Spillover Effects of Payouts on Asset Prices and Real Investment*

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Abstract

This paper uses the reinvestment of corporate payouts by financial institutions as a nonfundamental shock to prices of other stocks held in the same portfolio. Exploiting the separation between announcement and payment dates, we find dividends, in particular, generate payment date price pressure, but no announcement date news spillovers. We estimate an asset demand elasticity of 1.25 and document a releveraging market feedback effect on investment, where firms respond to price increases by issuing debt and use the funds to invest. Through this mechanism, \$10 paid in dividends by the average firm translates into \$2 of investment at other firms.

Keywords
Institutional Investors, Price Pressure, Market Feedback Effects, Corporate Payouts

JEL Codes
G11; G12; G23

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1 Introduction

Asset demand shocks that are plausibly exogenous to firm fundamentals are the basis for many important findings in empirical finance research. Asset demand shocks, interpreted as residual supply shocks, allow researchers to identify asset demand elasticities, shedding light on the extensive debate about whether stocks are perfect substitutes, a core implication of the efficient market hypothesis (Shleifer, 1986; Harris and Gurel, 1986). Asset demand elasticities are also necessary inputs for counterfactual experiments that investigate the role of institutions in financial markets (Koijen and Yogo, 2019). Moreover, by generating variation in asset prices that is unrelated to firm fundamentals, asset demand shocks create a laboratory to study market feedback effects — the causal effects of stock prices on the real economy (surveyed in Bond, Edmans, and Goldstein, 2012; Baker and Wurgler, 2013).

The literature uses two prominent asset demand shocks to identify asset demand elasticities and market feedback effects: index additions/deletions (e.g. Chang, Hong, and Liskovich, 2015) and mutual fund flow-induced fire sales (e.g. Edmans, Goldstein, and Jiang, 2012). Index additions/deletions cleanly identify the pricing effects of index membership, but in addition to triggering trades by indexers, index membership may also affect liquidity and corporate governance. Mutual fund flow-induced fire sales are intuitively appealing shocks, but their pricing effects could potentially be driven by reverse causality, as examined in Schmickler (2020).

In this paper, we introduce an alternative asset demand shock that addresses these drawbacks and reveals a relatively unknown feature in stock markets. Corporate payouts not only affect a company's own stock through reinvestment, but other stocks held in the same portfolio by investors. This insight enables us to identify the slope of the demand curve for stocks and then study how a nonfundamental increase in asset prices affects corporate financing and investment. Though our analysis exploits dividends, M&A, and share repurchases as payout sources, our approach instruments returns ultimately with dividends as these provide the cleanest setting and most credible identification strategy.

We summarize our demand shock and this paper's mechanism in Figure 1. Vanguard Equity Income Fund holds Pfizer and Lyft stocks in its portfolio. At t_0, Pfizer announces a cash dividend. This reveals fundamental information about Pfizer and potentially also about peer firms such as Johnson & Johnson. When the dividend is paid 45 days later, Vanguard receives the cash and reinvests into its portfolio. We call this demand pressure *payout-induced trading*. This demand shock pushes up Lyft's price relative to industry peers not held in the same portfolio — for example, Uber. Then, in response to the lower cost of capital, Lyft increases investment.

In order for this mechanism to work, the following set of conditions must hold robustly in the data — which we show is the case. Dividend payments must occur with sufficient lag from the payment announcement date, when fundamental information is arguably revealed. Then, institutions need to reinvest the cash into existing portfolio stocks. If they don't reinvest the cash, there is no shock. If they use the cash to rebalance away from existing holdings, buying pressure will be endogenous and not a valid source of variation for those stocks — at the same time, payouts will not be relevant as a shock to existing holdings. But even if all institutions reinvested 100% of payouts into existing holdings, they cannot all hold the market portfolio. If they did, there would be no cross-sectional variation in treatment exposure across stocks. Finally, when Lyft receives a demand shock from dividends paid by Pfizer, its stock must experience abnormal returns.

US public companies pay out almost 6% of total market equity in cash to shareholders via dividends, share repurchases, and M&A deals. Dollar total cash payouts peaked in 2018, at almost $2 trillion. Households largely consume cash payouts (Baker, Nagel, and Wurgler, 2007), but equities are primarily held by financial institutions, which do not consume and do not immediately pass-through payouts to households. Instead, institutional investors are likely to reinvest payouts, because cash is unproductive.

Accordingly, we find that financial institutions reinvest into their average portfolios following dividend payouts. This involves the payout stock (as in Kvamvold and Lindset (2018) and Chen (2020)), but also the other existing holdings. For mutual funds, a 1% payout is followed by a 0.65% increase in the position of *existing* portfolio stocks that did not issue payouts in that quarter. Across other institution types, we find extensive evidence of reinvestment — including for example asset managers, insurance companies, and endowments. Reinvestment follows not only corporate dividends, but also M&A cash payouts.

This intensive margin response is consistent with the fact that, since cash payouts do not increase the supply of shares, shareholders cannot all completely reinvest into the payout stock. Instead, to avoid falling behind their benchmarks, most institutional investors must deploy cash on hand into non-payout assets. Most of the increase in trading then is directed to already-held stocks rather than initiating new positions.

Underpinning the validity of payout-induced trading, we show that most financial institutions hold concentrated stock portfolios — at the 90th percentile, institutional portfolios have as little as 10% of the market depending on the investor type. Dividend announcement and payment dates also lag considerably — on average payouts occur 45 days after announced, plausibly creating enough distance between news effects and potential demand pressure. That is because, once announced, dividends are deterministic. Combined with the fact that 30 stocks pay dividends on a given day, many stocks are close to untreated and some stocks receive shocks with varying intensity relative to what they would receive were they held by only one investor.

We test whether payouts generate news and/or price pressure effects in an exhaustive analysis of returns around payout events. For each type of payout, we show daily abnormal returns of the firm itself, 3-digit SIC code industry peers, and stocks held in the same portfolio around announcements and payment dates. Our demand shock aggregates the hypothetical number of shares institutions buy in response to payout flows, if institutions reinvest in proportion to portfolio weights. This construction uses the idea from the mutual fund flow-induced fire sale literature that *hypothetical* trades in proportion to existing portfolio weights are likely exogenous to firm fundamentals, while actual trades may well be driven by fundamentals (e.g. Edmans, Goldstein, and Jiang, 2012). We construct this shock using US equity portfolio holdings and payout data from 1980 to 2017.

We find the payout firm itself experiences positive abnormal returns after both the announcement and payment dates, as is well-known (Ogden, 1994; Hartzmark and Solomon, 2013). Next, we test for spillovers on industry peers to detect whether dividends generate news spillover effects. We find no spillover effects on industry peers, neither at the announcement nor the payment date. This suggests that dividends do not reveal substantial fundamental information about other firms.

Other portfolio stocks do not experience abnormal returns around announcement dates, but they do exhibit persistent, positive abnormal returns following the payment date, with no pre-payment date trend. Specifically, the estimated price impact of payout-induced trading identifies a demand elasticity of 1.25 using dividends, consistent with the evidence from Russell 2000 additions and deletions in Chang, Hong, and Liskovich (2015). Elasticities are larger for institutions more likely to be benchmarked and thus reinvest into existing holdings, such as mutual funds and investment advisors.

