile are 57% (26%) lower (higher) compared to the average stock in our sample.

portfolio rebalancing by existing shareholders, in particular by open-end funds with volatile and correlated flows. This group of shareholders rebalances away from stocks with large pre-merger portfolio overlap in buyer- and target-affiliated funds are replaced by new shareholders with less volatile flow characteristics, especially by funds without open-end structures that trade less. In other words, these mergers induce changes in the composition of ownership of affected stocks that are conducive of lower stock price fragility, volatility, and liquidity in the post-merger periods. For this reason, we expect that the same events lead to an increase in share repurchases because lower fragility implies lower liquidity and a higher price impact for share repurchases.

To implement our empirical strategy, we obtain the global sample of mergers between asset management firms. This sample was first presented by Luo, Manconi, and Schumacher (2023) and is also employed in MSW. We include all mergers in our sample for which we have holdings information of both buyer and target funds in the year prior to the merger completion date. For those mergers, we include all stocks held by at least one buyer- or target affiliated fund in FactSet in the year prior to the merger completion and for which we have complete information on the main dependent and explanatory variables over the 4-year event window for each merger. This event window is centered on the year in which the merger completes and includes the 2 years before and the 2 years following the merger year. Because our outcome variables are measured at the annual frequency, we exclude, for each deal, the year in which the merger completes to avoid confounding the pre- and the post-event periods. This is important because MSW show that their results on portfolio rebalancing and changes in e.g., liquidity or fragility, are concentrated in the periods between merger announcement and completion with no additional effects after. In other words, we set out post-event periods such that they only start after all changes to fragility and liquidity have been realized and concluded. These inclusion restrictions

deliver a sample of 77 different mergers between asset management firms where funds have positions in 6,008 different stocks over the period from 2002 to 2012.

From this sample, we construct our treatment and control groups. First, we conjecture that these financial market effects will have a real impact on corporate policies, especially for the firms most heavily affected by these mergers – i.e., the firms with the highest level of pre-merger portfolio overlap between buyer- and target-affiliated funds – because MSW show that these firms experience the strongest changes in their ownership composition and, as a consequence, the strongest reductions in fragility and liquidity. As in MSW, we measure this portfolio overlap via the hypothetical increase in ownership concentration induced by the merger using the pre-merger holdings of buyer- and target-affiliated funds. Specifically, we define $Treat_{fd} = 1$ for all firms that fall in the top quintile of $\left(IO\ Acq_{fd} + IO\ Targ_{fd}\right)^2 - IO\ Acq_{fd}^2 - IO\ Targ_{fd}^2$ and 0 otherwise, where $IO\ Acq_{fd}$ ($IO\ Targ_{fd}$) is the combined holdings of all funds affiliated with the acquirer (target) asset management firm scaled by shares outstanding as in MSW.[12]

Second, we select a "control" firm from the lowest quintile of the same treatment measure for each "treated" firm based on their key characteristics one year prior to the merger completion. Matching metrics include country and industry affiliation, log of total assets, log of book-to-market ratio, cash flows, and institutional ownership given that pre-merger ownership is correlated with such observable firm characteristics (MSW 2021). We implement one-to-one "nearest neighbor" propensity score matching and allow for sampling with replacement.[13]

[12] As an alternative treatment definition, we set $Treat_{fd} = 1$ for firms that fall in the top quintile of combined pre-merger buyer- and target-ownership and we show robustness for our main results using this alternative treatment definition and results from a direct sorting of firms based on the realized reductions in fragility in the Internet Appendix.

[13] The matching algorithm minimizes the Mahalanobis distance across all matching characteristics by selecting the closest neighbor as a match. Specifically, for each treated firm *i*, a matched firm *j* is such that the Mahalanobis distance is given by: $||X_i - X_j|| = ((X_i - X_j)' W_x^{-1} (X_i - X_j))^{1/2}$, where X is a k-dimensional vector of covariates and W_X^{-1} is the inverse of the covariance matrix of the covariates.

We implement this approach because preliminary tests, reported in the Internet Appendix, Table IA.2 show that treated firms exhibit different stock characteristics than the remaining stocks likely because institutional investors exhibit preferences for specific stock characteristics. As such, some firms are more heavily held in the portfolios of both buyer- and target-affiliated funds. Such stocks tend to be older and larger-cap stocks with high institutional ownership. Since many of these characteristics can be expected to affect corporate policies, including share repurchases and capital expenditures, we implement one-to-one "nearest neighbor" propensity score matching. The matching process delivers a final sample of 2,291 stock pairs, including 1,673 unique treated stocks and 1,258 unique control stocks. This final sample constitutes only 46% of the starting sample of treated and control stocks precisely because of the differences in observables between those stocks in the full sample. However, as Column 2 of Panel A in Table IA.2 confirms, observable characteristics (including some characteristics not included in the matching exercise) no longer predict the treatment status in our final matched sample. In Table 4, we present the customary tests of equality of means in observables between treated and control stocks both before and after propensity score matching. As expected, there are significant differences in observables between treatment and control firms before but no longer after the propensity score matching.

Table 4 here

Our main empirical specification is a difference-in-difference estimation at the annual frequency for treatment and control firms of the following form:

$$Y_{fdt} = \beta_1 T_{fd} + \beta_2 Post_{dt} + \beta_3 T_{fd} \times Post_{dt} + \gamma_1' X_{ft-1} + \gamma_2' \left(Post_{dt} \times X_{ft-1}\right) + \alpha_t + \alpha_f + \alpha_d + \epsilon_{fdt,} \quad (2)$$

where Y_{fdt+1} measures several outcome variables for firm *f* affected by deal d in year *t*, T_{fd} is the treatment indicator that equals 1 if firm *f* is in the top quintile of the treatment variable for

deal *d* and 0 otherwise, $Post_{dt}$ is an indicator equal to 1 for the years following the completion of deal *d* and 0 otherwise as well as firm-level control variables and the interaction with the post-indicator to control for any residual effect observables could have on firm outcomes in the post-merger periods. We include year, firm, and deal fixed effects (denoted by α_t, α_f and α_d respectively) and draw inference from firm-clustered standard errors.

Given that our focus is on corporate policies, we include an event window of 4 years around each merger event. This spans the 2 years prior to the year in which deal *d* completes and the 2 years following the deal completion (and excluding the year in which the deal completes). The main coefficient of interest in equation (2) is β_3, the coefficient on the interaction term between the post and treatment indicators. We interpret this coefficient as the causal effect of changes in financial fragility on the outcome variable.

This interpretation hinges on the identifying parallel-trend assumption in the outcome variable. MSW already show that this parallel trend assumption likely holds in their setting and we will provide additional validation tests for the outcome variables we consider. In addition, we highlight that our sample construction further addresses any remaining concerns in this respect. For example, similarities in observable characteristics between treatment and control stocks mitigate omitted variable concerns that changes in corporate policies are ultimately driven by differences in firm characteristics rather than differences in the treatment status. This increases the likelihood that the parallel trend assumption is met. In this respect, we highlight the last 2 rows of Table 4. They show no significant differences in the dynamics of share repurchases or CAPEX for treated versus control in the pre-event period. We will provide further tests as we discuss our main findings including a more extensive discussion on possible biases from heterogenous treatment effects in staggered difference-in-difference designs in Section IV.C.

Before estimating equation (2), we provide two validation tests that treated firms indeed experience a reduction in *realized* financial fragility and liquidity in the post-merger periods relative to control firms and undergo the same changes in the composition of their institutional ownership (a drop in mutual fund ownership compensated by an increase in long-term institutional ownership) as documented in MSW. As these results are not new (see MSW), we present them in the Internet Appendix, Table IA.3. Beyond confirming that the original findings hold in our sample, these results are important for the interpretation of our results in Section V that analyze repurchase announcements because it confirms that any repurchase announcement in the post-merger periods occur after any drop in realized fragility or liquidity.

B. Main Result: Changes in Share Repurchases

Having laid out our empirical strategy, we seek to substantiate the motivating results from Section III via difference-in-difference estimates. We begin with the effect on share repurchases and estimate equation (2) with the dependent variable $Repurchase_{ft}$. Table 5, Panel A presents the results. Column 1 starts with the simplest specification with only the key explanatory variables $Treat_{fd}$, $Post_{dt}$, and the interaction term between the two. We immediately find a positive and significant effect – treated firms register more share repurchases in the post-merger periods compared to the control firms. The point estimate in column 1 of 0.0056 on the interaction term, significant at the 1% level, suggests that treated firms increase their actual share repurchases by 0.56% of total assets relative to control firms. To put this estimate into economic perspective, both treated and control firms, on average, repurchase shares worth 2.6% of book assets per year prior to the merger event. Therefore, an increase of 0.56% represents a 22% increase in repurchase behavior, which is an economically sizeable effect.

Table 5 here

In the remaining columns, we successively add control variables and the various fixed effects to the specification until we arrive at the fully saturated difference-in-difference estimate as specified in equation (2) in column 5 of the table. Across these specifications, we find the difference-in-difference estimate to be remarkably stable – the point estimate fluctuates very little and remains at 0.0055 in column 5, almost identical to the simple estimate in column 1.[14] In column 6, we provide additional evidence that the parallel trend assumption is likely met in our empirical setting. We decompose the $Post_{dt}$ indicator into individual indicators for each event year and then include event-period indicators for each year separately: $Before1_{dt}$ is the year prior to the deal completion date of deal d, $After1_{dt}$ is the first year following deal completion year, and $After2_{dt}$ is the second year following the deal completion year. The only omitted year to avoid collinearity is the year 2 prior to the deal completion. The estimates in column 6 show no significant change in share repurchases in the year prior to the merger but a sharp increase in the 2 years after – the coefficients on the interaction terms between $After1_{dt}$ and the treatment indicator and $After2_{dt}$ and the treatment indicator are positive and statistically significant but the interaction term between $Before1_{dt}$ and the treatment indicator is insignificant, suggesting that the parallel trend assumption is not violated.

In Panel B of Table 5, we provide a falsification test to support our argument that firms opportunistically take advantage of changes in financial fragility and price impact when they operate in the stock market. We repeat the same specifications as in Panel A but use the dividend payout of the firm as a dependent variable. We postulate that the change in fragility shifts incentives towards repurchases to take advantage of current stock market conditions. Share

[14] We also note that our difference-in-difference estimates appear stable across specifications with and without controls, which mitigates concerns about confounding effects from endogenous control variables highlighted in e.g., Hillert, Maug, and Obernberger (2016).

repurchases are known to be more opportunistic than dividend increases because the latter, while creating a positive stock price impact, also creates an expectation for higher future dividends that may be undesirable in this context. Moreover, increased price impact will be better exploited by corporate actions that are expected to have a bigger impact on the demand curve, and the relatively larger size and lumpiness of share repurchases make them the ideal candidate. Indeed, when we repeat our estimation using changes in dividends, we find no significant difference between treatment and control firms in dividend policy – the difference-in-difference estimates are statistically insignificant throughout Panel B of Table 5.

C. Robustness of Main Result

We discuss robustness of our main result along two dimensions. First, recent literature in econometrics highlights possible biases that can arise in staggered difference-in-difference designs, especially in the presence of heterogeneous treatment effects that can confound the causal interpretation of estimates in the standard two-way fixed effect specification (e.g., Callaway and Sant'Anna (2021), Sun and Abraham (2021)). Beyond assessing possible violations of the parallel trend assumption via a decomposition of our event-time indicators into event-year indicators, we implement and discuss additional tests and different estimators, in the Internet Appendix, Table IA.4. None of those suggest that possible biases from heterogenous treatment effects in staggered difference-in-difference designs affect our main result.

Second, our main results in Table 5 are based on the same sample of natural experiments as in MSW but focus on how treatment status affects a different corporate outcome, particularly share repurchases. Heath et al. (2023) argue that such "reusing" of natural experiments creates a multiple testing problem. As a result, these authors recommend different cut-off values for *t*-statistics as well as additional remedies. We discuss how our *t*-statistics compare vis-à-vis their

recommendation. For example, the smallest *t*-statistic we report in Table 5, columns 1 to 5 is 3.84 (our column 4) which is larger than the maximum recommended cut-off of 3.69 reported in Heath et al. (2023). We also implement and discuss additional tests and present evidence from another natural experiment – the 2016 Tick Size Pilot Program – in the Internet Appendix, Table IA.5. Overall, this shows that possible multiple testing biases are unlikely to drive our conclusions.

V. Repurchases for Price Impact

In this section, we provide direct evidence that the increase in repurchases of treated firms is driven by an increase in repurchases that seek to maximize stock price impact.

A. Price Impact: Tender Offers versus Open-Market Repurchases

Our argument contrasts with the existing literature that argues for a positive relationship between liquidity and repurchases for good economic reasons: firms repurchase shares to "time the market" and benefit from the current undervaluation of their stock. If so, firms seek to repurchase stock when it is liquid to minimize the price impact of these repurchases. Likewise, some firms may seek to create or improve liquidity by repurchasing shares. Our argument posits that repurchases can be conducted for alternative reasons, namely, to create a positive stock price impact. This repurchase motive is rooted in the firm's desire to "signal" undervaluation and the most effective, costly, and credible way to accomplish this is to repurchase illiquid stock.

We now seek to clarify this tension by first examining differences in the type of repurchases that treated firms execute and second the liquidity and valuation effects of these repurchase announcements. To do so, we collect all details on the repurchase programs of treated and control firms over the sample period from Thomson SDC, including the announcement and implementation dates and the type of repurchase programs. The idea is simple: under our

hypothesis, if treated firms seek to maximize the price impact of repurchases, we should see not only more repurchases of treated firms but also more repurchases that are conducted through tender offers as these are well known to have the highest share price impact, especially compared to open-market repurchases.[15] Overall, we identify 4,784 repurchase announcements in our sample. At the same time, and consistent with prior studies that examine tender offers (Grullon and Ikenberry 2000), only about 2.3% of these repurchase announcements are tender offers. We nevertheless start by focusing on the choice of tender offers and relate the propensity of treated firms to execute their repurchases via tender offers to the key explanatory variables $Treat_{fd}$, $Post_{dt}$, and the interaction term between the two as well as control variables. The dependent variable $Tender\ Offer_{fdt}$ is an indicator equal to 1 if the repurchase announcement of firm *f* in deal *d* and year *t* is a tender offer repurchase and 0 otherwise.

We report the results in the Internet Appendix, Table IA.6. Across all specifications, we estimate that treated firms are between 1.5% and 2.5% more likely to announce a tender offer repurchase than control firms. Given that only 2.3% of repurchases are carried out via tender offers, this is about double the unconditional mean and much larger than the 22% increase in overall repurchases documented in Table 5. So, we do not argue that the few tender offers alone drive our results. Nevertheless, the result supports our hypothesis and stands in contrast to the extant literature because if market-timing were the motivation behind the increase in repurchases we document, tender offers should be less likely for treated firms.

[15] We do not separate the tender offers further into fixed price or dutch auction tender offers as it is established in the repurchase literature that both types of tender offers generate stronger stock price responses relative to open market repurchases (e.g., Comment and Jarrell 1991, Kamma, Kanatas and Raymar 1992, Peyer and Vermaelen 2009).

B. Liquidity Effects

While the extant literature appears to favor a positive relationship between stock liquidity and repurchases, the same literature has also found that at times, firms repurchase shares to provide liquidity. Perhaps our results pick up such instances in which case the increase in repurchases we document should improve stock liquidity. We directly test this interpretation by examining changes in stock liquidity around repurchase announcements. We consider two measures of stock liquidity (Amihud's (2002) price impact measure and average daily bid-ask spreads) as well as alternative horizons (1-, 3-, 6-, or 12-months around the repurchase announcement) over which to measure changes in stock liquidity.