Like dividends, M&A payouts drive up portfolio stocks but not peer stocks after the payment date, consistent with demand pressure effects. Unlike dividends, M&A announcements are informative about other stocks' fundamentals; specifically, M&A announcements lift peer and connected stocks. Hence, an instrument for returns leveraging M&A payouts would not be adequate. Similarly to share repurchases, since high-frequency data are unavailable as it is the case with dividends.

Having established dividend payouts as a plausible instrument candidate, we move on to estimate the causal effect of an increase in stock prices on corporate outcomes — market feedback effects (surveyed in Bond, Edmans, and Goldstein, 2012; Baker and Wurgler, 2013). Specifically, we ask how firms change and finance investment as secondary market prices fluctuate. This is an important question in the context of the variation exploited by our mechanism, particularly given the policy debate on whether to restrict corporate payouts (see e.g. Boissel and Matray, 2019). The typical reasoning is that firms should invest instead of returning capital to shareholders. While Boissel and Matray (2019) do indeed find that firms increase investment after an exogenous decrease in payouts, we estimate a new spillover effect of payouts: capital investment still occurs despite payouts — it just happens at other firms.

Using dividend-induced trading as an instrumental variable for stock returns, we find firms respond to an exogenous stock price increase by issuing debt, that is, moving back towards their target leverage ratio, and using the funds to increase investment. We estimate that firms undo about a quarter of a stock price increase's impact on their debt to equity ratio by issuing debt over the following year. This subsidy-like effect is non-trivial: \$10 of payouts translates into \$2 of investment at other firms whose stocks are held by the same financial institution.

As an additional line of defense against news as a confounding channel, we repeat the exercise using only the expected component of dividends because, by definition, only surprise dividends, not expected dividends, convey news. Constructing expected dividends is simple; exploiting that managers smooth split-adjusted dividends per share achieves an R^2 of 93%. Instrumenting returns with *expected* dividend-induced trading gives the same results as before, further supporting the notion that the financing and investment responses constitute market feedback effects.

Related literature. This paper uses payout-induced trading as an alternative to two prominent asset demand shocks in the literature: index additions/deletions (e.g. Chang, Hong, and Liskovich, 2015) and mutual fund flow-induced fire sales (e.g. Edmans, Goldstein, and Jiang, 2012). Index additions/deletions cleanly identify the pricing effects of index membership, but in addition to triggering trades by indexers, index membership may also affect liquidity and corporate governance. Further, index additions/deletions cannot be used to study heterogeneity in demand elasticities across firm size, because treated stocks are of similar size, by virtue of being close to index inclusion thresholds. Finally, while index additions/deletions generate price effects that are statistically significant, they are weak instruments for returns (see table 4 of Chang, Hong, and Liskovich, 2015), making it difficult to leverage them to identify market feedback effects. Next, mutual fund flow-induced fire sales are intuitively appealing shocks, but their pricing effects could potentially be driven by reverse causality, as we suggest in Schmickler (2020). Recent work also casts doubt on the mechanical construction of the fund flow-based shock (Wardlaw, 2020).

Overall, in contrast to these two asset demand shocks, payout-induced trading has several desirable features: the instrument is constructed using quantities determined at minimum several days before returns, does not change liquidity, passes weak instrumental variable tests, and can be used to investigate heterogeneous and high-frequency (e.g. daily) price pressure and market feedback effects.

This paper is closely related to Kvamvold and Lindset (2018) and Chen (2020) who also examine the payout-induced trading mechanism. The main difference is that our paper examines returns of connected stocks around announcements and payments to separate news from demand pressure effects. In particular, we find that dividends generate price pressure but no news spillover effects.

Next, this paper contributes to the market feedback effect literature. Since Edmans, Goldstein, and Jiang (2012), the literature instruments returns with mutual fund outflow-induced fire sales and finds that undervaluation reduces investment (e.g. Edmans, Goldstein, and Jiang, 2012; Derrien, Kecskes, and Thesmar, 2013; Phillips and Zhdanov, 2013; Bonaime, Gulen, and Ion, 2018; Eckbo, Makaew, and Thorburn, 2018; Lou and Wang, 2018; Dessaint et al., 2018).[1] While this literature focuses on how asset price decreases affect investment, the effect need not be symmetric. In fact, Binsbergen and Opp (2019) argue that overpricing leads to larger real inefficiencies than underpricing because capital adjustment costs are asymmetric; divesting is costly and firms rarely do. Hence, it is important to understand how stock price *increases* affect investment. While mutual fund outflow-induced trading is an experiment for asset price decreases, payout-induced trading is an instrument for asset price increases.

Further, this paper contributes to the large literature that examines pricing effects of payout events. The bulk of this literature focuses on how payout events affect the firm itself. For dividends, firms experience positive abnormal returns after announcements, before ex-dates, and after payments (e.g. Ogden, 1994; Hartzmark and Solomon, 2013). For share repurchases, firms experience positive abnormal returns after share repurchase program announcements (e.g. Vermaelen, 1981; Grullon and Michaely, 2004; Bargeron, Kulchania, and Thomas, 2011). For M&A, targets experience large, positive returns after announcements; acquirors experience muted returns (e.g. Asquith, Bruner, and Mullins, 1983; Jensen and Ruback, 1983; Mitchell, Pulvino, and Stafford, 2004). This paper contributes to this literature by systematically examining the returns of peer and connected stocks around announcement and payment dates of all types of payouts.

[1]Earlier studies that examine how stock prices affect investment but do not instrument returns include Blanchard, Rhee, and Summers (1993), Baker, Stein, and Wurgler (2003), Gilchrist, Himmelberg, and Huberman (2005), Chen, Goldstein, and Jiang (2007), Polk and Sapienza (2009), and Bakke and Whited (2010).

2 Data

Constructing payout-induced trading, which is the central object of this paper, requires two types of data: portfolio holdings of financial institutions and firm payout data. Our empirical tests also require additional institution and firm characteristics. We detail all of these in this section.

2.1 Institutional Stock Holdings

Portfolio holdings come from two sources. First, stock holdings for all institutions except mutual funds are from the Thomson Reuters (TR) Institutional Holdings Database. This includes banks, insurance companies, investment advisors (e.g. asset managers), pension funds, and other investors (e.g. endowments). We apply the institution type correction from Koijen and Yogo (2019). Thomson Reuters' sources are SEC 13F filings. All financial institutions managing above $100 million must report long positions. 13F holdings are at the institution level.

Second, more micro-level data are available for mutual funds, as the Thomson Reuters Mutual Fund Holdings database provides fund-level portfolios. For example, instead of Vanguard's holdings, the database provides the holdings of the Vanguard Dividend Growth Fund. The sources for this database are SEC-mandated disclosures in Forms N-30D, N-Q, and N-CSR as well as voluntary disclosures. Holdings data dictate our sample, which spans 1980 to 2017.

In addition, for mutual funds, detailed institution-level data are available. We take mutual fund AuM, returns, and distributions from the CRSP Mutual Fund Database and households' reinvestments of mutual fund distributions and portfolio equity shares from Morningstar.[2] For other institution types, we infer AuM and returns from portfolio holdings. We measure AuM as the total market value of all observed portfolio positions, and portfolio returns as the portfolio weighted-mean of stock returns, assuming that institutions only trade at the end of each quarter.

We use AuM and portfolio returns to compute investment flows as a control variable in some of our baseline specifications. Here, we follow the standard definition of investment flows. In time period t, institution i receives investment flows of $Flow_{i,t} = (A_{i,t} - A_{i,t-1}(1 + r_{i,t}))/A_{i,t-1}$, where $A_{i,t}$ are AuM and $r_{i,t}$ are institution returns (Coval and Stafford, 2007).

[2] We preferentially take mutual fund data from the CRSP Mutual Fund Database, unless the data quality of Morningstar data is higher, as is the case for portfolio equity shares.

2.2 Corporate Payouts

Ordinary cash dividends (distribution code *"distcd"* 1000-1399) and stock dividends (*distcd* 5530-5539) are from CRSP. The typical source for M&A data, SDC Platinum, does not provide payment dates; hence, we take M&A payment dates, and also announcement dates for consistency, from CRSP (*distcd* 3000-3399 for cash and *distcd* 3700-3799 for stock deals). CRSP payment dates assume M&A payment after delisting.

Share repurchase program announcement dates are from SDC Platinum; payment date data are unavailable. Quarterly share repurchases are from Compustat North America Fundamentals Quarterly. Firms only have to report the actual number of shares repurchased since 2004. Accordingly, researchers must infer share repurchases for the pre-2004 period. We infer share repurchases following Banyi, Dyl, and Kahle (2008) who show that measures based on the Compustat item *purchases of common stock* provide the most accurate estimate of actual shares repurchased.

Together, holdings and payout data allow us to construct dollar payout flows to each institution as:

$$PayoutFlow_{i,t} = \sum_{n=1}^{N} Payout_t(n) \times Shares_{i,t-1}(n). \tag{1}$$

where institution i holds $Shares_{i,t-1}(n)$ shares of stock n, which pays out $Payout_t(n)$ per share, and dollar payout flows are the sum of the payouts from all N stocks.

We construct payout flows separately for each type of payout, resulting in cash dividend, stock dividend, cash M&A, stock M&A, and share repurchase flows. For payouts in stock — which we use in placebo tests — we imput the market value of the securities paid. For share repurchase flows, we do not observe which institutions sell to repurchase programs. Hence, we assume all institutions sell a fraction equal to the fraction of shares repurchased by the firm. Finally, we construct industry payout as the market capitalization-weighted mean payout to price ratio of firms with the same 3-digit SIC code.[3]

2.3 Firm Characteristics and Main Variables

Stock data are from CRSP, accounting data are from Compustat North America Fundamentals Annual and Quarterly. The main dependent variable we use in the paper is log returns.

[3]We choose SIC codes over NAICS codes, which CRSP assigns starting in 2004, and over Hoberg and Phillips (2016) industry classifications, which start in 1996, because they cover the full 1980-2017 sample. We choose 3-digit SIC codes because this corresponds to the industry level. It is also the level of granularity targeted by Hoberg and Phillips (2016) industry classifications.

The main control variables are the characteristics corresponding to a standard six-factor asset pricing model (Fama and French, 2018), i.e. beta, log market equity, log Tobin's Q, profitability, investment, and momentum, because they are the most prominent drivers of expected returns. We also control for dividend to book equity as in Koijen and Yogo (2019), because of the outsize importance of dividends in this paper, though controlling for payouts is often redundant, because we exclude payout stocks in the main specifications to isolate spillover effects. The firm characteristics are constructed following Koijen and Yogo (2019). Accounting data are released with a delay, so we lag accounting data by 6 months. To construct market beta, we take the 1-month T-bill rate and the market return from Kenneth French's website. We calculate market betas using 60-month rolling window regressions.

When testing for market feedback effects on investment, we follow Dessaint et al. (2018). This means we measure the investment rate as the ratio of capital expenditures and property plant and equipment and exclude financial firms (SIC codes 6000-6999) and utilities (SIC codes 4000-4999). We also test whether the investment response is financed by debt or equity. We measure debt as total liabilities and equity as common equity. In all exercises, we winsorize characteristics cross-sectionally at the 1 and 99% level, as for example in Green, Hand, and Zhang (2017) and Dessaint et al. (2018). We restrict the sample to US ordinary common stocks that trade on the NYSE, AMEX, and Nasdaq; have non-missing market values and returns; and for which the holdings data cover at least 1% of shares outstanding.

2.4 Summary Statistics

2.4.1 Corporate Cash Payouts

Figure 2 shows summary statistics. The first plot compares total cash payouts to two prominent, alternative sources of asset demand shocks: total absolute extreme mutual fund outflows and the total market value of firms that are added/deleted from the Russell 1000/2000 index. All three variables are scaled by total market equity. Index additions/deletions are from Chang, Hong, and Liskovich (2015). Total cash payouts, the sum of cash dividends, share repurchases, and cash M&A payouts, fluctuate around almost 6% of total market equity per year. This is the key aggregate number describing the magnitude of the shock. Firms pay out 6% of market equity in cash, so the average investor receives a 6% annual payout flow, and that translates into a 6% demand shock if all investors reinvest. At the end of the sample, total market equity is about $30 trillion, and total cash payouts are almost $2 trillion.

In comparison, total index/additions deletions are about 2%, and lower since Russell Inc. smoothed index transitions in 2007. Total extreme mutual fund outflows are around 1%, and are lower at the beginning of the sample, before the rise of mutual funds. Payouts are significantly larger than fund flows and index additions/deletions. That said, these shocks do not translate into demand shocks in the same way. How payouts translate into demand shocks depends on the fraction of investors that reinvests; for index additions/deletions it depends on the fraction of investors tracking the Russell 1000/2000; and for fund flows it depends on how mutual funds accommodate flows.

The second plot breaks cash payouts into their three components. From 1980 to 2017, dividends dropped from 4% to 2% of total market equity — which still puts it above alternative schocks in absolute terms. Companies substituted dividends for share repurchases, which increased from 0% to 2%. Lastly, cash M&A transactions fluctuate between 0 and 3%. Overall, cash payouts are economically large, suggesting they have the potential to create large demand shocks and consequently large price impacts and real effects.

2.4.2 Financial Institutions

Table 1 summarizes financial institution information by institution type and decade. For mutual funds and investment advisors (e.g. asset managers), the number of institutions and market share increased steadily over the sample but remained largely stable for banks, pension funds, insurance companies, and unclassified institutions (e.g. endowments).

Mutual funds and investment advisors are the most important institution types in terms of equity market share. In the most recent decade, mutual funds and investment advisors held combined almost half of the market — 25% and 21%, respectively. About one third of equities were held by households and foreing investors. Banks held another 12%, and 5% were held between pension funds and insurance companies. Unclassified institutions held the remaining 2%. Mutual funds, investment advisors, and unclassified institutions tend to be small in terms of AuM; banks, pension funds, and insurance companies tend to be larger. However, mutual funds and investment advisors are by far the most frequent institution entity in the data. For mutual funds, this is due to the availability of fund-level instead of institution-level data.

Most institutions hold concentrated portfolios. Mutual funds, investment advisors, and unclassified institutions are the least diversified, with a median of only 59, 53, and 27 stocks held during the last decade, respectively. Even at the 90th percentile, they only hold 368, 270, and 441 out of approximately 4000 stocks. The median insurance company, bank, and pension fund are not diversified either, with 185, 187, and 512 stocks. However, they are diversified at the 90th percentile, with 2068, 1232, and 1581 stocks.