We present the results in Table 6. We find no evidence that the repurchases we document improve stock liquidity across several measures and several horizons – all our difference-in-difference estimates in Table 6 are either insignificant or, when weakly significant, carry the wrong sign. Complementary tests in Table IA.7 that use *Turnover* or *Log(Trading Volume)* as alternative liquidity measures confirm this. In other words, if anything, we find weak evidence of further deteriorating liquidity which may be consistent with a negative effect of informed trading on stock liquidity but is certainly inconsistent with firms providing liquidity.

Table 6 here

C. Short- and Long-Run Valuation Effects

We now focus on the price reaction to the repurchase announcements of treated versus control firms. We expect announcement returns to be more positive for treated relative to control stocks, at least in the short run. But overall, if treated firms repurchase stock over extended periods of time or in general adjust their corporate policies to positively affect their valuation in the stock market, we might even expect a long-run positive stock performance of repurchase announcements of treated relative to control stocks.

Therefore, we analyze the short- and long-term returns following the repurchase announcements. We compute abnormal stock returns of treated and control stocks at the daily frequency relative to different benchmark portfolios and then compound them to cumulative abnormal returns (CARs) over several event windows. For the short-run CARs, we consider the 1-day abnormal return on the announcement date and the 3-day CARs centered around the announcement day (i.e., from trading days -1 to +1 inclusive). For the long-run CARs, we consider the 12- and 24-month CARs starting on the first trading day of the month after the month of the repurchase announcements. As benchmarks for the CARs, we consider (i) a local market benchmark, (ii) a local industry benchmark, or (iii) a local size-value-momentum benchmark, all defined in the variable appendix. Abnormal daily returns are then simply the difference between the return of the stock and the return on the benchmark. For each treated and control stock in our sample, we compute these different CARs for each repurchase announcement and analyze the differences via the same panel specification as before.

Table 7 here

We report the results in Table 7. Panel A examines the short-run CARs immediately around the share repurchase announcements. We find that the repurchase announcements of treated firms that occur in the post-merger periods generate a significantly higher short-run stock price impact – the coefficient for $Post_{dt} \times Treat_{df}$ is positive and significant across all benchmark and event window specifications and the estimates imply an additional positive announcement effect between 0.5% and 1.0%. Panel B of the same table repeats the test but uses the 12- or 24-month CARs as the dependent variables to examine the long-run valuation effects of these repurchase announcements. We again find those repurchase announcements of treated firms register a significantly stronger long-run valuation effect when announced in the post-merger periods – the

effect amounting to between 4.4% and 7.0%, depending on the measurement horizon and the benchmark correction.

We further complement the analysis in Table 7 in two ways. First, in Table IA.8, we find that this positive short-run and long-run effect is particularly pronounced for (but not exclusively attributable to) tender offer announcements of treated firms in the post-merger periods. Second, we collect more granular data on actual repurchase days when firms are active in the market for their stock versus non-repurchase days to more clearly isolate that price impact is concentrated on days in which firms actually repurchase shares. We describe these additional tests in more detail in the Internet Appendix, Table IA.9. We find that positive abnormal returns for treated firms are entirely concentrated in actual repurchase days with no effect on non-repurchase days, substantiating that fragility-induced repurchases are associated with more price impact.

D. Effects on Other Financing Policies

Proceeding to the next prediction, we examine other financing policies that may be affected as well by those same considerations. We start with the "twin policy" to share repurchases: equity issuances. Under our argument, the increased price impact that comes with the reduction in financial fragility should not only incentivize share repurchases but also reduce the firm's desire to issue equity – the low liquidity would make the negative price impact of an equity issuance larger. We further include in this test other financing policies including changes in cash holdings, leverage, and debt issuances, because we are generally interested in how the increased repurchases are financed. For example, our argument does not clearly predict that cash holdings should decline. In fact, firms may simply change where they deploy cash without spending more or less overall (or even returning excess cash to shareholders, see Section VI).

We re-estimate equation (2) and use the following dependent variables as financing measures: equity issuances, debt issuances (long- versus short-term), leverage, and finally, cash holdings. We present the results in Table 8. In column 1, we find that treated firms significantly reduce their equity issuances relative to control firms in the two years following the merger. The negative and significant result on equity issuances is directly consistent with the firm's incentives to repurchase more shares – those same firms should have an equally strong incentive not to issue shares as the market impact has increased due to the lower fragility. In columns 2 to 5, we find no significant changes in the firm's cash or debt financing policies, suggesting that changes in fragility merely change how firms spend cash, not necessarily the overall amount.

Table 8 here

E. Effects on Corporate Investment

If treated firms increase share repurchases and reduce equity issuances while keeping debt issuances and cash holdings constant, then how do they finance these repurchases? It appears that the only source of cash flow left would then be a reduction in corporate investment as firms effectively substitute investment with repurchases. Such behavior has been documented in the literature before. For example, Almeida, Fos and Kronlund (2016) document how firms opportunistically execute repurchases at the expense of investment to positively influence EPS. We now investigate if our treated firms behave in the same way. If so, it would suggest that changes in financial fragility would ultimately affect not only share repurchases but also corporate investment policies. In fact, our motivating results from Section III have already documented such an association: fragile firms not only show lower repurchases but also higher capital expenditure in the cross-section. This motivating result is surprising in itself as one might expect the opposite when focusing only on the volatility-related consequences of stock price

fragility: high return volatility can negatively affect investment as shown in e.g., Vanousi and Papanikolaou (2012), a competing channel to the one operating through liquidity proposed here. We present the results of these tests in Table 9. The table follows the now familiar layout with only the dependent variable changed to $CAPEX_{ft}$. In column 1, we find a negative and significant effect on capital expenditure for treated relative to control firms that is robust to saturating the specification with controls and fixed effects. The fully specified estimation in column 5 suggests that treated firms register a reduction in capital expenditures of 0.35% of total assets. In economic terms, this reduction amounts to about 6% of the average annual capital expenditure for both treatment and control firms prior to the merger events. Again, a sizeable economic effect. Consistent with this drop in capital expenditure, columns 6 and 7 of the same table show that these firms also register lower total investment and lower total asset growth. We report robustness tests for all tests presented in this section using the alternative treatment specification in the Internet Appendix, Table IA.10. All these tests support the results shown here. Overall, this confirms that the price impact channel we document has a large effect on corporate policies on both sides of the balance sheet – financing & investment.

Table 9 here

VI. Why Repurchase for Price Impact?

To further clarify if seeking price impact is a key motive for firms to engage in repurchases, we examine cross-sectional heterogeneity of our results along a dimension that would give managers a strong incentive to maximize share prices: equity-based compensation. If firms focus on generating stock price impact, it is reasonable to expect executive compensation as a possible source of that motive (e.g., Kahle 2002, Bergstresser and Philippon 2006, Burns and Kedia 2006, Laux 2014, Peng and Roell 2008, 2013).

We collect CEO compensation information from the Execucomp database. While this database provides detailed compensation information, it is limited to US firms only, which restricts our sample for the tests in this section to US stocks only. We are interested in two related questions: First, are changes in repurchase behavior of treated stocks related to the firm's equity-based compensation policies? Second, is there any evidence that CEOs of treated firms trade differently in the firm's own stock around the asset management merger events?

To answer the first question, we augment the estimation of Table 5 with triple interaction terms that capture various dimensions of how executives of treated firms are exposed to the stock performance of their firms. We start with the market-to-book ratio as a simple proxy of managerial incentives (to increase the stock valuation) and then move to explicit measures of equity-based compensation: different measures of outstanding executive stock options as well as stock grants. All measures are as in Cheng and Warfield (2005) and defined in the Appendix. We present the results in Table 10. Across all measures, we find evidence supporting that the motive to repurchase for price impact is a compensation-based one: treated growth firms and treated firms with large amounts of outstanding executive stock options, option grants or executive stock ownership are more likely to repurchase shares in the post-merger periods, consistent with prior literature (e.g., Kahle 2002).

Table 10 here

In the next step, we examine the trading behavior of treated firm's CEOs in the firm's stock. To do this, we construct two measures of the trading activity of each firm's CEO: the first measure is as in Jin and Kothari (2008) and measures the net number of shares sold by a CEO in a year scaled by shares outstanding. The net number of shares sold is computed as the CEO's total stock holdings from the prior year plus the current year's stock grants and stocks received from option exercises minus the end-of-year total stock holdings. The second alternative measure only

focuses on option exercises: It is simply the value from option exercises reported in Execucomp scaled by the beginning of the year market value of equity of the firm. We report the results in Table 11. The results show that CEOs of treated firms sell significantly more shares in the post-merger periods compared to CEOs of control firms. A point estimate of 0.0007 (column (5)) indicates a 26.5% increase relative to the pre-merger unconditional sample average. In the Internet Appendix, Table IA.11, we confirm the result for an measure of CEO option exercise.

Table 11 here

VII. Discussion & Alternative Explanations

In this section, we further discuss our results, implement alternative econometric designs, and address alternative interpretations.

A. Price Impact versus Regulatory Constraints on Repurchase Activity

An immediate objection to our results and interpretation is that many jurisdictions regulate corporate repurchase programs to minimize opportunities for firms to unduly impact their stock prices via repurchase activities. For example, in the US, Rule 10b-18 outlines safe harbor provisions for repurchases that protect repurchasing firms from allegations of stock price manipulation. These safe harbor provisions contain, among others, price, and volume restrictions on repurchase programs. Globally, Wang, Yin, and Yu (2021) document that such price and volume restrictions are common in the regulation of (and impact on) corporate repurchase programs in many jurisdictions around the world.

However, while these restrictions seem to be important, anecdotal evidence and recent policy debate indicate that they may not be binding for firms, at least in the US. For example, in May 2023, the Securities and Exchange Commission (SEC) enacted new disclosure requirements that will take effect in 2024 and will mandate US firms to disclose on a quarterly basis quantitative repurchase information at a daily frequency, including, among others, the daily volume and

average price of repurchased securities. This points to significant concerns on the part of the SEC about the lack of disclosure and a better understanding of the motivations behind repurchase activities. Moreover, in its calls for comments on the new rules,[16] the SEC cites several academic studies indicating how repurchase activities could be linked to issues such as earnings management or executive compensation. To give one example, Cook, Krigman, and Leach (2003) use privately disclosed data on repurchase activities of a sample of firms and examine compliance rates with Rule 10b-18. While they find high levels of compliance in their sample, only 2 of their sample firms consistently comply with Rule 10b-18 in all their repurchase.

In light of this discussion, we now examine how our main results from Table 5 vary across countries that differ in their regulations (particularly regulations on price constraints) of repurchase activities. We report those results in Table 12. We split our sample first into US (columns 1 to 3) and non-US firms (columns 4 to 6) and subsequently investigate cross-sectional differences within these two subsamples. We treat the US separately because (a) it is the single biggest country in our sample, contributing about two-thirds of observations, and (b) one might have a prior that because of Rule 10b-18, repurchases for price impact could be more limited for US firms. However, the discussion above indicates that this need not be the case.

Table 12 here

In column 1 of Table 12, we show that our main results hold strongly for US firms. This raises the question of whether existing safe harbor provisions are ineffective or if there are certain types of firms in our sample that might find it easier to repurchase for price impact even in the presence of those restrictions. We explore two such dimensions in columns 2 and 3 of the same table and find that repurchases increase even more for small firms (column 2) or firms with

[16] Accessed on September 13, 2023 via https://www.sec.gov/files/rules/proposed/2021/34-93783.pdf

volatile stock returns (column 3), both of which might find it easier to avoid scrutiny for not complying the safe harbor conditions set out in Rule 10b-18.

In the remaining columns, we perform similar tests for all non-US firms in our sample who collectively contribute about one-third of our sample. We are interested in both re-estimating our main results for this subsample and exploring cross-sectional differences across countries with or without price (or price and volume) restrictions in their regulations for repurchase programs. We collect this information from Wang, Yin and Yu (2021), and define an indicator $Price\ Constraints_{ct}$ equals to 1 if country *c* imposes any form of price constraints on repurchase activities in year *t* and 0 otherwise. In a variation, we also consider if country *c* imposes any additional volume constraints. The estimates in column 4 show that in the subsample of non-US firms, our result is not significant. Columns 5 and 6 clarify that this is because there is no treatment response for firms in countries that impose any form of price restrictions on repurchase programs: the triple interaction term with $Price\ Constraints_{ct}$ is negative and significant. In contrast, the coefficient of $Post \times Treat$ is positive and of similar magnitude as the general effect for US firms, indicating that firms in countries without price restrictions on their repurchase activities show the same treatment response as US firms in general. Further controlling for additional volume restrictions (column 6) leads to similar but slightly stronger coefficient estimates.

We identify two take-away's from this discussion. First, a mediating effect of regulatory price constraints on our main result for non-US firms further supports our main hypothesis that firms indeed, at least at times, seek to repurchase for price impact. Second, the strength of our result for US firms and the recent context of regulatory changes in the US that aim to strengthen disclosure of repurchase activities gives further credence to the concern that existing safe harbor

provisions do not fully prevent firms from repurchasing for price impact. Future research will have to explore how the new disclosure rule affects repurchase activities of US firms. Having said that, we iterate our discussion from the introduction that our results do not equate fragility-driven repurchases with stock price manipulation. In fact, our analysis of long-run valuation effects (Section V.C) indicates a positive result: such repurchases are, if anything, indicative of long-run value creation.

B. Alternative Explanations and Econometric Designs

Next, we discuss and rule out several alternative explanations for our results. In the interest of brevity, we defer this discussion to the Internet Appendix, including Tables IA.12 and IA.13 and the complementary discussion there. We address three alternative explanations: First, we discuss if our results can be driven by changes in the firm's cost of capital that are linked to changes in stock liquidity. Second, we investigate whether changes in common ownership are likely behind our findings. Third, we examine if our findings are due to governance changes that could follow our natural experiments. We conclude that neither of these alternatives drives our results.

In a final effort to demonstrate that the channel behind our results operates through changes in fragility, we repeat our main results under alternative empirical designs. This includes changing how we assign the treatment status to stocks based on realized changes in fragility rather than pre-merger ownership characteristics or implementing an instrumental variable approach where we instrument the change in fragility of each stock using our original treatment indicator. We report those results in the Internet Appendix, Table IA.14.

VIII. Concluding Remarks

We highlight an important but overlooked characteristic of fragile stocks. While fragile stocks register higher return volatility, they also exhibit higher stock liquidity. We argue that the muted price impact of financial fragility is ultimately driven by the same forces that make fragile stocks volatile: their exposure and sensitivity to volatile but ultimately non-fundamental demand shocks. This element of stock price fragility affects corporate actions. Specifically, firms with fragile stocks have a lower incentive to repurchase their shares because the high liquidity of fragile stocks attenuates the positive stock price impact of share repurchases. We establish causality by relying on natural experiments that exogenously change stock price fragility and that we show to be directly affected by repurchase behavior. Furthermore, changes in repurchase behavior are driven by changes in tender offers that are known to generate the strongest price impact and do not lead to changes in stock liquidity but significant changes in stock prices, thereby ruling out that the changes in repurchases we document are implemented to affect stock liquidity as some prior literature would predict. We also show that the lower incentives to engage in share repurchases allow firms with fragile stocks to invest more. Our results shed light on the important but unexpected real implications that are associated with financial fragility.

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Table 1. Summary Statistics

This table presents summary statistics of the variables used in the panel regressions. *Total volatility* is computed as the annualized standard deviation of daily stock returns. *Amihud* is defined as the monthly average of the daily Amihud, which is computed as absolute daily stock return divided by the dollar trading volume (in million US$) on that day. *Spread* is the monthly average of daily bid-ask spreads. *Repurchase* is the share repurchase of common and preferred stocks scaled by the beginning-of-period total assets. *CAPEX* is capital investments scaled by the beginning-of-period total assets. $\sqrt{Fragility}$ is defined as in Massa, Schumacher, and Wang (2021) where fragility is computed based on the holdings, returns, and flows of all funds in FactSet (i.e., including open-end and non-open-end funds). *Firm size* is the logarithm of total assets. *Log(B/M)* is the logarithm of the book value of equity divided by the market value of equity. *Cashflow* is computed as the income before extraordinary items plus depreciation scaled by the beginning-of-period total assets. *IO* is the total institutional ownership calculated as the sum of all holdings of all funds in FactSet divided by shares outstanding. *Age* is the logarithm of the number of years since a firm appears in DataStream. *Cash holdings* is the total cash holdings divided by the beginning-of-period total assets. *Leverage* is the long-term debt plus current liabilities divided by the beginning-of-period total assets. *Dividend* is the cash dividends paid by a firm scaled by the beginning-of-period total assets. *Mom* is the trailing twelve-month total stock return.

	Mean	P25	Median	P75	SD	Obs.
Total volatility	0.4149	0.2802	0.3743	0.5133	0.1810	61,123
Amihud	0.2174	0.0021	0.0167	0.1484	0.4659	61,123
Spread	0.0075	0.0010	0.0033	0.0083	0.0127	61,123
Repurchase	0.0113	0.0000	0.0000	0.0034	0.0330	61,123
CAPEX	0.0587	0.0158	0.0349	0.0697	0.0795	61,123
$\sqrt{Fragility}$	0.3805	0.1042	0.3005	0.5736	0.3379	61,123
Firm size	6.4973	5.2911	6.4058	7.6392	1.7347	61,123
Log(B/M)	-0.5188	-1.0081	-0.4781	0.0187	0.7898	61,123
Cash flow	0.0932	0.0482	0.0933	0.1520	0.1382	61,123
IO	0.3288	0.0553	0.1823	0.5809	0.3289	61,123
Age	2.4705	1.9459	2.6391	3.1355	0.8855	61,123
Cash holdings	0.2020	0.0504	0.1262	0.2618	0.2536	61,123
Leverage	0.2243	0.0073	0.1707	0.3830	0.2220	61,123
Dividend	0.0182	0.0000	0.0074	0.0210	0.0339	61,123
Mom	0.2051	-0.1489	0.1084	0.4174	0.6153	61,123

Table 2. Fragility, Return Volatility, and Illiquidity

This table presents the results of the relation between total return volatility, illiquidity, and fragility for the global sample of stocks from the Worldscope universe. All variables are as defined in Table 1. Columns 1, 4, and 7 present the results of the specification including country and industry fixed effects. Columns 2, 5, and 8 present the results of the specification including country, industry, and year fixed effects. Columns 3, 6, and 9 present the results of the specification including stock and year fixed effects. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that allow for clustering at the stock level.

	1	2	3	4	5	6	7	8	9
Dep. Var.:	Total Volatility	Total Volatility	Total Volatility	Amihud	Amihud	Amihud	Spread	Spread	Spread
$\sqrt{Fragility}$	0.0142***	0.0178***	0.0107***	-0.0981***	-0.0911***	-0.0227***	-0.0011***	-0.0012***	-0.0010***
	(4.52)	(6.27)	(3.51)	(-9.27)	(-8.46)	(-2.60)	(-3.92)	(-4.48)	(-3.56)
Firm size	-0.0251***	-0.0255***	-0.0287***	-0.1154***	-0.1168***	-0.1256***	-0.0024***	-0.0025***	-0.0026***
	(-33.98)	(-36.91)	(-11.93)	(-50.26)	(-50.27)	(-19.59)	(-45.02)	(-45.82)	(-12.82)
Log(B/M)	0.0244***	0.0028**	0.0211***	0.1174***	0.1054***	0.1119***	0.0026***	0.0020***	0.0029***
	(17.19)	(2.12)	(11.62)	(28.45)	(24.90)	(22.84)	(23.37)	(18.60)	(18.51)
Cash flow	-0.1367***	-0.1658***	-0.0816***	-0.0877***	-0.0806***	-0.2463***	-0.0047***	-0.0048***	-0.0048***
	(-20.63)	(-27.77)	(-11.48)	(-4.51)	(-4.13)	(-12.14)	(-7.59)	(-7.91)	(-6.88)
IO	-0.0720***	-0.0881***	-0.0786***	-0.3102***	-0.3218***	-0.1966***	-0.0058***	-0.0063***	-0.0011
	(-11.74)	(-15.37)	(-8.29)	(-16.29)	(-16.81)	(-8.15)	(-12.39)	(-13.70)	(-1.49)
Age	-0.0202***	-0.0190***	-0.0133***	0.0301***	0.0295***	0.0398***	0.0003***	0.0003***	0.0005***
	(-18.62)	(-19.15)	(-4.82)	(10.08)	(9.80)	(5.86)	(4.38)	(3.89)	(2.61)
Cash holdings	0.0413***	0.0232***	-0.0199***	-0.1186***	-0.1262***	-0.0976***	-0.0021***	-0.0026***	-0.0032***
	(10.53)	(6.82)	(-4.75)	(-12.20)	(-12.85)	(-9.85)	(-7.96)	(-9.82)	(-8.70)
Leverage	0.0859***	0.0747***	0.0724***	0.1249***	0.1233***	0.0840***	0.0026***	0.0026***	0.0022***
	(18.09)	(17.32)	(12.33)	(9.82)	(9.68)	(6.16)	(8.33)	(8.31)	(5.12)
Dividend	-0.4218***	-0.5679***	-0.1741***	-0.1424*	-0.2321***	-0.4581***	-0.0034*	-0.0087***	-0.0064**
	(-13.96)	(-21.01)	(-6.68)	(-1.90)	(-3.07)	(-5.29)	(-1.73)	(-4.55)	(-2.57)
Mom	-0.0334***	0.0209***	0.0225***	-0.0490***	-0.0450***	-0.0367***	-0.0022***	-0.0010***	-0.0006***
	(-25.52)	(17.70)	(19.00)	(-17.88)	(-14.61)	(-13.15)	(-25.62)	(-11.69)	(-6.47)
Country F.E.	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Industry F.E.	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Year F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Stock F.E.	No	No	Yes	No	No	Yes	No	No	Yes
N	61,123	61,123	61,123	61,123	61,123	61,123	61,123	61,123	61,123
adj. R^2	0.26	0.54	0.72	0.37	0.38	0.71	0.34	0.37	0.60

Table 3. Fragility, Repurchase, and Capital Expenditures

This table presents the results of the relation between share repurchases, capital expenditures, and fragility for the global sample of stocks from the Worldscope universe. All specifications are as in Table 2, only the dependent variables are exchanged: Columns 1 to 3 use *Repurchase* as the dependent variable, columns 4 to 6 use *CAPEX*. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that allow for clustering at the stock level.

	1	2	3	4	5	6
Dep. Var.:	Repurchase	Repurchase	Repurchase	CAPEX	CAPEX	CAPEX
$\sqrt{Fragility}$	-0.0037***	-0.0041***	-0.0033***	0.0117***	0.0096***	0.0033*
	(-4.49)	(-5.01)	(-3.71)	(6.42)	(5.17)	(1.75)
Firm size	0.0002	0.0002	0.0029***	-0.0056***	-0.0053***	-0.0311***
	(1.00)	(1.26)	(4.94)	(-14.14)	(-13.35)	(-17.95)
Log(B/M)	-0.0079***	-0.0078***	-0.0051***	-0.0086***	-0.0070***	-0.0087***
	(-18.97)	(-18.07)	(-10.77)	(-11.92)	(-9.24)	(-9.32)
Cash flow	0.0343***	0.0339***	0.0164***	0.1010***	0.0977***	0.0556***
	(18.66)	(18.14)	(7.44)	(20.48)	(19.88)	(12.39)
IO	0.0244***	0.0245***	0.0112***	0.0013	0.0027	0.0339***
	(15.61)	(15.67)	(3.82)	(0.41)	(0.86)	(6.52)
Age	0.0010***	0.0010***	0.0025***	-0.0058***	-0.0058***	-0.0064***
	(4.25)	(4.19)	(3.68)	(-9.12)	(-8.96)	(-3.77)
Cash holdings	0.0001	0.0001	-0.0038***	0.0036	0.0038	0.0138***
	(0.16)	(0.13)	(-3.22)	(1.06)	(1.15)	(3.11)
Leverage	-0.0172***	-0.0172***	-0.0294***	0.0217***	0.0217***	-0.0280***
	(-15.78)	(-15.76)	(-16.68)	(7.85)	(7.88)	(-8.28)
Dividend	-0.0328***	-0.0327***	-0.0203**	-0.1553***	-0.1463***	0.0579***
	(-3.76)	(-3.73)	(-2.00)	(-10.27)	(-9.68)	(3.62)
Mom	-0.0036***	-0.0029***	-0.0012***	0.0029***	0.0064***	0.0001
	(-15.60)	(-11.06)	(-4.49)	(4.42)	(8.21)	(0.10)
Country F.E.	Yes	Yes	No	Yes	Yes	No
Industry F.E.	Yes	Yes	No	Yes	Yes	No
Year F.E.	No	Yes	Yes	No	Yes	Yes
Stock F.E.	No	No	Yes	No	No	Yes
N	61,123	61,123	61,123	61,123	61,123	61,123
adj. R^2	0.17	0.17	0.42	0.25	0.26	0.63

Table 4. Propensity Score Matching

This table reports the results of the propensity score matching to construct the sample of treated and control firms for the difference-in-difference tests. We first define the treatment and control samples as follows: for each merger deal, firms are sorted into quintiles based on a measure of pre-merger portfolio overlap between buyer- and target-affiliated funds as described in Section IV.A. Firms in the top (bottom) quintile of this measure are considered treatment (control) firms. We then match firms in the top quintile with the firms in the bottom quintile using one-to-one nearest neighbor propensity score matching. The matching firm characteristics include country and industry affiliation, log of total assets, log of book-to-market ratio, cash flows, and total institutional ownership. All variables are as defined in Appendix A. The table reports the univariate comparison of the matching firm characteristics between treated and control firms before and after propensity score matching and their corresponding *t*-statistics. Additional estimates from a Probit model for treated and control firms before and after propensity score matching are presented in the Internet Appendix, Table IA.2 * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively. Standard errors are clustered at the firm level.

	Before matching				After matching			
	Treat	Control	Difference	*t*-stats	Treat	Control	Difference	*t*-stats
Firm size$_{-2}$	7.751	7.078	0.672***	(10.06)	7.763	7.755	0.007	(0.15)
Log(B/M)$_{-2}$	-0.880	-0.747	-0.133***	(-4.60)	-0.866	-0.859	-0.007	(-0.25)
Cashflow$_{-2}$	0.123	0.119	0.004	(1.23)	0.119	0.118	0.001	(0.45)
IO$_{-2}$	0.614	0.377	0.236***	(15.80)	0.651	0.622	0.029	(1.19)
Age$_{-2}$	2.706	2.530	0.176***	(5.88)	2.629	2.635	-0.007	(-0.17)
Cash holdings$_{-2}$	0.188	0.187	0.001	(0.02)	0.179	0.175	0.003	(0.75)
Leverage$_{-2}$	0.295	0.255	0.039***	(3.70)	0.281	0.287	-0.005	(-0.84)
Dividend$_{-2}$	0.016	0.019	-0.003***	(-3.29)	0.017	0.018	-0.001	(-0.87)
Mom$_{-2}$	-0.005	0.021	-0.027	(-1.49)	-0.074	-0.098	0.024	(0.85)
D_Repurchase$_{-2 \text{ to } 0}$	-0.004	-0.002	-0.002***	(-3.49)	-0.005	-0.005	0.001	(0.73)
D_CAPEX$_{-2 \text{ to } 0}$	-0.007	-0.008	0.001	(1.03)	-0.008	-0.007	-0.001	(1.32)

Table 5. Difference-in-Difference Analysis of Changes in Payout Policy

This table reports the regression estimates of changes in payout policy from the post-matching difference-in-difference analysis. The estimated regression is as follows:

$$Payout_{fdt} = \beta_1 T_{fd} + \beta_2 Post_{dt} + \beta_3 T_{fd} \times Post_{dt} + \gamma_1' X_{ft-1} + \gamma_2' \left(Post_{dt} \times X_{ft-1}\right) + \alpha_t + \alpha_f + \alpha_d + \epsilon_{fdt,}$$

where $Payout_{fdt}$ refers to one of the two payout measures of firm *f* affected by deal *d* in year *t*, namely, dividends, or share repurchase. The treatment variable T_{fd} is equal to 1 for treated stocks and 0 for control stocks. $Post_{dt}$ is an indicator equal to 1 for two years after merger *d* and 0 for two years before the mergers and the main coefficient of interest is β_3 on the interaction term between $Post_{dt}$ and T_{fd}. The regression further includes year fixed effects denoted by α_t, stock fixed effects denoted by α_s, deal fixed effects denoted by α_d, stock characteristics denoted by the vector X_{ft-1} and additional interaction terms between the stock characteristics and the $Post_{dt}$ indicator. The vector X_{ft-1} includes the following stock characteristics introduced in Table 2. Panel A (Panel B) reports the regression estimates of difference-in-difference analysis of changes in share repurchase (dividends). The use of fixed effects is indicated at the bottom of each column. In Column 6, the variable $Post_{dt}$ is decomposed into period-specific indicator variables: $Before1_{dt}$ is equal to 1 for one year before the mergers and 0 otherwise, $After1_{dt}$ is equal to 1 for one year after the mergers and 0 otherwise, and $After2_{dt}$ is equal to 1 for two years after the mergers and 0 otherwise. All variables are as defined in Appendix A. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level.