The fact that most institutions hold concentrated portfolios is important for our analysis. If all investors held the market portfolio, every investor would reinvest payouts in proportion to market weights and there would be no cross-sectional variation in the payout-induced trading demand shock. Finally, many institutions receive large payout flows. In the current decade, the median institution receives payout flows between 5 and 6%, with pension funds showing the largest tilt towards payout firms, likely because of their tax-exempt status. At the 90th percentile, payout flows range from 6.8% for banks to 9.2% for unclassified institutions.

3 Price Pressure Effects

This section traces out our shock to asset prices from a host of empirical results we assemble step-by-step. We first show that financial institutions reinvest payouts into their existing portfolios — not only into the paying stocks, but also into the other stocks in the portfolio. We then discuss and show evidence supporting the sources of variation needed to use dividends, in particular, as a plausible exogenous shock to demand pressure without confounding news effects. We conclude the section by estimating asset demand elasticities for different payout sources.

3.1 Trading Response to Payouts

We want to first show that institutions reinvest payouts into existing portfolio stocks — particularly those not making the payment. This is because hypothetical reinvestment in proportion to existing portfolio weights is likely exogenous to firm fundamentals (at the trading time), while unrestricted trades may well be driven by firm fundamentals — for example, by initiating a position using extra cash on hand based on new signals.

Following the previous notation, institution i holds split-adjusted $Shares_{i,t}(n)$ of stock n at time t. We denote relative payout flows as $PayoutFlow_{i,t} = PayoutFlow_{i,t}/A_{i,t-1}$. Our goal here is to test whether payout flows trigger a trading response. At the portfolio position level, if institutions reinvest payouts in proportion to portfolio weights, payout flows prompt a relative change in split-adjusted shares held of

$$\frac{\Delta Shares_{i,t}(n)}{Shares_{i,t-1}(n)} = \alpha_i + \alpha_t(n) + \beta PayoutFlow_{i,t} + \gamma' X_{i,t}(n) + \epsilon_{i,t}(n), \tag{2}$$

This is a regression of portfolio position-level trading on payout flows, a specification analogous to Lou (2012) who investigates the relationship between trading and mutual fund flows instead of payout flows. The left side is only meaningful for existing holdings. Hence,

β captures reinvestment into the *existing* portfolio. If institutions reinvest all payout flows into their existing portfolio perfectly in proportion to pre-existing portfolio weights, $\beta = 1$. If they do not reinvest at all (or invest in completely new stocks without substitutions from current holdings), $\beta = 0$.

Model 2 also includes institution and stock×time fixed effects. Institution fixed effects address the concern that institutions holding payout stocks may differ from institutions that do not. For example, Harris, Hartzmark, and Solomon (2015) show that mutual funds that make large dividend distributions attract inflows, which they may invest into their existing portfolio. The same example also motivates that our inclusion of fund flows in the vector of control variables, X. The stock×time fixed effects accommodates when a stock is the source of payouts. By definition, shareholders of payout stocks receive payout flows. So if institutions systematically bought payout stocks (in antecipation), this could drive a positive β estimate; however, this problem is unlikely to be severe, as shareholders cannot reinvest into the payout stock on average because the supply of shares remains constant — there is residual cash on hand. In addition to including this set of fixed effects, we further address this potential concern by additionally estimating equation 2 on the subsample that excludes payout stocks when they make payments.

Yet, estimates of β only provide a lower bound for the propensity to engage in payout-induced trading. This is because portfolio holdings are only available at the quarterly frequency and therefore miss within-quarter trading. Institutions may well initiate a new position on the first day of a quarter and engage in payout-induced trading during that quarter. This would not be captured by the regression, because $Shares_{i,t-1}(n)$ would be zero and the relative change in shares held would be missing. Similarly, institutions may well engage in payout-induced trading during the quarter but sell a position one day before the end of the quarter. From a quarterly perspective, this erroneously looks like evidence against payout-induced trading behavior. Luckily, the stock level analysis in section 3 does not suffer from this attenuation bias because it examines returns at the daily frequency.[4]

Portfolio holdings allow us to examine the reinvestment behavior of institution types beyond mutual funds. We nevertheless present the evidence starting with mutual funds because data availability and their organizational structure make the results most reliable. Mutual funds are the only institution type for which fund-level instead of institution-level portfolio holdings data are available. Mutual funds are also the only institution type for

[4]In addition to the fixed effects included in model 2, the high-frequency nature of the shocks we construct also helps to sidestep potential issues with how portfolio weights were determined to begin with. The literature on the determinants of institutional holdings is sparse (e.g. Lettau, Ludvigson, and Manoel (2021)), and it seems plausible to assume that weights are correlated with stock fundamentals at the time of the portfolio construction.

which assets under management, portfolio returns, and investment flows are observable. For other institution types, we need to infer these variables from reported portfolio holdings. And while mutual funds are generally not levered and largely hold equities, for other institutions, changes in leverage or reallocations between asset classes often drive large trades.

3.1.1 Mutual Funds

Table 2 reports regression estimates of different specifications of equation 2 for mutual funds. We report results using the full sample (columns 1 and 2) and from a sample that excludes payout stocks (columns 3 and 4). We also report results including and excluding control variables. Using the full sample, the estimate of β is highly statistically significant, with an effect of 0.8 on dividend flows. In response to a 1% dividend flow, mutual funds increase their average portfolio position by 0.8%.

Excluding dividend paying stocks in columns 3 and 4, the coefficient decreases to 0.65 but remains highly statistically significant. Note the interpretation here — for a 1% payout of portfolio stocks within a quarter, the average mutual fund increases the position in its other portfolio stocks in that quarter by 0.65%. This crucially shows that mutual funds reinvest the majority of dividend payouts into their existing portfolio.

For mutual funds, payout reinvestment is explained by the fact that, on average, households reinvest 83% of mutual fund distributions. This means, in practice, that mutual funds make most distributions in additional fund shares instead of cash. Knowing the fraction of retail investors that subscribe to reinvestment plans, mutual funds can permanently reinvest the majority of firm payouts.[5]

Panels (a) and (b) of Appendix Figure A2 plot the time series of aggregate mutual fund dividend and capital gains distributions. The plot does not differentiate between distributions in cash or shares. We highlight the last quarter of each year in blue — when mutual funds distribute significantly more dividends and almost all capital gains in each exercise. This strategic timing is influenced by two legal provisions. First, since 1954, Internal Revenue Code Section 855 effectively forces a fund to distribute payouts within one year after the end of the fund's tax year. This implies a maximum of two years from the time the fund receives a payout from a portfolio stock to the time it must distribute. Second, since 1987, Internal Revenue Code Section 4982 altered this window and required funds to distribute payouts within the same calendar year. While funds held on to corporate payouts for longer

[5]Appendix Figure A1 shows a histogram of the mutual fund distribution reinvestment rate. Over half of all funds face reinvestment rates above 90% and the vast majority of funds faces reinvestment rates above 50%. In addition, mutual funds need not distribute income immediately and have therefore a reinvestment window until distribution deadlines.

before, starting in the late 1980s, the time series shows significant bunching in each year's Q4 for capital gains distributions.