Panel A: Repurchase

	1	2	3	4	5	6
Dep. Var.:	Repurchase	Repurchase	Repurchase	Repurchase	Repurchase	Repurchase
POST x Treat	0.0056***	0.0059***	0.0061***	0.0054***	0.0055***	
	(4.22)	(4.34)	(4.53)	(3.82)	(3.88)	
Before1 x Treat						-0.0028
						(-1.63)
After1 x Treat						0.0045**
						(2.29)
After2 x Treat						0.0037*
						(1.84)
Before1						0.0125**
						(2.07)
After1						-0.0090
						(-1.03)
After2						-0.0078
						(-0.82)
POST	-0.0039***	-0.0079*	0.0010	-0.0112*	-0.0183**	
	(-3.69)	(-1.77)	(0.19)	(-1.79)	(-2.57)	
Treat	-0.0009	-0.0028*	-0.0029**	-0.0016	-0.0018	-0.0004
	(-0.57)	(-1.96)	(-2.04)	(-1.36)	(-1.57)	(-0.26)
Firm size		-0.0009*	-0.0008	0.0042**	0.0055***	0.0059***
		(-1.80)	(-1.53)	(2.11)	(2.69)	(2.74)
Log(B/M)		-0.0154***	-0.0148***	-0.0110***	-0.0083***	-0.0105***
		(-12.11)	(-10.71)	(-8.06)	(-5.69)	(-6.04)
Cashflow		0.0681***	0.0663***	0.0347***	0.0307***	0.0487***
		(8.65)	(8.45)	(4.22)	(3.75)	(4.46)
IO		0.0343***	0.0342***	0.0224*	0.0181	0.0260**
		(13.88)	(13.79)	(1.87)	(1.55)	(2.20)
Age		0.0019**	0.0020**	-0.0005	0.0084**	0.0077**
		(2.31)	(2.39)	(-0.14)	(2.32)	(2.08)
Cash holdings		-0.0041	-0.0029	-0.0190***	-0.0145***	-0.0142**
		(-0.98)	(-0.69)	(-3.27)	(-2.61)	(-2.11)
Leverage		-0.0386***	-0.0385***	-0.0557***	-0.0527***	-0.0565***
		(-9.31)	(-9.26)	(-8.88)	(-8.51)	(-8.22)
Dividend		-0.0821**	-0.0879***	-0.0005	-0.0094	-0.0065

		(-2.57)	(-2.76)	(-0.02)	(-0.29)	(-0.17)
Mom		-0.0011	0.0039***	-0.0061***	-0.0004	0.0037
		(-0.82)	(2.68)	(-5.71)	(-0.21)	(1.62)
Deal F.E.	No	No	No	No	Yes	Yes
Firm F.E.	No	No	No	Yes	Yes	Yes
Year F.E.	No	No	Yes	No	Yes	Yes
N	18,328	17,723	17,723	17,723	17,723	17,723
adj. R^2	0.00	0.22	0.22	0.58	0.58	0.59

Panel B: Dividend

	1	2	3	4	5	6
Dep. Var.:	Dividend	Dividend	Dividend	Dividend	Dividend	Dividend
POST x Treat	0.0006	0.0008	0.0008	0.0004	0.0005	
	(0.95)	(1.15)	(1.22)	(0.60)	(0.73)	
Before1 x Treat						0.0000
						(0.05)
After1 x Treat						0.0004
						(0.41)
After2 x Treat						0.0006
						(0.64)
Before1						-0.0060
						(-1.62)
After1						-0.0039
						(-0.68)
After2						-0.0029
						(-0.44)
POST	-0.0008	-0.0010	-0.0013	-0.0068*	-0.0041	
	(-1.27)	(-0.32)	(-0.37)	(-1.77)	(-1.07)	
Treat	-0.0011	-0.0009	-0.0009	-0.0010*	-0.0011*	-0.0011
	(-1.23)	(-1.09)	(-1.18)	(-1.76)	(-1.91)	(-1.27)
Controls	No	Yes	Yes	Yes	Yes	Yes
Deal F.E.	No	No	No	No	Yes	Yes
Firm F.E.	No	No	No	Yes	Yes	Yes
Year F.E.	No	No	Yes	No	Yes	Yes
N	18,328	17,723	17,723	17,723	17,723	17,723
adj. R^2	0.00	0.22	0.23	0.69	0.69	0.69

Table 6. Liquidity Effects

This table examines changes in stock liquidity around repurchase announcements of treated and control firms around the merger events. The sample includes all the share repurchase announcements from SDC Platinum matched to the difference-in-difference sample and the dependent variables include different measures of stock liquidity. Panels A examines the changes in Amihud of affected stocks by merger events over 1-month, 3-month, 6-month, 12-month, and 24-month windows around the repurchase announcements. Panel B repeats the same test but uses bid-ask spreads as liquidity measure. All variables are defined in Table 1. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level.

Panel A: Amihud

	1	2	3	4	5
Event Window	1-month	3-month	6-month	12-month	24-month
Dep. Var.	ΔAmihud	ΔAmihud	ΔAmihud	ΔAmihud	ΔAmihud
POST x Treat	0.0000	0.0000**	0.0000**	0.0000	0.0000
	(1.00)	(2.34)	(2.00)	(1.47)	(0.92)
Treat	-0.0000	-0.0000**	-0.0000**	-0.0000	-0.0000
	(-0.88)	(-2.52)	(-2.37)	(-1.45)	(-0.73)
POST	0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(0.53)	(-0.81)	(-0.78)	(-1.34)	(-0.59)
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	4,088	4,088	4,073	3,980	3,279
adj. R^2	0.79	0.69	0.77	0.78	0.85

Panel B: Spread

	1	2	3	4	5
Event Window	1-month	3-month	6-month	12-month	24-month
Dep. Var.	ΔSpread	ΔSpread	ΔSpread	ΔSpread	ΔSpread
POST x Treat	0.0000	-0.0000	0.0000	0.0000	0.0001
	(0.58)	(-0.55)	(0.29)	(0.29)	(1.32)
Treat	-0.0000	0.0000	-0.0001	-0.0000	-0.0001**
	(-0.26)	(0.15)	(-1.16)	(-0.97)	(-2.02)
POST	-0.0001	-0.0010	-0.0020**	-0.0029**	-0.0050**
	(-0.13)	(-1.62)	(-2.42)	(-2.57)	(-2.20)
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	3,992	3,954	3,903	3,793	2,738
adj. R^2	0.33	0.56	0.62	0.79	0.89

Table 7. Short- and Long-Run Valuation Effects

This table examines short- and long-run cumulative abnormal returns (CARs) around repurchase announcements of treated and control firms. The sample and specification are as in Table 6 and the dependent variables include different measures of cumulative abnormal stock returns relative to different benchmark portfolios. Panels A examines short-run CARs immediately around the share repurchase announcements and Panel B examines long-run CARs over the following 12 or 24 months following the month of the repurchase announcements. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level.

Panel A: Short-Run Cumulative Abnormal Returns

	1	2	3	4	5	6
Event Window	1-day	1-day	1-day	3-day	3-day	3-day
Benchm adj.	Market	Industry	DGTW	Market	Industry	DGTW
POST x Treat	0.0051*	0.0066**	0.0061**	0.0091**	0.0085**	0.0102**
	(1.86)	(2.52)	(2.22)	(2.10)	(2.13)	(2.29)
Treat	-0.0024	-0.0034**	-0.0032*	-0.0051	-0.0046	-0.0067**
	(-1.31)	(-1.97)	(-1.72)	(-1.62)	(-1.63)	(-2.13)
POST	0.0051	0.0032	0.0078	0.0272	0.0290	0.0339
	(0.23)	(0.15)	(0.35)	(0.80)	(0.87)	(1.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	3,753	3,749	3,704	3,738	3,733	3,690
adj. R^2	0.31	0.31	0.32	0.33	0.31	0.34

Panel B: Long-Run Cumulative Abnormal Returns

	1	2	3	4	5	6
Horizon	12 months	12 months	12 months	24 months	24 months	24 months
Benchm adj.	Market	Industry	DGTW	Market	Industry	DGTW
POST x Treat	0.0747***	0.0436*	0.0650**	0.0707**	0.0635*	0.0593
	(2.81)	(1.82)	(2.38)	(1.98)	(1.88)	(1.46)
Treat	-0.0381**	-0.0161	-0.0384*	-0.0276	-0.0196	-0.0279
	(-1.97)	(-0.93)	(-1.90)	(-0.99)	(-0.77)	(-0.90)
POST	-0.2405	-0.2205	-0.2292	-0.2578	-0.1889	-0.3402
	(-1.51)	(-1.52)	(-1.34)	(-1.15)	(-0.89)	(-1.53)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	4,043	4,043	3,970	3,909	3,909	3,807
adj. R^2	0.44	0.47	0.47	0.54	0.52	0.56

Table 8. Effects on Other Financing Policies

This table examines if treated firms register changes in other corporate policies, specifically, financing policies. The specifications are as in Table 5 but use alternative outcome variables to measure changes in other financing policies including *Chgcash* is the change in cash or cash equivalent, *Chgstdebt* is the change in current debt scaled by the beginning-of-period total assets, *Chgltdebt* is long-term debt issuance minus long-term debt reduction scaled by the beginning-of-period total assets, *Chglev* is the change in leverage where leverage is computed as long-term debt plus current liabilities divided by the beginning-of-period total assets, and *Equityiss* is the sale of common and preferred stocks scaled by the beginning-of-period total assets. All other specifications are unchanged. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level, computed from standard errors clustered at the firm level.

Dep. Var.:	1 Equityiss	2 Chgstdebt	3 Chgltdebt	4 Chglev	5 Chgcash
POST x Treat	-0.0060***	-0.0003	-0.0011	0.0010	-0.0016
	(-2.67)	(-0.34)	(-0.28)	(1.13)	(-0.59)
Treat	0.0030*	0.0006	-0.0002	-0.0008	0.0012
	(1.88)	(0.99)	(-0.06)	(-1.16)	(0.60)
POST	0.0077	0.0017	-0.0478**	-0.0097*	0.0173
	(0.41)	(0.31)	(-2.47)	(-1.75)	(0.78)
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	17,723	17,723	17,723	17,723	17,723
adj. R^2	0.47	0.05	0.28	0.07	0.46

Table 9. Difference-in-Difference Analysis of Changes in Investment Policy

This table reports the regression estimates of changes in investment policy from the post-matching difference-in-difference analysis. The specifications are as in Table 5, only the dependent variables are exchanged. Columns 1 to 5 use *CAPEX* as the dependent variable, column 6 uses *Total Investment*, and column 7 uses *Total Asset Growth*. All variables are as defined in Appendix A and the use of fixed effects is indicated at the bottom of each column. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level.

Dep. Var.:	1 CAPEX	2 CAPEX	3 CAPEX	4 CAPEX	5 CAPEX	6 Total Investment	7 Total Asset Growth
POST x	-0.0043***	-0.0049***	-0.0047***	-0.0035**	-0.0034**	-0.0051*	-0.0193*
Treat	(-2.75)	(-3.13)	(-3.05)	(-2.31)	(-2.25)	(-1.74)	(-1.93)
POST	-0.0059***	0.0096	0.0119*	-0.0056	-0.0125*	-0.0216	-0.0527
	(-4.34)	(1.43)	(1.73)	(-0.86)	(-1.75)	(-1.32)	(-0.77)
Treat	0.0042*	0.0045**	0.0044**	0.0022**	0.0023**	0.0009	0.0081
	(1.90)	(2.07)	(2.03)	(2.04)	(2.23)	(0.43)	(1.00)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	No	No	No	No	Yes	Yes	Yes
Firm F.E.	No	No	No	Yes	Yes	Yes	Yes
Year F.E.	No	No	Yes	No	Yes	Yes	Yes
N	18,328	17,723	17,723	17,723	17,723	17,723	17,723
adj. R^2	0.01	0.08	0.08	0.74	0.74	0.58	0.43

Table 10. Cross-sectional Heterogeneity by Equity-based Executive Compensation

This table examines changes in repurchases for firms with different equity-based executive compensation policies. The specification is as in Table 5 but the sample is restricted to sample firms in the Execucomp database. The estimation is augmented with triple interaction terms for different measures of equity-based executive compensation. All measures are defined in the Appendix. All other specifications are unchanged. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level, computed from standard errors clustered at the firm level.

Dep. Var.:	1 Repurchase	2 Repurchase	3 Repurchase	4 Repurchase	5 Repurchase
POST x Treat x Log(B/M)	-0.0092***				
	(-3.12)				
POST x Treat x % Ex. Options		0.5475**			
		(2.08)			
POST x Treat x % Unex. Options			2.9313***		
			(5.58)		
POST x Treat x % Option Grants				3.4590***	
				(3.44)	
POST x Treat x % Stock Own					0.0600*
					(1.93)
Treat	-0.0021	-0.0024	-0.0006	-0.0026	-0.0053***
	(-1.08)	(-1.21)	(-0.31)	(-1.42)	(-2.74)
POST x Treat	0.0006	0.0075***	0.0034	0.0069**	0.0105***
	(0.26)	(2.81)	(1.19)	(2.45)	(3.89)
POST	-0.0248**	-0.0418*	-0.0309	-0.0333	-0.0354
	(-2.23)	(-1.90)	(-1.42)	(-1.51)	(-1.58)
Dual-interactions	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	11,581	6,609	6,609	6,609	6,609
adj. R^2	0.59	0.61	0.61	0.61	0.60

Table 11. CEO Trading Decisions

This table examines the equity trading decisions of CEOs of treated versus control firms. The specification is as in Table 5 but the sample is restricted to sample firms in the Execucomp database. The dependent variable *Sold Stock* is defined as the net number of shares sold by a CEO in a year scaled by shares outstanding (Jin and Kothari 2008). All variables are as defined in Appendix A and the use of fixed effects is indicated at the bottom of each column. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level.

Dep. Var.:	1 Sold Stock	2 Sold Stock	3 Sold Stock	4 Sold Stock	5 Sold Stock
POST x Treat	0.0005*	0.0007***	0.0007***	0.0007**	0.0007**
	(1.96)	(2.67)	(2.74)	(2.29)	(2.40)
Treat	-0.0004	-0.0008***	-0.0008***	-0.0003	-0.0003*
	(-1.48)	(-2.71)	(-2.73)	(-1.62)	(-1.71)
POST	-0.0012***	-0.0056***	-0.0050**	-0.0067***	-0.0064***
	(-5.01)	(-2.61)	(-2.30)	(-3.23)	(-3.19)
Controls	No	Yes	Yes	Yes	Yes
Deal F.E.	No	No	No	No	Yes
Firm F.E.	No	No	No	Yes	Yes
Year F.E.	No	No	Yes	No	Yes
N	6,766	6,754	6,754	6,754	6,754
adj. R^2	0.01	0.10	0.10	0.32	0.32

Table 12. Cross-sectional Heterogeneity in US and Rest of World

This table examines changes in repurchases for US and Rest of World firms separately. The specification is as in Table 5 but the sample is restricted to US firms in columns 1 to 3 and Rest of World firms in columns 4 to 6. The estimations in columns 2, 3, 5, and 6 are further augmented with triple interaction terms. In columns 2 and 3, interaction terms with the variables *Firm Size* and *Total Volatility* are added for US firms. In columns 5 and 6, interaction terms that indicate if the listing country of the stock has price restrictions (column 5) or price and volume restrictions (column 6) for repurchasing firms in place are added for Rest of World firms. Information on Price and Volume restrictions in repurchase programs across countries are collected from Wang, Yin, and Yu (2021). All other specifications are unchanged. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level, computed from standard errors clustered at the firm level.

	1	2	3	4	5	6
Sample		US Stocks			Rest of World Stocks	
Dep. Var.:	Repurchase	Repurchase	Repurchase	Repurchase	Repurchase	Repurchase
POST x Treat x Firm Size		-0.0030***				
		(-2.78)				
POST x Treat x Total Volatility			0.0191**			
			(1.99)			
POST x Treat x Price Constraints					-0.0080**	
					(-2.06)	
POST x Treat x Price & Volume Constraints						-0.0114**
						(-2.07)
POST x Treat	0.0079***	0.0314***	-0.0015	0.0010	0.0084**	0.0115**
	(3.92)	(3.33)	(-0.31)	(0.65)	(2.26)	(2.13)
Treat	-0.0029*	-0.0064	0.0035	0.0000	0.0008	-0.0018
	(-1.78)	(-0.68)	(0.80)	(0.03)	(0.18)	(-0.39)
POST	-0.0277**	-0.0413***	-0.0147	-0.0016	-0.0070	-0.0107
	(-2.34)	(-3.17)	(-1.28)	(-0.22)	(-0.91)	(-1.20)
Dual-interactions	NA	Yes	Yes	NA	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	11,581	11,290	10,850	6,142	6,142	6,142
adj. R^2	0.59	0.59	0.59	0.36	0.36	0.36

Appendix A: Variable Definitions

This appendix includes a full list of all variables and their definitions.