Importantly, Panel (c) of the figure shows that the timing of firm dividend payouts do not follow a similar pattern. Firm dividends are paid remarkably evenly distributed across all quarters and Appendix Figure A3 shows that the same is true for share repurchases and M&A payouts. This difference in timing between corporate payouts and mutual fund distributions provides a clean setting for a first check on whether mutual funds reinvest corporate payouts. If they do not reinvest, their equity portfolio share would mechanically decrease by about 1.5% per quarter — the average quarterly cash payout — over the calendar year as equity is turned into cash because of corporate payouts. The equity portfolio share should then spike during Q4 when funds distribute the cash. This is not the case. Panel (d) of Figure A2 plots the equity share time series starting in 1990, when the variable becomes readily available. The equity share is close to constant over the calendar year, suggesting that mutual funds reinvest corporate payouts into equities.

3.1.2 Other Institution Types

Table 3 reports results for other institution types using the strictest specification, i.e. excluding dividend stocks and including control variables. From columns 1 to 6, we report results for mutual funds (repeated for comparison), investment advisors, pension funds, banks, insurance companies, and unclassified institutions. We find all institution types except banks reinvest dividends into their existing portfolios (excluding the payout stock in that quarter). For banks, the coefficient is positive, but close to zero and statistically insignificant. Apart from banks and mutual funds, investment advisors (e.g. asset managers) are the most important institution type. As documented in Table 1, they have an equity market share of 21% at the end of the sample, as compared to 3, 2, and 2% for pension funds, insurance companies, and unclassified institutions, respectively. The estimated coefficient is smaller for investment advisors than for mutual funds, 0.4 versus 0.6, but still highly statistically significant.

For the small institution types, we estimate coefficients of 1.8, 0.4, and 0.6. It is unexpected that the estimate is greater than one for pension funds. It suggests that our construction of payout flows underestimates the payouts pension funds receive, likely because pension funds are tax-exempt, potentially incentivizing them to take on more payouts. Overall, all institution types except banks engage in payout-induced trading. Again, note that these regressions capture portfolio-wide responses excluding reinvestments into the stocks that pay dividend in a period.[6]

[6]In Appendix Tables A1 and A2, we report analogous analyses for M&A instead of dividend payouts.

3.2 Measuring the Demand Shock

Having established that the average institution reinvests stock payouts into its existing portfolio, in line with Chen (2020), we now detail the construction of our asset demand shock. The approach leverages the idea that reinvestment in proportion to existing portfolio weights is likely exogenous to firm fundamentals, while actual trades may well be driven by firm fundamentals. This insight can be used to construct an asset demand shock by aggregating the hypothetical number of shares institutions buy in response to payout flows. Scaling by total shares held by all institutions gives the relative demand shock:

$$PIT_t(n) = \frac{\sum_{i=1}^{I} PayoutFlow_{i,t} \times Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}. \tag{3}$$

We construct $PIT_t(n)$ separately for different payout types. This produces (i) cash dividend-induced trading, (ii) stock dividend-induced trading, (iii) cash M&A-induced trading, (iv) stock M&A-induced trading, and (v) share repurchase-induced trading. The definition of payout-induced trading is closely related to mutual fund flow-induced trading (e.g. Lou, 2012; Edmans, Goldstein, and Jiang, 2012). Replacing payout flows with mutual fund flows yields their instrument, though the exact construction here also takes the Wardlaw (2020) critique into account.

Two sources of variation drive the payout-induced trading shock. First, the timing of payouts, which we support below. Second, investor heterogeneity. While the first source backs the validity of our approach, the second is important for empirical measurement. If all investors held the market portfolio, every investor would reinvest payouts in proportion to market weights, so dollar payout-induced trading would be exactly proportional to market weights in the cross-section. In this world, every stock would get the same cross-sectional shock, making it impossible to identify its price impact.

Figure 3 shows that in practice this concern is not warranted. It plots histograms of the ratio between a stock's share of dollar payout-induced trading on a given day and its market weight in panels (a) and (b). This construction eliminates time series variation. With homogeneous investors, all values would be equal to one. The left histogram first shows the cash dividend shock. The distribution is approximately exponential, with most values falling between 0 and 2 and a small mass at 5 (where we winsorize it for illustration

The results are qualitatively the same. For share repurchases, we do not observe which institutions sell to share repurchase programs; further, inferring sales to share repurchase programs from actual sales assigns high repurchase flows to institutions that engage in broad selloffs for unobserved reasons, biasing down and preventing reliable estimates.

purposes). This means that on a given day, many stocks are close to untreated and some stocks receive multiple times the shock they would receive in a homogeneous-investor world. The right histogram corresponds to the cash M&A instead of the dividend shock. The distribution is similar, but the spread is even wider. This is because on an average day, 30 firms pay a dividend, but zero or one firm is acquired. Overall, Figure 3 shows that investor heterogeneity generates large cross-sectional dispersion in payout-induced trading.

3.2.1 Identification Strategy

The key identification challenge to use equation 3 is that payouts reveal fundamental information which also changes asset prices. Therefore, if similar firms are held in the same portfolios, abnormal returns of connected stocks may be the result of news, not price pressure. The identification strategy thus needs to separate price pressure from news effects. Of course, news and price pressure are not mutually exclusive and can both affect asset prices.

However, demand pressure effects differ from news effects in terms of timing: news changes asset prices at announcement, while demand pressure changes asset prices at payment, when financial institutions receive cash and reinvest. Ordinary cash dividends in particular allow for the cleanest identification. Dividend announcements reveal fundamental information but once announced, dividends are deterministic. Dividends are then paid after a delay of one to three months. The payment reveals no fundamental information. Hence, the gap between the announcement and payment date separates the news from the demand pressure effect.

Panels (c) and (d) of Figure 3 illustrate this setting. The left plot shows the distribution of the number of days between the dividend payment and announcement date. Dividends are typically paid about one to three months after the announcement, with a mean delay of 45 days. The 1st percentile is 17 days, likely sufficient time for the market to incorporate any announcement date news into prices.

The right plot shows total cash dividends for each day in 2017. The plot highlights that on almost every business day of the year, at least one stock traded in the US pays a dividend. Each stock is held by many institutions and each institution holds many stocks. Consequently, institutions disperse payouts over many stocks. Hence, payout-induced trading affects almost every stock on almost every day, but with varying intensity. On most days, companies pay out in aggregate about $1 billion. On a few dozen days, companies pay out about $5 billion. The biggest daily shocks are about $10 billion, which means that differences in shock sizes are no greater than one order of magnitude.

As almost every firm is treated on almost every day and with varying intensity, we investigate the price impact of payout-induced trading using a distributed lag model. In particular, we estimate the daily frequency, cross-sectional regression

$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n), \tag{4}$$

of stock returns on a vector of shocks Z, which contains the payout to price ratio, the industry payout ratio, and payout-induced trading. We use the payout to price ratio to measure the payout's impact on the firm itself, the industry payout ratio to measure spillovers on peers, and payout-induced trading to measure spillover effects on connected stocks. We include $\bar{L}$ lags and $\underline{L}$ leads. The coefficients on the lags measure the price impact of the shock; $\gamma_l > 0$ means stocks experience positive abnormal returns l days after the shock. We include the leads to test for pre-event trends. This empirical strategy is the analogue to an event study for continuous treatment. For the case when Z is one-dimensional and binary, the results are numerically identical to event study estimates (Schmidheiny and Siegloch, 2020). Lastly, X is a vector of control variables as described in the data section. We cluster standard errors by time because returns are highly correlated in the cross-section.