Variables	Definition
Dependent variables	
Total Volatility	Annualized standard deviation of daily stock returns over the period.
Amihud	Average of daily price impact measure of Amihud (2002) over the period defined as the absolute daily stock return divided by the dollar trading volume on the day in million US$.
Spread	Average daily bid-ask spread
Log Trading Volume	Natural logarithm of average daily trading volume per month, in million US$.
Turnover	Average of daily stock turnover defined as number of shares traded divided by shares outstanding.
Dividend	Cash dividends scaled by the beginning-of-period total assets.
Repurchase	Purchase of common and preferred stocks scaled by the beginning-of-period total assets.
D_ Repurchase$_{-2\ to\ 0}$	The simple difference in *Repurchase* between years 0 and -2.
CAPEX	Capital expenditure scaled by the beginning-of-period total assets.
D_ CAPEX$_{-2\ to\ 0}$	The simple difference in *CAPEX* between years 0 and -2.
Total Investments	Sum of capital, R&D and acquisition expenditure scaled by the beginning-of-period total assets.
Total Asset Growth	Log changes in total assets.
Tender Offer	Indicator equal to 1 if the firm conducts the repurchase via a tender offer and 0 otherwise.
CAR	Cumulative abnormal stock returns over different event windows or horizons in excess of different benchmark portfolios: "Market" refers to the domestic market portfolio of firm f, "Industry" refers to a domestic industry portfolio of firm f, and "DGTW" refers to a domestic size-value-momentum matched portfolio of firm f. All the benchmark portfolios are value-weighted and constructed from the full universe of stocks in the Worldscope database that (i) share the same primary listing country as the stock, (ii) share the same primary listing location and Datastream industry classification as the stock, or (iii) share the same primary listing location and are matched on their size, value, and momentum characteristics as the stock
Chgcash	Cash or cash equivalent increase or decrease scaled by the beginning-of-period total assets.
Chgstdebt	Current debt changes scaled by the beginning-of-period total assets.
Chgltdebt	Long-term debt issuance minus long-term debt reduction scaled by the beginning-of-period total assets.
Chglev	Leverage in period t minus the leverage in period $t-1$. The leverage is computed as the long-term debt plus current liability divided by the beginning-of-period total assets.
Equityiss	Sales of common and preferred stocks scaled by the beginning-of-period total assets.
Control variables	
$\sqrt{Fragility}$	Square root of the fragility measure of Greenwood and Thesmar (2011) but computed including holdings, return, and flow information for all fund types in FactSet as in Massa, Schumacher, and Wang (2021).
Firm Size	Logarithm of the beginning-of-period total assets.
Log(B/M)	Logarithm of book value of equity divided by the market value of equity.
Cash flow	Income before extraordinary items plus depreciation scaled by the beginning-of-period total assets.
IO	Total institutional ownership calculated as the sum of all holdings of all funds in FactSet divided by shares outstanding.
Age	Logarithm of the number of years since a firm appears in DataStream.

Cash holdings	Total cash holdings divided by the beginning-of-period total assets.
Leverage	Long-term debt plus current liabilities divided by the beginning-of-period total assets.
Mom	The trailing-twelve-month total stock return.
Price Constraints	Indicator equal to 1 if the listing country of the firm has any price constraints for corporate repurchase programs in place and 0 otherwise. The information is collected from Wang, Yin, and Yu (2021).
Price & Volume Constraints	Indicator equal to 1 if the listing country of the firm has any price or volume constraints for corporate repurchase programs in place and 0 otherwise. The information is collected from Wang, Yin, and Yu (2021).
Executive compensation measures	
% Ex. Options	Shares of exercisable options divided by the total shares outstanding as in Cheng and Warfield (2005).
% Unex. Options	Shares of unexercisable options (excluding option grants) divided by the total shares outstanding as in Cheng and Warfield (2005).
% Option Grants	Shares of option grants divided by the total shares outstanding as in Cheng and Warfield (2005).
% Stock Own	Total shares held by CEOs divided by total shares outstanding as in Cheng and Warfield (2005).
Sold Stock	Net number of shares sold by the CEO over the year scaled by beginning-of-year shares outstanding. The net number of shares sold is computed as the CEOs total stock holdings from the prior year plus the current years stock grants and stocks received from option exercises minus the end of year total stock holdings.
Option Exer	The value of options exercised by CEOs in a given year divided by the beginning-of-year total market capitalization of the firm.
Pre-merger ownership characteristics	
IO_oef	Total ownership of open-ended funds (OEFs).
IO_flowvola	The stock ownership of funds that are in the top quartile of the fund flow volatility. Fund flow volatility is the standard deviation of the flows over the past three years.
Flow correlation	The average flow correlation of each pair of funds, weighted by the maximum market value of the position of one of two pair members.
IO_excessweight	The stock ownership of funds that are on the top quartile of the excess weight, which is computed as the difference between the portfolio weight and its corresponding benchmark weight.
Corporate governance measures	
G index	*Gindex* is the governance index as in Gompers, Ishii, and Metrick (2003) from ISS/RiskMetrics.
E index	*Eindex* is the entrenchment index as in Bebchuk et al. (2009) from ISS/RiskMetrics.
Logn_blckholder	The log number of institutions that held more than 5 percent of the total shares.
%ShareholderProposal	The percentage of the shareholder proposal, defined as the total number of shareholder proposals scaled by the total number of proposals in any firm-year.
Participation	The average participation in any firm-year, defined as the total number of ballots divided by the total share outstanding.
%AgainstMgmt	The average percentage of votes against management proposal in any firm-year.
%Pass	The fraction of management proposals that "Pass" for each firm-year. The average is taken over all proposal outcomes in a given year where a proposal outcome is equal to 1 if it "Passes" and 0 otherwise.

Internet Appendix for

"Repurchases for Price Impact: Evidence from Fragile Stocks"

This internet appendix presents additional results to complement those presented in the main manuscript.

Part A presents additional robustness tests.

- Table IA.1 presents the robustness tests for Tables 2 and 3.
- Table IA.2 presents probit regressions to predict treatment status both before and after propensity score matching.
- Table IA.3 presents validation tests for our sample of treatment and control stocks.
- Table IA.4 presents robustness tests to assess possible biases in staggered DiD estimates.
- Table IA.5 presents complimentary evidence from another natural experiment.
- Table IA.6 examines the propensity of treated firms to execute repurchases via tender offers.
- Table IA.7 presents robustness tests for Table 6 using alternative liquidity measures.
- Table IA.8 presents additional estimates for Table 7 on short- and long-run valuation effects with interaction terms for tender offer repurchases.
- Table IA.9 presents additional tests for Table 7 using CARs on repurchase versus non-repurchase days.
- Table IA.10 presents robustness tests for Tables 5 to 9 using an alternative definition of the treatment variable.
- Table IA.11 presents robustness tests for Table 11 using an alternative measure for CEO trading decisions.

Part B presents additional tests and discussion of possible alternative interpretations and alternative empirical designs, as indicated in Section VII.B. This part includes discussion and 3 additional tables:

- Table IA.12 presents additional tests using cross-sectional differences in pre-merger ownership characteristics.
- Table IA.13 presents additional tests to address an alternative interpretation of our results based on changes in firm governance.
- Table IA.14 presents the main results using alternative empirical designs (i.e., assigning the treatment status based on realized changes in fragility rather than the original treatment variable of MSW or using an instrumental variable instead of a difference-in-difference approach).

PART A: Additional Robustness Tests

Table IA.1. Fragility Tests: Robustness

This table presents robustness tests for Table 2 and Table 3. Panel A presents the results of the relation between total volatility, illiquidity, and fragility quintiles. Panel B presents the results of the relation between repurchase, capital expenditures, and fragility quintiles. *Fragility Q5* are the fragility quintiles transformed from the continuous form of fragility computed based on the holdings of all funds in FactSet (i.e., including open-end and non-open-end funds) as in Massa, Schumacher, and Wang (2021). All regressions follow specifications in Table 2 and 3. ***/**/* indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that allow for clustering at the stock level.

Panel A: Fragility quintiles, volatility and liquidity

	(1) Total Volatility	(2) Total Volatility	(3) Total Volatility	(4) Amihud	(5) Amihud	(6) Amihud	(7) Spread	(8) Spread	(9) Spread
Fragility Q5	0.0053***	0.0054***	0.0017**	-0.0362***	-0.0367***	-0.0112***	-0.0006***	-0.0005***	-0.0004***
	(6.14)	(6.95)	(2.18)	(-12.76)	(-12.95)	(-4.93)	(-8.11)	(-8.04)	(-4.81)
Firm size	-0.0255***	-0.0259***	-0.0289***	-0.1130***	-0.1148***	-0.1253***	-0.0024***	-0.0025***	-0.0026***
	(-34.92)	(-37.97)	(-12.04)	(-50.01)	(-50.08)	(-19.58)	(-44.92)	(-45.57)	(-12.78)
Log(B/M)	0.0242***	0.0029**	0.0212***	0.1183***	0.1054***	0.1115***	0.0026***	0.0020***	0.0029***
	(17.10)	(2.20)	(11.65)	(28.77)	(25.02)	(22.82)	(23.55)	(18.65)	(18.47)
Cash flow	-0.1372***	-0.1663***	-0.0815***	-0.0840***	-0.0750***	-0.2459***	-0.0046***	-0.0047***	-0.0048***
	(-20.72)	(-27.88)	(-11.47)	(-4.34)	(-3.86)	(-12.14)	(-7.47)	(-7.78)	(-6.87)
IO	-0.0782***	-0.0915***	-0.0748***	-0.2691***	-0.2712***	-0.1769***	-0.0048***	-0.0055***	-0.0007
	(-12.63)	(-15.85)	(-7.91)	(-14.04)	(-14.06)	(-7.43)	(-10.34)	(-12.09)	(-0.98)
Age	-0.0201***	-0.0190***	-0.0133***	0.0294***	0.0289***	0.0388***	0.0003***	0.0003***	0.0005**
	(-18.56)	(-19.11)	(-4.80)	(9.92)	(9.64)	(5.72)	(4.24)	(3.77)	(2.49)
Cash holdings	0.0410***	0.0230***	-0.0201***	-0.1166***	-0.1248***	-0.0976***	-0.0021***	-0.0026***	-0.0032***
	(10.48)	(6.76)	(-4.80)	(-12.05)	(-12.78)	(-9.86)	(-7.89)	(-9.76)	(-8.67)
Leverage	0.0867***	0.0756***	0.0726***	0.1192***	0.1165***	0.0827***	0.0025***	0.0025***	0.0022***
	(18.25)	(17.52)	(12.36)	(9.40)	(9.18)	(6.08)	(8.03)	(7.99)	(5.02)
Dividend	-0.4248***	-0.5696***	-0.1739***	-0.1220	-0.2161***	-0.4572***	-0.0030	-0.0085***	-0.0064**
	(-14.06)	(-21.07)	(-6.67)	(-1.63)	(-2.87)	(-5.28)	(-1.55)	(-4.42)	(-2.56)
Mom	-0.0337***	0.0208***	0.0225***	-0.0472***	-0.0443***	-0.0367***	-0.0022***	-0.0010***	-0.0006***
	(-25.67)	(17.64)	(19.02)	(-17.24)	(-14.39)	(-13.15)	(-25.34)	(-11.56)	(-6.47)
Country F.E.	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Industry F.E.	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Year F.E.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Stock F.E.	No	No	Yes	No	No	Yes	No	No	Yes
N	61,123	61,123	61,123	61,123	61,123	61,123	61,123	61,123	61,123
adj. R^2	0.27	0.54	0.72	0.37	0.38	0.72	0.23	0.24	0.60

Panel B: Fragility quintiles, repurchase, and CAPEX

	(1) Repurchase	(2) Repurchase	(3) Repurchase	(4) CAPEX	(5) CAPEX	(6) CAPEX
Fragility Q5	-0.0016***	-0.0016***	-0.0012***	0.0036***	0.0037***	0.0015***
	(-9.51)	(-9.40)	(-6.06)	(7.85)	(8.18)	(3.29)
Firm size	0.0003	0.0003*	0.0030***	-0.0059***	-0.0055***	-0.0311***
	(1.54)	(1.82)	(5.05)	(-14.91)	(-13.92)	(-18.00)
Log(B/M)	-0.0078***	-0.0078***	-0.0052***	-0.0086***	-0.0070***	-0.0086***
	(-18.92)	(-18.13)	(-10.83)	(-12.02)	(-9.28)	(-9.27)
Cash flow	0.0345***	0.0341***	0.0164***	0.1008***	0.0972***	0.0556***
	(18.78)	(18.25)	(7.45)	(20.46)	(19.81)	(12.37)
IO	0.0267***	0.0264***	0.0126***	-0.0009	-0.0020	0.0315***
	(16.69)	(16.46)	(4.30)	(-0.27)	(-0.65)	(6.10)
Age	0.0010***	0.0010***	0.0024***	-0.0058***	-0.0058***	-0.0063***
	(4.13)	(4.09)	(3.54)	(-9.05)	(-8.88)	(-3.69)
Cash holdings	0.0002	0.0002	-0.0038***	0.0033	0.0037	0.0138***
	(0.25)	(0.20)	(-3.21)	(0.99)	(1.10)	(3.11)
Leverage	-0.0174***	-0.0174***	-0.0295***	0.0222***	0.0224***	-0.0278***
	(-16.01)	(-16.00)	(-16.77)	(8.04)	(8.12)	(-8.23)
Dividend	-0.0318***	-0.0321***	-0.0202**	-0.1572***	-0.1479***	0.0578***
	(-3.66)	(-3.66)	(-2.00)	(-10.38)	(-9.78)	(3.62)
Mom	-0.0035***	-0.0029***	-0.0012***	0.0027***	0.0063***	0.0001
	(-15.31)	(-10.99)	(-4.48)	(4.16)	(8.12)	(0.09)
Country F.E.	Yes	Yes	No	Yes	Yes	No
Industry F.E.	Yes	Yes	No	Yes	Yes	No
Year F.E.	No	Yes	Yes	No	Yes	Yes
Stock F.E.	No	No	Yes	No	No	Yes
N	61,123	61,123	61,123	61,123	61,123	61,123
adj. R^2	0.17	0.17	0.42	0.25	0.26	0.63

Table IA.2. Propensity Score Matching: Robustness

This table presents additional estimates from a Probit model that predicts treatment status based on observable characteristics both before and after propensity score matching. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively. Standard errors are clustered at the firm level.

	Before matching	After matching
Dependent variable	Treat = 1	Treat = 1
Firm size	0.2225***	0.0271
	(17.55)	(1.34)
Log(B/M)	-0.0259	-0.0155
	(-1.08)	(-0.39)
Cash flow	0.4717***	0.1004
	(3.13)	(0.37)
IO	1.7285***	0.1832
	(17.55)	(1.03)
Age	0.0540***	0.0112
	(2.78)	(0.34)
Cash holdings	0.5134***	-0.0072
	(6.19)	(-0.05)
Leverage	-0.2493***	-0.2337
	(-3.25)	(-1.64)
Dividend	-0.8435	-1.6310
	(-1.32)	(-1.70)
Mom	-0.0175	0.0885
	(-0.62)	(1.63)
Industry F.E.	Yes	Yes
Country F.E.	Yes	Yes
Observations	9,493	4,343
Pseudo R^2	0.190	0.032

Table IA.3. Validation Tests: Changes in Fragility, Illiquidity, and Volatility and Ownership Composition Changes Around the Mergers

This table presents the test results of changes in fragility and illiquidity around the merger events in Panel A. All variables are defined in Table 1 and all regressions follow the specifications in Equation (2) and use $\sqrt{Fragility}$, *Amihud*, *Spread*, or *Volatility* as dependent variables. Panel B presents test results of changes in ownership composition around the merger events. All regressions follow the specification in Columns 1/3/5/7 of Panel A but use ownership variables as dependent variables. *IO* is the total institutional ownership. *IO Short-term* is the ownership of funds with portfolio turnover in the top quartile. *IO Long-term* is the total institutional ownership deducting *IO Short-term*. *Average Portfolio Turnover* is the average portfolio turnover aggregated at the firm-level. ***/**/* indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that allow for clustering at the stock level.

Panel A: Changes in Fragility, Illiquidity, and Volatility

Dep. Var.:	(1) $\sqrt{Fragility}$	(2) $\sqrt{Fragility}$	(3) Amihud	(4) Amihud	(5) Spread	(6) Spread	(7) Total Volatility	(8) Total Volatility
POST x Treat	-0.0231**		0.0133***		0.0008***		-0.0155**	
	(-2.02)		(2.79)		(4.82)		(-2.22)	
Before1 x Treat		-0.0027		-0.0013		-0.0002		-0.0031
		(-0.25)		(-0.17)		(-0.91)		(-0.38)
After1 x Treat		-0.0225*		0.0137***		0.0007***		-0.0162**
		(-1.67)		(2.62)		(4.58)		(-2.10)
After2 x Treat		-0.0254*		0.0117*		0.0006***		-0.0161*
		(-1.68)		(1.87)		(3.35)		(-1.65)
Before1		-0.0269		0.3316***		0.0128***		-0.0727*
		(-0.56)		(5.07)		(8.98)		(-1.91)
After1		-0.1114		0.2201***		0.0031**		0.0199
		(-1.49)		(5.18)		(2.47)		(0.43)
After2		-0.2221**		0.2391***		0.0023		-0.1470**
		(-2.43)		(5.28)		(1.60)		(-2.51)
POST	-0.1317**		0.0694		-0.0043***		0.0096	
	(-2.13)		(1.47)		(-3.17)		(0.25)	
Treat	0.0134*	0.0150	-0.0075***	-0.0068*	-0.0004***	-0.0003**	0.0157***	0.0170***
	(1.68)	(1.47)	(-2.79)	(-1.67)	(-4.21)	(-2.46)	(3.49)	(2.87)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	17,723	17,723	17,723	17,723	17,723	17,723	17,723	17,723
adj. R^2	0.62	0.62	0.55	0.57	0.66	0.68	0.65	0.66

Panel B: Ownership Composition Changes

	(1) IO	(2) IO Short-term	(3) IO Long-term	(4) Average Portfolio Turnover
POST x treat	0.0064	-0.0036***	0.0083**	-0.0032**
	(1.27)	(-3.70)	(2.33)	(-2.15)
Treat	-0.0030	0.0036***	-0.0050*	0.0451***
	(-0.93)	(3.15)	(-1.76)	(13.87)
POST	0.0315	-0.0038	-0.0523***	0.0140
	(1.15)	(-0.64)	(-2.69)	(1.35)
Controls	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
N	17,629	17,629	17,629	16,513
adj. R^2	0.71	0.54	0.96	0.66

Table IA.4. Robustness Test: Assessing biases in staggered difference-in-difference estimates

This table presents robustness tests to assess potential biases arising in staggered difference-in-difference estimation as highlighted in recent literature in econometrics, especially in the presence of heterogeneous treatment effects that can confound the causal interpretation of estimates in the standard two-way fixed effect specification (e.g., Callaway and Sant'Anna (2021), Sun and Abraham (2021)). We examine the main result presented in Table 5, Column 5. Column 1 repeats the specification but limits the sample to the largest asset management merger in the sample, i.e., the merger of BlackRock with BGI in 2009. This merger alone affected such a large cross-section of stocks that we can implement our tests with sufficient empirical power. Given that it is a single event, there are no confounding events from other stocks in the regression. We replicate the main positive effect on share repurchases for this single merger. Columns 2 and 3 repeat the specification of Table 5, and Column 5 but add more stringent firm × event or year × event fixed effects to create a "stacked event study design" and find our results hold. Columns 4 and 5 implement a "stacked" estimator similar to Cengiz et al. (2019) for which we create a dataset for each asset management merger in our matched sample, estimate the main specification separately "event-by-event", and then report the average difference-in-difference estimate across all those "events". Column 4 presents this estimate using the same sample as in Table 5, Column 5 while column 4 further excludes all firms that are designated as "treated" in one merger-event but as "control" in another merger-event with an overlapping event window. Again, our main effect replicates. We conclude that possible biases from heterogenous treatment effects in staggered difference-in-difference designs do not affect our main result.

	(1) Repurchase	(2) Repurchase	(3) Repurchase	(4) Repurchase	(5) Repurchase
POST x Treat	0.0052*	0.0055***	0.0055***	0.0046**	0.0076**
	(1.78)	(4.15)	(4.18)	(2.55)	(2.20)
Treat			-0.0018*		
			(-1.70)		
POST	-0.0506***	-0.0171**			
	(-3.30)	(-2.53)			
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
Firm × Event F.E.		Yes			
Year × Event F.E.			Yes		
N	6,386	17,723	17,723	17,723	14,550
adj. R^2	0.59	0.50	0.59	0.48	0.47

Table IA.5: Complementary Evidence from the 2016 Tick Size Pilot Program

In this table, we present and discuss complementary evidence from 2016 Tick Size Pilot Program to further. Our main results in Table 5 are based on the same sample of natural experiments as in MSW but focus on how treatment status affects a different corporate outcome, particularly share repurchases. Heath et al. (2023) argue that such "reusing" of natural experiments creates a multiple testing problem. These authors recommend different cut-off values for t-statistics. Their recommended cut-off values for t-statistics at the customary 5% confidence level fall in the range of 2.5 to 3.0 when 5 to 20 alternative outcome variables have already been tested, about 3.4 when 100 alternative outcome variables have been tested, and a maximum reported cut-off value of 3.69 when 293 prior tests have used the natural experiment. Among the estimated t-statistics in Table 5, columns 1 to 5, the smallest one is a t-statistic of 3.82 (our column 4) which is well above the maximum cut-off of 3.69 reported in Heath et al. (2023), indicating that our main result is robust to a possible multiple testing bias, even when accounting for the fact that asset management mergers have been used as natural experiments in other studies, not just in MSW. Having said that and as a preview of upcoming tests in which we use some additional outcome variables in similarly structured tests (e.g., Tables 8 and 9), we find that our estimated *t*-statistics are, for the most part, at or above cut-off values if one limits the discussion to the studies that use the merger sample employed here (i.e., MSW and Luo, Manconi, and Schumacher (2023)) with a few instances where they fall slightly below. However, in a broad interpretation of Heath et al. (2023) that would include all studies in which (some) asset management mergers are used as a natural experiment (including different sample, different time periods, different geographies, etc), some of our later tests fall below the recommended cut-off. For example, we use sub-sample tests (Tables 6 and 7) that use a similar specification as Table 5 but on specific sub-samples of e.g., repurchasing firms only. We also modify our specifications, explore different dimensions of cross-sectional heterogeneity to improve testing of specific empirical predictions of our hypothesis (e.g., Tables 10 to 12) or use an alternative definition of our treatment variable (Table IA.10).

Another remedy to assess multiple testing biases from reusing natural experiments, as suggested in Heath et al. (2023), is complementary evidence from an alternative test setting. We seek to provide such evidence from another experiment that past literature has shown to affect repurchase behavior: the 2016 Tick Size Pilot Program in which the Securities and Exchange Commission (SEC) in the US randomly selected 1,200 treatment and 1,199 control stocks. For treated stocks, the program increased their tick size from 1 to 5 cents. Li, Ye, and Zheng (2023, LYZ hereafter) show that this led to a large increase in dealer competition at wider tick sizes for treated firms, which, in turn, led to a sharp reduction in repurchases. We adopt this setting and test if the reduced incentives to repurchase because of an exogenous increase in dealer competition at wider tick sizes has a stronger impact on those firms who a priori benefit less from repurchases: the more fragile firms. Fragile firms are also the ones that have less price impact power to offset the shock to dealer competition, implying that we expect the results of LYZ to be concentrated for fragile firms. We collect the same sample of treated and control stocks and perform two tests, both of which are presented in the table below. Specifically, the sample includes 595 treated and 605 control stocks designated by the tick size program following the sample selection process of Li, Ye, and Zheng (2023) and matching to our sample of US stocks. Columns 1 and 4 use the full sample of treated and control stocks, columns 2 and 5 restrict the sample to "tick-constrained" stocks whose pre-experiment tick sizes were below 5 cent and columns 3 and 6 use the remaining sample of "tick-unconstrained" stocks. The vector *Controls* includes the same control variables as in previous tests and the use of fixed effects is indicated at the bottom of the panel. Columns 4 to 6 augment the specification with a triple interaction with $\sqrt{Fragility_{2016Q2}}$ which is a measurement of each stocks fragility as in Table 2 as of June 2016, the last quarter prior to the implementation of the tick size pilot. All specifications include the same vector of firm level controls as before as well as firm and year fixed effects. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level.

Our findings are as follows: In columns 1 to 3, we first replicate the main finding of LYZ: in column 1, we confirm the negative effect of the treatment status on repurchases. In columns 2 and 3, we split the sample into tick-constrained versus tick-unconstrained firms, where constrained firms are those whose tick size was below 5 cents prior to the pilot program, i.e., firms for whom the treatment status actually had an effect. As in LYZ, we find the result to be entirely concentrated in the tick-constrained sample. In columns 4 to 6 of the same table, we develop

their test along the dimension of interest of our paper and explore cross-sectional heterogeneity in the treatment response for more versus less fragile stocks. We use each stock's fragility as of 2016-Q2 (the last quarter before the start of the pilot program) in the same way as in Table 3 and augment the specification with triple interaction terms. The estimates in column 4 show that the results of LYZ are pronounced for fragile stocks – such firms reduce their repurchases by more. Columns 5 and 6 again confirm that this result is entirely concentrated in the sub-sample of tick-constrained firms. We therefore conclude that fragility impacts the incentives to initiate share repurchases in the direction we expected and in line with our working hypothesis.

Sample	(1) All Stocks	(2) Constrained	(3) Unconstrained	(4) All Stocks	(5) Constrained	(6) Unconstrained
Dep. Var.:	Repurchase	Repurchase	Repurchase	Repurchase	Repurchase	Repurchase
POST x Treat x $\sqrt{Fragility_{2016Q2}}$				-0.0114**	-0.0119**	0.0075
				(-2.00)	(-2.03)	(1.02)
POST x Treat	-0.0031***	-0.0039***	-0.0005	-0.0027***	-0.0031***	-0.0008
	(-3.51)	(-3.55)	(-0.40)	(-2.74)	(-2.72)	(-0.41)
POST x $\sqrt{Fragility_{2016Q2}}$				0.0025*	0.0027**	0.0012
				(1.91)	(2.07)	(0.23)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	16,569	11,654	4,915	14,894	11,650	3,244
adj. R^2	0.44	0.44	0.44	0.44	0.44	0.46

Table IA.6. Propensity to Conduct Tender Offer Repurchases

This table examines the propensity of treated firms to execute their repurchases via tender offers. The sample includes all the share repurchase announcements from SDC Platinum matched to the difference-in-difference sample and the dependent variable $Tender\ Offer_{fdt}$ is an indicator equal if the repurchase is executed via a tender offer and 0 otherwise. All other specifications are unchanged. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level.

Dep. Var.:	(1) Tender Offer	(2) Tender Offer	(3) Tender Offer	(4) Tender Offer	(5) Tender Offer
POST x Treat	0.0231*	0.0253**	0.0241**	0.0147**	0.0157**
	(1.79)	(2.26)	(2.26)	(2.16)	(2.24)
Treat	-0.0119	-0.0094	-0.0082	-0.0117**	-0.0119**
	(-1.00)	(-0.98)	(-0.91)	(-2.01)	(-2.05)
POST	-0.0182	0.0020	0.0029	0.0353	0.0189
	(-1.60)	(0.03)	(0.04)	(0.37)	(0.20)
Controls	No	Yes	Yes	Yes	Yes
Deal F.E.	No	No	No	No	Yes
Firm F.E.	No	No	No	Yes	Yes
Year F.E.	No	No	Yes	No	Yes
N	4,117	4,096	4,096	4,096	4,096
adj. R^2	0.00	0.01	0.02	0.24	0.25

Table IA.7. Liquidity Effects: Using *Turnover* or *Log(Trading Volume)*

This table presents robustness tests for Table 6 but uses *Turnover* (Panel A) or *Log(Trading Volume)* (Panel B) as alternative liquidity measures. All other specifications are unchanged.

Panel A: Turnover

	(1)	(2)	(3)	(4)	(5)
Event Window	1-month	3-month	6-month	12-month	24-month
Dep. Var.	ΔTurnover	ΔTurnover	ΔTurnover	ΔTurnover	ΔTurnover
POST x Treat	0.0000	-0.0006	-0.0005	-0.0003	0.0005
	(0.06)	(-1.34)	(-1.05)	(-0.64)	(0.70)
Treat	-0.0003	0.0005	0.0005*	0.0005	0.0001
	(-0.50)	(1.52)	(1.73)	(1.11)	(0.22)
POST	0.0018	0.0059*	0.0078**	0.0067*	0.0086*
	(0.43)	(1.73)	(2.39)	(1.79)	(1.91)
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	4,089	4,089	4,073	3,982	3,281
adj. R^2	0.24	0.37	0.42	0.45	0.60

Panel B: Log(Trading Volume)

	(1)	(2)	(3)	(4)	(5)
Event Window	1-month	3-month	6-month	12-month	24-month
Dep. Var.	ΔLog(Trade Vol.)	ΔLog(Trade Vol.)	ΔLog(Trade Vol.)	ΔLog(Trade Vol.)	ΔLog(Trade Vol.)
POST x Treat	-0.0311	-0.0817**	-0.0627	-0.0402	0.0231
	(-0.69)	(-2.15)	(-1.64)	(-1.20)	(0.48)
Treat	0.0159	0.0496**	0.0445*	0.0248	0.0068
	(0.54)	(2.06)	(1.75)	(1.00)	(0.23)
POST	-0.2153	0.1507	0.2282	0.3986	0.4311
	(-0.64)	(0.61)	(1.05)	(1.58)	(1.20)
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	4,089	4,089	4,073	3,982	3,281
adj. R^2	0.29	0.40	0.53	0.67	0.72

Table IA.8. Short- and Long-Run Valuation Effects – Interaction terms with *Tender Offer*

This table presents additional estimations to accompany Table 7, Panels A and B. Interaction terms with the *Tender Offer* indicator are added. All other specifications are unchanged.

Panel A: Short-Run Cumulative Abnormal Returns & Tender Offers

	(1)	(2)	(3)	(4)	(5)	(6)
Event Window	1-day	1-day	1-day	3-day	3-day	3-day
Benchm adj.	Market	Industry	DGTW	Market	Industry	DGTW
POST x Treat ×	0.0570***	0.0572***	0.0582***	0.0382*	0.0372*	0.0414**
Tender Offer	(2.88)	(3.81)	(3.10)	(1.80)	(1.90)	(1.98)
Treat × Tender	-0.0615***	-0.0593***	-0.0619***	-0.0583***	-0.0588***	-0.0599***
Offer	(-3.74)	(-4.29)	(-3.95)	(-3.01)	(-3.58)	(-3.21)
Post × Tender	-0.0331**	-0.0331**	-0.0341**	0.0139	0.0182	0.0108
Offer	(-2.24)	(-2.46)	(-2.56)	(1.02)	(1.18)	(0.82)
Tender Offer	0.0411***	0.0412***	0.0411***	0.0207	0.0130	0.0224*
	(2.91)	(3.23)	(3.09)	(1.50)	(1.15)	(1.78)
POST x Treat	0.0046*	0.0061**	0.0056**	0.0088**	0.0083**	0.0099**
	(1.68)	(2.32)	(2.03)	(2.03)	(2.06)	(2.20)
Treat	-0.0019	-0.0029*	-0.0026	-0.0045	-0.0041	-0.0062*
	(-1.00)	(-1.68)	(-1.40)	(-1.44)	(-1.44)	(-1.94)
POST	0.0070	0.0060	0.0098	0.0272	0.0289	0.0340
	(0.31)	(0.29)	(0.44)	(0.80)	(0.86)	(1.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	3,753	3,749	3,704	3,738	3,733	3,690
adj. R^2	0.32	0.32	0.33	0.33	0.31	0.34