PIT depends on the payouts of the firm itself. This is consistent with the theoretical reasoning behind the payout-induced trading demand shock. When a firm pays out cash, investors want to reinvest some of this cash into that firm. However, it is known that the firm experiences positive abnormal returns after payouts (Ogden, 1994). The innovation of this paper is to show payouts' spillover effects on other stocks. While we control for firm payouts, this may not fully eliminate the concern. Further, firms held in the same portfolios are similar, so their payout schedules may be correlated and it is known that firms experience abnormal returns around dividend announcement and ex-dividend dates (Hartzmark and Solomon, 2013). We address both concerns by estimating spillover effects on the subsample that excludes payout firms, i.e. firms that pay out during the year centered around the date of the observation. Hence, the estimates are free of the known self-effects and isolate spillover effects.

3.3 The Return Pattern

We test whether payouts generate news and/or price pressure effects in a systematic analysis of returns around payout events. For each type of payout, we report daily abnormal returns of the firm itself, peer stocks, and connected stocks around announcement and around payment dates.

3.3.1 Corporate Dividends

Figure 4 investigates return patterns around cash dividend events at the daily frequency. On the left, we show returns around the announcement date, which capture the effects of news. On the right, we show returns around the payment date, which capture the effects of price pressure. The first row reports returns of the payout firm itself, the second row presents spillover effects on industry peers, and the last row displays spillover effects on stocks held in the same portfolio. The plots report estimates of equation 4. We plot cumulative coefficients and 95% confidence intervals from a Wald test as described in Schmidheiny and Siegloch (2020). Note that the coefficient estimates are comparable across columns, but not across rows, because the different rows report coefficients on different variables.

Plots (a) and (b) find the payout firm experiences abnormal returns after both the announcement and the payment. The cumulative coefficients reach 0.2 and 0.07, meaning a 1% dividend triggers a 20 basis point return after the announcement and a 7 basis point return after the payment. Both graphs exhibit modest, positive, pre-event trends, indicating that investors anticipate dividend events. These self-effects in panels (a) and (b) are well known (Ogden, 1994; Hartzmark and Solomon, 2013).

Plots (c) and (d) find no evidence for spillovers on industry peers, though there may be modest, positive, temporary spillover effects after the *announcement* date. If dividends conveyed news about other firms' fundamentals, we would expect to see spillover effects on industry peers. Therefore, the absence of spillover effects on industry peers, even after the announcement, suggests that dividends do not reveal substantial information about fundamentals of other firms. This is unsurprising, because in the vast majority of cases, firms simply keep dividends constant on a split-adjusted dividend per share basis. In fact, we exploit this behavior in section 4.2.1 where we construct expected dividends and find that simply predicting that past dividend behavior continues yields an R^2 of 93%.

Lastly, panels (e) and (f) examine spillover effects on other stocks held in the same portfolios of financial institutions. To test for spillover effects on connected firms after the announcement, we construct hypothetical dividend-induced trading if dividends were paid on the announcement date. We find no evidence for spillover effects after the dividend announcement. While there is a modest increase from day three to day ten, the effect is almost precisely 0 over the three days following the shock, as well as over the full $\pm$ 10 day window. This indicates that dividends do not reveal fundamental information about other portfolio stocks. This is expected, as dividends do not even generate spillover effects on industry peers.

Yet, panel (f) documents that stocks held in the same portfolio rise after the payment. This return response is the price impact of payout-induced trading. Connected stocks experience positive, abnormal returns the day of and for two days after the payment. The coefficients on the following lags, as well as the estimated pre-event trends, are close to zero and statistically insignificant. The reaction is strongest one day after the payment. We estimate that a 1% demand shock translates into a 0.8% return. This implies an asset demand elasticity of 1.25, consistent with the evidence from Russell 2000 additions and deletions in Chang, Hong, and Liskovich (2015). Overall, we find dividends generate no news spillover effects but do produce large price pressure spillover effects on connected stocks.

Further, we find no reversal after the demand shock. Chen (2020) provides related results at low-frequency, documenting that the price impact of dividend- and share repurchase-induced trading is persistent. However, he does find reversal after M&A-induced trading price pressure effects. In the literature, whether price pressure effects are followed by reversal depends on the setting. Ogden (1994) finds no reversal of the dividend payment date effect. Neither do newer studies exploiting index/additions deletions, including Kaul, Mehrotra, and Morck (2000) and Chang, Hong, and Liskovich (2015). However, early studies of S&P 500 index additions do find price reversal (Shleifer, 1986; Harris and Gurel, 1986). Similarly, Greenwood (2005) finds partial reversal. Studies of mutual fund flow-induced fire sales also document reversal (e.g. Edmans, Goldstein, and Jiang, 2012). However, Wardlaw (2020) finds that factor loadings explain the reversal following flow-induced fire sales.

3.3.2 Cash M&A Payouts

So far, we have estimated the price impact of PIT using ordinary cash dividends. While dividends provide a clean setting to separate news from price pressure, there are other payout types. However, as there are no high-frequency payment data for share repurchases and for M&A payouts, announcement and payment are less cleanly separated than for dividend payouts, because in rare cases, M&A deals fail even after a merger agreement is signed. While this setting does not meet the same burden for identification of our previous analysis, we still report estimated effects as supportive evidence of the overall mechanism.

Figure 5 repeats the analysis in Figure 4 for cash M&A instead of cash dividend payouts. Note that the top right panel, "Payment, payout stock", is missing because the M&A target ceases to exist. All other panels are analogous. Panel (a) shows the well-known empirical fact that M&A target stocks experience positive abnormal returns after deal announcements, with a statistically significant pre-event trend, suggestive of information leakage (e.g. Jensen and Ruback, 1983). The second row looks at spillover effects on peer firms. In contrast to dividend payouts, M&A payouts reveal fundamental information about other stocks. Peer stocks rise after announcements but are flat around payments. The latter suggests that failures of closed M&A deals are so rare that payments do not reveal fundamental information about other firms. Accordingly, any payment date spillover effects on connected stocks can be interpreted as price pressure effects. In the last row, we show spillover effects on connected stocks, which exhibit gains after M&A announcements and payments, consistent with price pressure effects, particularly as there are no payment date spillover effects on peer stocks.

M&A cash payments generate qualitatively the same payment date pattern as cash dividend payments. Portfolio stocks experience positive abnormal returns the day of and the two days after payments. Again, the cumulative coefficient flattens after that. In addition, while largely statistically insignificant, there is a positive pre-event trend. This is expected, because many investors sell their positions in M&A targets and reinvest early. The coefficients sum to 0.25. This is smaller than in the dividend-induced trading exercise, likely because M&A deals trigger trading frenzies. Consequently, the quarterly holdings snapshots mismeasure payout recipients, biasing down the estimates.