Panel B: Long-Run Cumulative Abnormal Returns & Tender Offers

	(1)	(2)	(3)	(4)	(5)	(6)
Horizon	12 months	12 months	12 months	24 months	24 months	24 months
Benchm adj.	Market	Industry	DGTW	Market	Industry	DGTW
POST x Treat ×	0.3265***	0.2389**	0.3458***	0.2979**	0.1966*	0.2839**
Tender Offer	(2.97)	(2.20)	(2.65)	(2.29)	(1.87)	(2.40)
Treat × Tender	-0.0894	-0.0425	-0.0772	-0.1434	-0.0757	-0.1214
Offer	(-0.93)	(-0.50)	(-0.81)	(-1.47)	(-0.87)	(-1.32)
Post × Tender	-0.2439***	-0.1358*	-0.2909***	-0.2812***	-0.1002	-0.2602***
Offer	(-4.06)	(-1.91)	(-3.28)	(-2.97)	(-1.31)	(-2.95)
Tender Offer	0.1028*	0.0156	0.1081**	0.1709**	0.0309	0.1855***
	(1.89)	(0.30)	(2.18)	(2.24)	(0.44)	(2.67)
POST x Treat	0.0719***	0.0420*	0.0620**	0.0672*	0.0616*	0.0552
	(2.70)	(1.74)	(2.26)	(1.87)	(1.81)	(1.36)
Treat	-0.0363*	-0.0155	-0.0365*	-0.0255	-0.0188	-0.0255
	(-1.88)	(-0.89)	(-1.82)	(-0.92)	(-0.74)	(-0.83)
POST	-0.2248	-0.2135	-0.2126	-0.2446	-0.1870	-0.3313
	(-1.41)	(-1.47)	(-1.24)	(-1.09)	(-0.87)	(-1.47)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	4,043	4,043	3,970	3,909	3,909	3,807
adj. R^2	0.46	0.47	0.47	0.54	0.52	0.56

Table IA.9. Valuation Effects: Abnormal returns on Actual Repurchase vs Non-Repurchase Days

To complement the analysis of CARs around repurchase announcement dates of Table 7, we collect information on the actual repurchase days during which firms repurchase their shares. For each repurchase announcement, SDC designates time periods over which each firm performs the actual repurchases. For some firms, this may include several time windows following an announcement, sometimes with gaps in between. We take advantage of this more granular data to examine abnormal returns on actual repurchase days versus non-repurchase days. For 65% of the repurchase announcements, the actual repurchases begin with the announcement date and for another 18% of the announcements, the actual repurchases begin within 1 week of the announcement date. On average, about 80% of trading days in each repurchase program are designated as repurchase days, suggesting that the results of Table 7 are likely dominated by abnormal returns on actual repurchase days, a conjecture we now test. We compute the average daily abnormal returns on the actual repurchase days as well as on the non-repurchase days in the same manner as before. For non-repurchase days, we include a 30-day window prior to the first actual repurchase day and a 30-day window following the last actual repurchase day and also include any non-repurchase days (if any) in between. We estimate the same specification as before, separately for repurchase and non-repurchase days. The table below examines average daily abnormal returns on actual repurchase days (columns 1 to 3) versus non-repurchase days (columns 4 to 6). The sample and specification are as in Table 7 but daily abnormal returns are not computed around the repurchase announcement dates but instead on the actual time periods during which firms repurchase shares. For each repurchase announcement, we compute average daily abnormal returns on actual repurchase days and we also compute average daily abnormal returns on non-repurchase days. For non-repurchase days, we include a 30-day window before the first actual repurchase day and a 30-day window after the last repurchase day and also include non-repurchase days in between actual repurchase days (if any) as companies at times execute repurchases over several time periods with non-repurchasing windows in between. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level respectively, computed from standard errors that cluster at the firm level. We find significant positive average daily abnormal returns for treated firms in the post-merger periods on repurchase days only (columns 1 to 3) but no effect during non-repurchase days (columns 4 to 6 of the same table). This substantiates our conjecture that the results in Table 7 are driven by repurchase days.

Sample	**Repurchase Days**			**Non-Repurchase Days**		
	(1)	(2)	(3)	(4)	(5)	(6)
Benchm adj.	Market	Industry	DGTW	Market	Industry	DGTW
POST x Treat	0.0009***	0.0008**	0.0009***	-0.0007	-0.0005	-0.0007
	(2.63)	(2.28)	(2.88)	(-1.61)	(-1.30)	(-1.59)
Treat	-0.0008*	-0.0006	-0.0008*	0.0007**	0.0004	0.0007**
	(-1.69)	(-1.46)	(-1.81)	(2.37)	(1.61)	(2.42)
POST	-0.0039*	-0.0029	-0.0038*	0.0022	0.0011	-0.0000
	(-1.88)	(-1.39)	(-1.84)	(0.75)	(0.45)	(-0.01)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	3,765	3,764	3,724	3,464	3,463	3,435
adj. R^2	0.16	0.15	0.16	0.33	0.31	0.34

Table IA.10. Robustness Tests: Alternative Treatment Definition

This table presents robustness tests for the main results presented in Sections IV and V (Tables 5 to 9). Stocks are now assigned to the treatment group based on the alternative treatment variable $IO\ Total_{df} = IO\ Acq_{df} + IO\ Targ_{df}$ and the control group is then re-constructed accordingly following the same procedure as in the main test. All other specifications are unchanged.

Panel A: Table 5, Panel A with alternative treatment definition

	(1) Repurchase	(2) Repurchase	(3) Repurchase	(4) Repurchase	(5) Repurchase	(6) Repurchase
POST x Treat	0.0050***	0.0043***	0.0042***	0.0046***	0.0046***	
	(3.64)	(2.85)	(2.86)	(2.93)	(3.00)	
Before1 x Treat						0.0026
						(1.31)
After1 x Treat						0.0060***
						(2.76)
After2 x Treat						0.0062***
						(2.75)
Before1						0.0182***
						(3.31)
After1						-0.0018
						(-0.19)
After2						-0.0019
						(-0.17)
POST	-0.0036***	-0.0078*	0.0046	-0.0112*	-0.0168**	
	(-3.20)	(-1.75)	(0.91)	(-1.87)	(-2.44)	
Treat	-0.0023	-0.0032**	-0.0032**	-0.0022*	-0.0028**	-0.0042**
	(-1.53)	(-2.28)	(-2.26)	(-1.88)	(-2.34)	(-2.41)
Controls	No	Yes	Yes	Yes	Yes	Yes
Deal F.E.	No	No	No	No	Yes	Yes
Firm F.E.	No	No	No	Yes	Yes	Yes
Year F.E.	No	No	Yes	No	Yes	Yes
N	20,968	19,313	19,313	19,313	19,313	19,313
adj. R^2	0.00	0.21	0.22	0.54	0.55	0.55

Panel B: Table 5, Panel B with alternative treatment definition

	(1) Dividend	(2) Dividend	(3) Dividend	(4) Dividend	(5) Dividend	(6) Dividend
POST x Treat	0.0010	0.0007	0.0007	0.0009	0.0009	
	(1.20)	(0.82)	(0.85)	(1.06)	(1.13)	
Before1 x Treat						-0.0010
						(-1.37)
After1 x Treat						-0.0001
						(-0.14)
After2 x Treat						0.0009
						(0.82)
Before1						0.0004
						(0.17)
After1						-0.0021
						(-0.36)
After2						-0.0043
						(-0.59)
POST	-0.0009	-0.0006	-0.0020	-0.0104**	-0.0042	
	(-1.18)	(-0.17)	(-0.49)	(-2.20)	(-0.76)	
Treat	-0.0011	-0.0004	-0.0004	-0.0012**	-0.0010*	-0.0005
	(-1.06)	(-0.43)	(-0.46)	(-2.05)	(-1.71)	(-0.68)
Controls	No	Yes	Yes	Yes	Yes	Yes

Deal F.E.	No	No	No	No	Yes	Yes
Firm F.E.	No	No	No	Yes	Yes	Yes
Year F.E.	No	No	Yes	No	Yes	Yes
Observations	20,968	19,313	19,313	19,313	19,313	19,313
Adjusted R^2	0.00	0.23	0.23	0.72	0.72	0.72

Panel C: Table 6, Panel A with alternative treatment definition

	(1)	(2)	(3)	(4)	(5)
Event Window	1-month	3-month	6-month	12-month	24-month
Dep. Var.	ΔAmihud	ΔAmihud	ΔAmihud	ΔAmihud	ΔAmihud
POST x Treat	-0.0000	0.0000	0.0000	0.0000	0.0000
	(-0.16)	(0.34)	(0.81)	(1.52)	(0.31)
Treat	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(-0.41)	(-1.15)	(-1.47)	(-1.46)	(-0.65)
POST	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(-0.66)	(-1.30)	(-1.10)	(-1.06)	(-1.06)
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	4,258	4,252	4,242	4,136	3,426
adj. R^2	0.92	0.91	0.91	0.84	0.92

Panel D: Table 7, Panel A with alternative treatment definition

	(1)	(2)	(3)	(4)	(5)	(6)
Event Window	1-day	1-day	1-day	3-day	3-day	3-day
Benchm adj.	Market	Industry	DGTW	Market	Industry	DGTW
POST x Treat	0.0062**	0.0079***	0.0068**	0.0117***	0.0127***	0.0123***
	(2.19)	(2.89)	(2.33)	(2.70)	(3.12)	(2.75)
Treat	-0.0041*	-0.0051***	-0.0046**	-0.0051*	-0.0052*	-0.0056*
	(-1.96)	(-2.59)	(-2.03)	(-1.65)	(-1.74)	(-1.74)
POST	-0.0096	-0.0193	-0.0111	0.0307	0.0253	0.0339
	(-0.45)	(-0.91)	(-0.52)	(1.06)	(0.89)	(1.16)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	3,901	3,906	3,857	3,892	3,890	3,852
adj. R^2	0.62	0.63	0.61	0.47	0.47	0.48

Panel E: Table 7, Panel B with alternative treatment definition

	(1)	(2)	(3)	(4)	(5)	(6)
Horizon	12 months	12 months	12 months	24 months	24 months	24 months
Benchm adj.	Market	Industry	DGTW	Market	Industry	DGTW
POST x Treat	0.0462*	0.0490*	0.0425	0.0915**	0.1136***	0.0858**
	(1.77)	(1.82)	(1.55)	(2.41)	(2.83)	(2.05)
Treat	-0.0218	-0.0213	-0.0215	-0.0577*	-0.0718**	-0.0516*
	(-1.11)	(-1.07)	(-1.05)	(-1.94)	(-2.42)	(-1.66)
POST	0.0054	0.0532	-0.0139	-0.1167	0.0811	-0.1498
	(0.03)	(0.29)	(-0.07)	(-0.38)	(0.28)	(-0.46)

Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	4,201	4,201	4,128	4,041	4,039	3,936
adj. R^2	0.47	0.46	0.47	0.54	0.51	0.56

Panel F: Table 8 with alternative treatment definition

	(1) Equityiss	(2) Chgstdebt	(3) Chgltdebt	(4) Chglev	(5) Chgcash
POST x Treat	-0.0048**	0.0005	0.0016	-0.0000	-0.0008
	(-2.22)	(0.65)	(0.52)	(-0.02)	(-0.33)
Treat	0.0012	-0.0003	-0.0015	0.0002	0.0013
	(0.79)	(-0.46)	(-0.66)	(0.25)	(0.61)
POST	0.0172	-0.0071	-0.0246	-0.0072	0.0204
	(1.32)	(-1.57)	(-1.52)	(-1.38)	(1.35)
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	19,313	19,313	19,313	19,313	19,313
adj. R^2	0.44	0.07	0.27	0.07	0.35

Panel J: Table 9 with alternative treatment definition

Dependent variable	(1) CAPEX	(2) CAPEX	(3) CAPEX	(4) CAPEX	(5) CAPEX	(6) Total Investment	(7) Total Asset Growth
POST x	-0.0034**	-0.0050**	-0.0049**	-0.0044***	-0.0043***	-0.0057***	-0.0002
Treat	(-2.48)	(-2.42)	(-2.38)	(-3.00)	(-2.95)	(-2.70)	(-0.03)
POST	-0.0052***	0.0023	0.0055	-0.0040	-0.0040	-0.0004	-0.0666
	(-4.14)	(0.31)	(0.74)	(-0.64)	(-0.57)	(-0.03)	(-1.49)
Treat	-0.0002	0.0006	0.0006	0.0023**	0.0023**	0.0019	-0.0068
	(-0.10)	(0.30)	(0.26)	(2.30)	(2.32)	(1.28)	(-1.00)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	No	No	No	No	Yes	Yes	Yes
Firm F.E.	No	No	No	Yes	Yes	Yes	Yes
Year F.E.	No	No	Yes	No	Yes	Yes	Yes
N	20,968	19,313	19,313	19,313	19,313	19,313	19,313
adj. R^2	0.00	0.09	0.10	0.77	0.77	0.71	0.51

Table IA.11. CEO Trading Decisions

This table repeats the estimation from Table 11 but uses as the dependent variable *Option Exer*, defined as the option value exercised by CEOs in a given year divided by the total market capitalization of the firm. All other specifications are unchanged.

	(1) Option Exer	(2) Option Exer	(3) Option Exer	(4) Option Exer	(5) Option Exer
POST x Treat	0.0002	0.0004*	0.0004**	0.0004*	0.0004*
	(1.31)	(1.93)	(1.98)	(1.93)	(1.93)
Treat	-0.0002	-0.0003*	-0.0004*	-0.0002	-0.0002
	(-0.85)	(-1.79)	(-1.83)	(-1.51)	(-1.35)
POST	-0.0010***	-0.0024**	-0.0018*	-0.0022*	-0.0030**
	(-5.75)	(-2.49)	(-1.80)	(-1.79)	(-2.29)
Controls	No	Yes	Yes	Yes	Yes
Deal F.E.	No	No	No	No	Yes
Firm F.E.	No	No	No	Yes	Yes
Year F.E.	No	No	Yes	No	Yes
N	8,282	8,250	8,250	8,250	8,250
adj. R^2	0.02	0.08	0.08	0.27	0.28

PART B: Discussion and additional tests of alternative explanations and alternative econometric designs

As indicated in Section VII.B, we present additional discussion and tests to rule out 3 alternative interpretations that could drive our results and also present alternative econometric designs.

Alternative Explanation 1: Cost of Capital Changes

One alternative interpretation of our results states that the changes in corporate policies are not induced by changes in liquidity that accompany changes in fragility but instead by changes in the firm's cost of capital: lower liquidity increases the cost of capital of treated firms (e.g., Amihud and Mendelson 1986, Acharya and Pederson 2005) which leads to a reduction in investment. Indeed, MSW not only document a decline in liquidity for treated firms but also a negative valuation effect. In turn, a reduction in investment then frees up cash and as such, treated firms increase repurchases to return excess cash to shareholders.

We argue that such a cost of capital channel appears unlikely for several reasons. First, the economic magnitudes of the results in MSW do not align with the valuation effects of the share repurchases we document here. Specifically, the negative stock price effects for treated firms that MSW document around these asset management mergers amount to about 0.5% - 1% over a 2-day event window using comparable benchmark-adjusted returns as we use here (their Table 6, Panel C) with no evidence of longer-run price declines after. In contrast, the repurchase announcements we document generate about the same positive abnormal returns upon the announcement and additional 4%-7% long-run positive returns which would seem to heavily overcompensate for the increase in the cost of capital suffered by treated firms. Second, while a higher cost of capital might explain a drop in investment, it is not clear that it will lead firms to immediately return cash to shareholders via repurchases. In fact, a higher cost of capital might

lead firms to accumulate cash instead in order to avoid having to raise financing in the future at a higher cost of capital. Our results in Table 8 show that this is not the case. Third, and related to the previous point, our results in Table 10 indicate that repurchases increase more for growth firms with low book-to-market ratios. But these are firms with presumably strong growth opportunities for which we would not necessarily expect that a lack of investment opportunities motivates them to return cash to shareholders. In fact, past literature indicates that especially firms with low book-to-market ratios would prefer accumulating cash holdings (e.g., Opler et al. 1999). Fourth, the results in section VII.A indicate that regulatory price constraints mediate our main results at least for non-US firms, substantiating our price impact channel rather than a cost of capital alternative.