3.3.3 Share Repurchases

For completeness, Figure 6 repeats the analysis in Figure 4 for share repurchases. Note that the right column, "Payment, ...", is missing because there are no high-frequency payment data. Nevertheless, examining share repurchase announcement spillover effects allows us to investigate the speed of spillover effects. Fast spillover effects are likely driven by news; slow spillover effects are likely the result of price pressure. This is because SEC Rule 10b-18 requires the repurchasing company to delay actual repurchases to give the market sufficient time to absorb the new information. The rule also imposes volume limits, meaning most repurchase programs are not completed immediately. Hence, examining the speed of spillover effects helps gauge share repurchase-induced trading's suitability as an instrument for stock returns.

Panel (a) shows the well-known empirical fact that stocks experience positive abnormal returns after share repurchase program announcements (e.g. Vermaelen, 1981). Panel (b) demonstrates that peer firms largely do not experience abnormal returns, suggesting that repurchase announcements reveal predominantly idiosyncratic information. Lastly, panel (c) does not find statistically significant spillover effects on connected stocks, though there is a small positive response with a two-day delay. It is not clear whether this response is driven by news or price pressure. Either markets incorporate the announcement information about connected stocks with a delay of two days, or institutions start reinvesting share repurchase proceeds two days after repurchase announcements. While both explanations are certainly possible, connected stocks' gains appear to coincide with the beginning of the actual repurchases, not the announcements. The cumulative coefficient reaches 0.1, smaller than after dividend or M&A events. This is unsurprising, because most share repurchase programs are likely not completed 10 business days after the announcement.

3.4 Institutional Heterogeneity

One reason why institutions engage in payout-induced trading is likely because many are benchmarked. This implies institutional heterogeneity: the more an institution is benchmarked, the more likely it is to reinvest into its existing portfolio. To test this, we construct payout-induced trading using only one institution type at a time. We then estimate equation 4 for each institution type. This ranks institution types by price impact, and benchmarked institution types should lead the list. In particular, many mutual funds are closet indexers (Cremers and Petajisto, 2009) or index funds, so mutual funds are likely the most benchmarked institution type.

Figure 7 reports the same analysis as panel (f) of Figure 4 but by institution type. Broadly, we find price pressure effects for mutual funds and investment advisors (e.g. hedge funds and asset managers) but not for banks, pension funds, insurance companies, or unclassified institutions (e.g. endowments), which are not characteristically benchmarked. Panel (a) shows that dividend-induced trading by mutual funds generates largely the same price response pattern as aggregate payout-induced trading. Connected stocks experience positive abnormal returns after the payment. The cumulative coefficient is even larger, 1 instead of 0.7, consistent with mutual funds being the most benchmarked institution type.

Panel (b) displays the results for investment advisors. Again, the price response is positive, concave and statistically significant. However, the cumulative coefficient is now smaller, 0.3 instead of 0.7. There are three potential explanations for this decrease. First, investment advisors are less benchmarked than mutual funds. Second, investment advisors are more price elastic than mutual funds; they are less likely to continue buying as prices are rising. Finally, investment advisors, hedge funds in particular, may trade more frequently, meaning their quarterly portfolio snapshots mismeasure payout recipients with greater intensity.

Panels (c) and (d) present the analogous analysis for cash M&A payouts. The overall pattern is the same. The price impact of M&A-induced trading is positive and statistically significant for mutual funds and investment advisors. Again, the price impact is larger for mutual funds than for investment advisors. Further, section 3.3.2 argues that the estimated price impact of M&A-induced trading is likely lower than that of dividend-induced trading because many institutions sell their shares of M&A targets to merger arbitrageurs, implying quarterly holdings mismeasure who receives M&A payouts. This suggests further institutional heterogeneity: there should be no price impact for institution types that sell to merger arbitrageurs but there should be a price response for institution types that *are* merger arbitrageurs. As mutual funds and hedge funds (included in investment advisors) are the main institution types that run merger arbitrage strategies, the results are consistent with this reasoning.

3.5 Placebo Tests

We now turn to placebo tests based on stock instead of cash payouts. Our mechanism relies on the idea that when payouts are dispensed in cash, institutional investors end up deploying these transferred funds, primarily into portfolio holdings. Payouts based on stocks provide an interesting test to this demand pressure channel since they may similarly convey fundamental news at announcement, but work different upon payment. Stock dividends do not reallocate cash; they merely act as stock splits, just like stock received following an acquisition. Thus, while announcement estimates from these tests could also imply abnormal returns, we antecipate muted or nill impact on portfolio holdings at payment date.[7]

3.5.1 Dividend Payouts in Stock

We begin by examining pricing effects around stock dividend events. Figure A5 repeats the analysis in Figure 4 for stock dividends. The payout stock itself experiences positive abnormal returns after the announcement but only small, temporary positive abnormal returns after the payment. When firms pay out stock instead of cash, they do not experience payment date price pressure effects. Next, panels (c) to (f) show that stock dividends do not generate spillover effects, neither on peer nor connected stocks, neither at the announcement nor the payment date. As expected, stock payouts generate the no announcement and peer effects, and in contrast to cash dividends, generate no payout-induced trading price pressure effects.

3.5.2 M&A Payouts in Stock

Figure A6 repeats the analysis in Figure 5 for stock instead of M&A payouts. The return pattern is the same as for cash M&A payouts with one exception: Panel (e) shows that connected stocks' payment date price response is weaker. The price response to the shock is positive but statistically insignificant. The cumulative coefficients also rise later and more

[7]Arguably, stock dividends differ from cash dividends for reasons beyond the payment method. As stock dividends occur much less frequently than cash dividends, the placebo test may lack statistical power. Hence, stock dividends are not an ideal placebo test, but still informative. Stock M&A payouts, however, come closer to an ideal placebo test. Stock M&A payouts have a similar magnitude as cash M&A payouts and are very similar economically even if subtle differences remain; for example, payment in stock signals overvaluation of the acquiror (Shleifer and Vishny, 2003), which is likely unimportant here because the shareholders of the acquiree, not the acquiror, receive the payout.

slowly. This is consistent with institutions needing time to liquidate the shares they were paid. Moreover, again, there is a pre-event trend, likely because many institutions liquidate and reinvest early. In fact, the pre-event trend is stronger than for cash M&A payouts. This is expected — institutions may not sell when they will receive cash anyways. Overall, cash payouts generate price pressure effects in connected stocks, but stock payouts generate much weaker effects, which is consistent with institutions reinvesting cash but generally holding the stock they are paid.

3.6 Who Benefits?

The PIT shock a stock receives in aggregate is a function of how often it is held by investors and the propensity of other portfolio stocks to pay out dividends. If a stock is popular across mutual funds, for example, it will be more likely to be exposed to demand pressure — just like if the stock is usually held with dividend-paying corporations. So which types of firms face higher institutional buying pressure due to payouts?

Column (1) of Table shows correlations between characteristics of a firm and the exposure of its stock to payout-induced trading. Column (2) estimates correlations for the same factors but with respect to the dividend yields paid by firms. Relative to stocks that pay out dividends, companies more exposed to PIT have larger market cap, are less profitable, and invest less. Here, we do not attempt to disentangle which of these characteristics investors target — are larger caps more popular and they happen to be less profitable on a relative basis and so on. Rather, we set the stage for the next session where we estimate real effects from payout-induced increases in stock prices. Through our mechanism, investors effectively transfer cash paid by the averge dividend-paying firm to the average "spillover" stock price. Potential distributional consequences of such transfers are left for future research.