Alternative Explanation 2: Common Ownership Changes

Recent studies link increases in aggregate repurchases and declines in corporate investment to changes in common ownership or changes in the market power of firms (Gutierrez and Philippon 2017, Lee, Shin, and Stulz 2021). Our results are not consistent with this alternative channel because the setting of asset management mergers was shown by MSW to leave the level of institutional ownership unchanged. Furthermore, the ensuing reduction in financial fragility makes it unlikely that an increase in repurchases may be driven by increases in ownership concentration as such an increase is inconsistent with a decline in stock price fragility (Greenwood and Thesmar 2011). Nevertheless, as a further robustness check, we perform several tests to substantiate that our results are indeed driven by a channel that operates via changes in financial fragility rather than a mere change in the level of ownership.

We use the cross-sectional heterogeneity within treated firms along pre-merger ownership characteristics to demonstrate that changes in fragility are the channel through which our

argument operates, not mere changes in common ownership. We use four pre-merger ownership characteristics identified by MSW as leading to more aggressive portfolio rebalancing (and therefore stronger effects on fragility). These characteristics are described in the caption to Table IA.12 below. We then show that treated firms that score high on those characteristics more strongly increase repurchases (Table IA.12, Panel A) and reduce investment (Table IA.12, Panel B).

Alternative Explanation 3: Governance Changes

Finally, we seek to rule out one last interpretation of our results, namely that changes in repurchase behavior are not driven by changes in fragility but directly by changes in firm governance that may accompany the changes in institutional ownership composition that treated firms experience. We provide two empirical tests. First, if changes in payout are driven by changes in governance, we expect these changes to be particularly strong for firms with poor corporate governance prior to the merger events. Therefore, we define 3 different measures of firm-level governance: First, the natural logarithm of the number of block holders (defined as institutions that hold more than 5% of shares outstanding), second, the G-Index of Gompers, Ishii, and Metrick (2003), and third, the E-Index of Bebchuk et al. (2009). Then, we repeat the previous specifications with triple interaction terms for these governance measures. We present the results in Internet Appendix, Table IA.13, Panel A. In short, we find no evidence that the increase in share repurchases is particularly strong for treated firms with poor corporate governance indicators. All the triple interaction terms are statistically insignificant.

In a second test, we directly examine shareholder participation at the firms' annual meetings to examine if the change in the composition of institutional ownership results in more "voice" being exercised during – e.g., corporate voting. Crane, Michenaud, and Weston (2014) find that

changes in payout seem at least in part to be driven by changes in shareholder proposals and voting patterns. As such, we seek to test whether changes in shareholder proposals and voting patterns would lead to changes in payout. To implement this test, we bring in additional data on shareholder proposals and voting patterns from the ISS/RiskMetrics database. We match our treated and control firms to this data to construct measures of shareholder participation and voting from the records at the annual meetings of our treated and control firms. In total, we map approximately 60% of our treated and control firms to this data and we define the following new outcome variables: $\%ShareholderProposals_{ft}$, defined as the number of shareholder proposals divided by the total number of proposals up for voting at the annual meeting of firm *f* in year *t*, $Participation_{ft}$, defined as the average number of ballots cases divided by shares outstanding across all proposals voting at the annual meeting of firm *f* in year *t*, $\%AgainstMgmt_{ft}$, defined as the average vote share against management across all proposals at the annual meeting of firm *f* in year *t*, and $\%Pass_{ft}$ as the fraction of management proposals that "pass" at the annual meeting of firm *f* in year *t*. We then re-estimate equation (2) with these different outcome variables and present the results in Panel B of Table IA.13. Across all of these measures, we find no significant change in voting or shareholder participation for treated versus control firms in the post-merger periods, so we conclude that our results are unlikely driven by changes in governance that could accompany the changes in ownership structure induced by these asset management mergers.

Alternative Empirical Designs

We implement and discuss alternative empirical designs to further assess robustness of our main results. We refer to the discussion of those in the caption of Table IA.14.

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Table IA.12. Cross-sectional Heterogeneity by Pre-Merger Ownership Characteristics

This table examines changes in payout policies (Panel A) and investment (Panel B) for firms with different ownership characteristics before the mergers. MSW identify that these specific ownership characteristics lead to more aggressive portfolio rebalancing and therefore stronger reductions in fragility and liquidity. Such pre-merger ownership characteristics of treated firms include ownership by funds with open-end structures, funds with volatile and correlated flows, as well as funds with concentrated positions in a stock. Funds with these characteristics have the highest exposure to changes in financial fragility as per Greenwood and Thesmar (2011). As a result, MSW find that these funds rebalance their portfolio the most following the announcements of asset management mergers. We now test if stocks with high pre-merger ownership by funds with these same characteristics experience a stronger increase in share repurchases and a stronger decrease in capital expenditures, substantiating that our documented effects are not just driven by changes in ownership but by changes in ownership with a particular impact on financial fragility. The specification is as in Table 5, Panel A but augmented with triple interaction terms for specific ownership characteristics. Pre-merger ownership characteristics are identified in Massa, Schumacher, and Wang (2021) and include: *IO_oef* as the ownership by open-ended funds, *IO_flowvola* as the ownership of funds that are in the top quartile of three-year average monthly flow volatility, where fund flow volatility is the standard deviation of monthly flows over the past three years, *Flow correlation* as the position weighted-average flow correlation of each pair of funds that hold the stock, and *IO_excessweight* as the ownership of funds that overweight the stock relative to their benchmark, i.e., funds in the top quartile of the excess weight in the stock. All other specifications are unchanged. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level, computed from standard errors clustered at the firm level.

Panel A: Repurchases

Dep. Var.:	(1) Repurchase	(2) Repurchase	(3) Repurchase	(4) Repurchase	(5) Repurchase
POST x Treat x IO_oef	0.0430***				0.0295*
	(3.36)				(1.74)
POST x Treat x IO_excessweight		0.0620***			0.0288
		(2.94)			(1.03)
POST x Treat x IO_flowvola			0.3648***		0.2453**
			(3.29)		(2.02)
POST x Treat x Flow correlation				0.0543*	0.0495
				(1.66)	(1.55)
Treat	0.0042*	0.0009	-0.0018	-0.0034**	0.0017
	(1.82)	(0.65)	(-1.22)	(-2.19)	(0.62)
POST x Treat	-0.0041*	0.0009	0.0015	0.0033*	-0.0079***
	(-1.66)	(0.54)	(0.80)	(1.70)	(-2.73)

POST	-0.0132*	-0.0142**	-0.0179***	-0.0161**	-0.0106
	(-1.90)	(-2.13)	(-2.72)	(-2.40)	(-1.54)
Dual-interactions	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	17,723	17,723	17,723	17,723	17,723
adj. R^2	0.58	0.58	0.58	0.58	0.59

Panel B: CAPEX

	(1) CAPEX	(2) CAPEX	(3) CAPEX	(4) CAPEX	(5) CAPEX
POST x Treat x IO_oef	-0.0265*				0.0170
	(-1.86)				(1.00)
POST x Treat x IO_excessweight		-0.0540**			-0.0858***
		(-2.23)			(-2.87)
POST x Treat x IO_flowvola			-0.4395***		-0.4176***
			(-3.13)		(-3.02)
POST x Treat x Flow correlation				0.0051	-0.0131
				(0.13)	(-0.33)
Treat	0.0043*	0.0001	-0.0009	0.0019	0.0025
	(1.69)	(0.05)	(-0.57)	(1.18)	(0.84)
POST x Treat	0.0022	0.0008	0.0014	-0.0037*	0.0041
	(0.65)	(0.33)	(0.63)	(-1.65)	(1.04)
POST	-0.0200***	-0.0138*	-0.0162**	-0.0117*	-0.0212***
	(-2.69)	(-1.95)	(-2.39)	(-1.70)	(-2.74)
Dual-interactions	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes
N	17,723	17,723	17,723	17,723	17,723
adj. R^2	0.75	0.74	0.74	0.74	0.75

Table IA.13. Changes in Payout Policy, Investment Policy, and Corporate Governance

This table examines if changes in payout and investment policies are driven by governance changes. Panel A presents specifications as in Table 5 that are augmented with triple interaction terms for firm-level governance characteristics prior to the mergers. These governance characteristics include: *Logn_blckholder* is the log number of institutions that hold more than 5 percent of the total shares outstanding, *Gindex* is the governance index from Gompers, Ishii, and Metrick (2003), and *Eindex* is the entrenchment index is from Bebchuk et al. (2009). Both *Gindex* and *Eindex* are obtained from ISS/RiskMetrics. Panel B examines if changes in ownership concentration induced by the mergers impacts shareholder proposals or shareholder voting outcomes. The specifications are as in Table 5, column 5 but use different dependent variables to measure various outcomes related to management or shareholder proposals and voting behavior. These dependent variables include: *%ShareholderProposal* is the total number of shareholder proposals scaled by the total number of proposals in the given firm-year, *Participation* is the total number of ballots divided by total share outstanding, averaged across all proposals in a given firm-year, *%AgainstMgmt* is the average percentage of votes against management proposals in any firm-year, and *%Pass* is the fraction of management proposals that "Pass" in each firm-year. All other specifications are unchanged. * / ** / *** indicate statistical significance at the 10% / 5% / 1% level, computed from firm-level clustered standard errors.

Panel A: Cross-sectional Heterogeneity in Repurchases by Pre-Merger Firm Governance

	(1) Repurchase	(2) Repurchase	(3) Repurchase
POST x Treat x logn_blckholder	0.0053		
	(1.54)		
POST x Treat x Gindex		-0.0008	
		(-0.92)	
POST x Treat x Eindex			-0.0013
			(-0.69)
Treat	0.0032	-0.0075**	-0.0011
	(0.88)	(-2.00)	(-0.24)
POST x Treat	-0.0002	0.0125**	0.0122**
	(-0.07)	(2.28)	(2.02)
POST	-0.0178***	-0.0276	-0.0287*
	(-2.59)	(-1.62)	(-1.74)
Dual-interactions	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes
N	16,245	9,091	9,091
adj. R^2	0.60	0.60	0.60

Panel B: Shareholder proposals and voting outcomes

	(1) %Shareholder Proposals	(2) Participation	(3) %AgainstMgmt	(4) %Pass
POST x Treat	0.0029	-0.0033	-0.0027	-0.0069
	(0.70)	(-0.37)	(-0.80)	(-0.72)
Treat	-0.0018	0.0043	0.0002	0.0044
	(-0.57)	(0.76)	(0.08)	(0.72)
POST	-0.0323	0.0299	0.0628***	0.0821
	(-1.35)	(0.57)	(3.45)	(1.50)
Controls	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
N	10,518	10,482	10,300	10,516
adj. R^2	0.54	0.39	0.30	0.31

Table IA.14. Alternative Empirical Designs

This table presents estimates of alternative empirical designs to substantiate that changes in repurchase and investment behavior are driven by changes in fragility from the mergers between asset management firms. Panel A repeats the main specifications of Tables 5, 8, and 9 but creates the matching sample by assigning treatment status based on the realized changes in fragility that stocks experience as a result of these mergers. The approach borrows from studies such as Fang, Tian, and Tice (2014) who investigate how changes in liquidity affect firm innovation and who assign treatment status based on changes in liquidity that are caused by decimalization on US stock exchanges rather than the decimalization experiment directly. We implement a similar approach and sort stocks into quintiles based on the realized change in fragility around the mergers. We then reconstruct the matched sample based on this alternative and repeat the main specifications. All other aspects of the sample construction are otherwise unchanged but we highlight that all estimated coefficients here are expected to have opposite signs of those reported in Tables 5, 8, and 9 because the stocks with the *highest* pre-merger ownership of buyer and target funds are expected to experience the *lowest* (i.e., most negative) change in fragility (i.e., MSW (2021) find that the mergers lead to a *reduction* in financial fragility). We find analogous results to our main results: stocks with the most negative change in realized fragility (i.e., stocks that correspond to the highest pre-merger ownership as per MSW) experience an increase in repurchases, no change in dividends, and a decline in investment. Panel B implements an instrumental variable approach in the cross-section of treated and control stocks (defined as per the original treatment definition as in the rest of the paper) but we use the treatment status in the first stage to instrument the change in fragility that stocks experience between the pre- and the post-merger periods (column 1). The remaining columns then present the second-stage IV estimates of regressions of changes in the key outcome variables on the instrumented changes in fragility around the mergers. We document that, in line with our working hypothesis, an increase in fragility has a negative effect on repurchases, no effect on dividends, and a positive effect on investment policy.

Panel A: Assigning Treatment Status based on observed change in Fragility

	(1) Repurchase	(2) Dividend	(3) CAPEX	(4) Total Investment	(5) Asset Growth	(6) Equityiss
POST x Treat	-0.0044***	0.0003	0.0048***	0.0084***	0.0166*	0.0048*
	(-3.36)	(0.36)	(2.69)	(2.75)	(1.69)	(1.80)
Treat	0.0009	-0.0006	-0.0035**	-0.0089***	-0.0126	-0.0037
	(0.54)	(-0.61)	(-2.56)	(-3.23)	(-1.23)	(-1.60)
POST	-0.0025	-0.0009	-0.0038	-0.0053	-0.0107	0.0393**
	(-0.50)	(-0.22)	(-0.48)	(-0.39)	(-0.22)	(2.08)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Deal F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	22,155	22,155	22,150	22,150	22,150	22,150
adj. R^2	0.56	0.74	0.72	0.60	0.49	0.56

Panel B: Instrumental Variable Approach

Equation	(1) First Stage $\Delta\sqrt{Fragility}$	(2) Second Stage Δ Repurchase	(3) Second Stage Δ Dividend	(4) Second Stage Δ CAPEX	(5) Second Stage Δ Total Inv.	(6) Second Stage Δ Asset Growth
$\Delta\sqrt{Fragility}$ (IV)		-0.1890**	-0.0010	0.1117*	0.1599*	0.6548*
		(-2.33)	(-0.16)	(1.89)	(1.91)	(1.94)
Treat	-0.0245**					
	(-2.43)					
Δ Firm size	0.0086	0.0044	-0.0001	-0.0242***	-0.0458***	-0.1998***
	(0.46)	(1.06)	(-0.32)	(-6.28)	(-9.04)	(-8.40)
Δ Log(B/M)	0.0054	-0.0108***	-0.0007**	-0.0052**	-0.0077**	0.0026
	(0.39)	(-3.58)	(-2.35)	(-2.22)	(-2.32)	(0.18)
Δ Cashflow	0.1433*	0.0775***	0.0112***	0.0652***	0.1373***	0.7115***
	(1.75)	(3.99)	(5.04)	(3.25)	(5.08)	(6.58)
Δ IO	0.7898***	0.1357**	0.0005	-0.0778*	-0.1101*	-0.4102
	(12.66)	(2.22)	(0.10)	(-1.72)	(-1.72)	(-1.61)
Δ Age	0.0089	0.0051	0.0005	0.0012	0.0034	-0.0224
	(0.42)	(1.17)	(1.41)	(0.35)	(0.73)	(-1.12)
Δ Cash holdings	0.0307	0.0070	-0.0009	-0.0088	-0.0261	0.4521***
	(0.69)	(0.60)	(-0.90)	(-0.83)	(-1.64)	(8.68)
Δ Leverage	-0.0629	-0.0512***	-0.0037***	-0.0176*	-0.0348**	-0.0207
	(-1.10)	(-3.67)	(-3.56)	(-1.74)	(-2.56)	(-0.35)
Δ Dividend	-0.5601*	-0.1807**	0.5634***	0.0818	0.0140	-0.0227
	(-1.85)	(-2.37)	(16.64)	(1.31)	(0.15)	(-0.07)
Δ Mom	0.0177	-0.0072	-0.0001	-0.0087***	-0.0073	0.0704***
	(0.90)	(-1.64)	(-0.24)	(-2.70)	(-1.54)	(3.37)
N	4,581	4,581	4,581	4,581	4,581	4,581
adj. R^2	0.182					
F statistic	8.07					
p-value	0.005					