4 Market Feedback Effects

This section examines how payout-induced trading, by driving up stock prices, affects corporate financing and investment. We investigate these effects at the annual frequency to accommodate the seasonality of investment as well as that managers may respond slowly because of financial frictions (e.g. Fazzari et al., 1988; Kaplan and Zingales, 1997).

An extensive literature asks whether stock prices have causal effects on real corporate outcomes, predominantly investment (e.g. Edmans, Goldstein, and Jiang, 2012; Derrien, Kecskes, and Thesmar, 2013; Phillips and Zhdanov, 2013; Bonaime, Gulen, and Ion, 2018; Eckbo, Makaew, and Thorburn, 2018; Lou and Wang, 2018; Dessaint et al., 2018). To answer this question, researchers need a natural experiment that generates variation in stock returns that is independent of fundamentals. The state-of-the-art instrument for returns is mutual fund outflow-induced trading. The idea is that mutual fund redemptions are as good as random but force mutual funds to liquidate assets, driving down prices (Edmans, Goldstein, and Jiang, 2012). Hence, outflow-induced trading is an experiment for decreases in asset prices. All of the market feedback effect papers cited above use this instrument.[8]

Even though the literature focuses on how asset prices decreases affect investment, the effect need not be symmetric. In fact, Binsbergen and Opp (2019) argue that overpricing leads to larger real inefficiencies than underpricing because capital adjustment costs are asymmetric; firms rarely divest because it is costly. Hence, it is important to understand how asset price *increases* impact investment.

4.1 Identification Strategy

We start with a standard investment-Q regression, closely following the empirical setup in Dessaint et al. (2018):

$$\frac{I_t(n)}{K_{t-1}(n)} = \alpha_t + \alpha(n) + \beta Q_{t-1}(n) + \gamma' X_{t-1}(n) + \xi_t(n),$$

which implements a regression of the investment rate $\frac{I_t(n)}{K_{t-1}(n)}$ on Tobin's Q, control variables, time, and firm fixed effects. The regression is motivated by a Q-theory of investment model which relates investment to marginal Q (e.g. Almeida, Campello, and Galvao, 2010). Marginal Q is unobservable but equals average Q under the standard assumptions of constant returns to scale and perfect competition (Hayashi, 1982).

As we showed in the previous section, payout-induced trading is a relevant instrument for returns, and its source of variation is plausibly consistent with the exclusion restriction in this setting. Thus, we estimate the equation above in first differences, since returns are approximately first differences of log Tobin's Q:

$$\Delta \frac{I_t(n)}{K_{t-1}(n)} = \eta_t + \beta r_{t-1}(n) + \tilde{\gamma}' \tilde{X}_{t-1}(n) + \epsilon_t(n) \tag{5}$$

[8]Earlier studies that examine how stock prices affect investment but do not instrument returns include Blanchard, Rhee, and Summers (1993), Baker, Stein, and Wurgler (2003), Gilchrist, Himmelberg, and Huberman (2005), Chen, Goldstein, and Jiang (2007), Polk and Sapienza (2009), and Bakke and Whited (2010) The only market feedback effect paper investigating the impact of increases in asset prices that we are aware of is Khan, Kogan, and Serafeim (2012). They instrument returns with mutual fund inflow-induced trading. However, they investigate effects on SEOs, not investment.

The control variables are the risk factor characteristics as described in the data section (beta, log market equity, profitability, investment, and momentum), as well as the payout ratios for all payout types, i.e. cash dividends, stock dividends, cash M&A, stock M&A, and share repurchases. We also estimate alternative versions of this equation. Namely, we report specifications excluding control variables as a simple baseline and estimates replacing time fixed effects with time×industry fixed effects. This alternative functional form mitigates the potential concern that payout-induced trading captures news instead of price pressure spillover effects.

As discussed in section 3, $PIT_t(n)$ depends on the payouts of the firm itself. At high frequency, where data are abundant, a simple way to separate the spillover effect from the self-effect was to exclude payout firms from the estimating interval when the stock pays dividends. At lower frequency, with less abundant data, we use as baseline an alternative approach. Define

$$\widetilde{PayoutFlow}_{i,t}(n) = \sum_{m \neq n} Payout_t(m) \times Shares_{i,t-1}(m)$$

and substitute the payout flow in equation 3 for this measure free of self-effects. That is, for stock n, $\widetilde{PayoutFlow}(n)$ measures pure spillover payouts coming from stocks held in the same portfolio by each institution. We additionally report results that examine a subsample that excludes payout firms in a robustness check. Finally, all results below use only payouts in the form of dividends as an instrument for returns.

4.2 The Investment Response

Table 5 reports estimates of equation 5 at the top. We show the first and second stage of 2SLS estimates as well as the OLS estimate as a benchmark. In all specifications, we cluster standard errors by time because returns are highly correlated in the cross-section.[9] The first column reports the results of the OLS regression. There is a strong positive relationship between a firm's stock return and its investment over the next year. The coefficient is 0.08, meaning a 1% return is associated with a 8 basis point increase in investment. This corresponds to about 0.5% of the median annual investment rate of 17%. However, this coefficient estimate is not the causal effect of stock prices on investment. One prominent reason for this is that measurement error creates attenuation bias (e.g. Erickson and Whited, 2000; Almeida, Campello, and Galvao, 2010; Bakke and Whited, 2010). Specifically, Bakke and

[9]Appendix Table **??** demonstrates that clustering by time is more conservative than clustering by firm and as conservative as double clustering by time and firm.

Whited (2010) argue returns that are ignored by managers can be treated econometrically as measurement error and show that applying errors-in-variables techniques significantly increases the estimated investment-to-Q sensitivity. Another attenuation bias comes from the investment factor, i.e. the empirical fact that high-investment firms have lower expected returns, likely because firms with a lower cost of capital invest more (Titman, Wei, and Xie, 2004; Hou, Xue, and Zhang, 2015).

The next columns report results from instrumental variable regressions. Columns 2 and 3 present the first and second stage of a baseline 2SLS estimate that does not include control variables. The first stage regression shows that payout-induced trading is unlikely to be a weak instrument, with a Kleibergen and Paap (2006) F-statistic of 50. In the second stage, we estimate a highly statistically significant coefficient of 10 basis points. While this point-estimate is higher than the OLS coefficient, consistent with the common finding that instrumenting returns mitigates attenuation bias (e.g. Erickson and Whited, 2000; Almeida, Campello, and Galvao, 2010; Bakke and Whited, 2010), confidence intervals for the OLS and IV coefficients overlap.

In columns 4 and 5, we add control variables. The IV coefficient remains statistically significant and increases from 0.1 to 0.17. In the last two columns, we include time$\times$industry fixed effects. News spillovers should be strongest within an industry. Hence, if the results were driven by news spillovers, the coefficients in the first and second stage should decrease significantly with this specification. This is not the case. While the coefficient in the second stage decreases slightly to 0.15, the investment-return elasticity here is not statistically different from the specification with only time fixed effects. Putting a dollar figure on these effects in terms of dividends implies the following: $10 paid in dividends by the average firm translates into as much as $2 of investment at firms held in the same portfolio.

4.2.1 Expected Payout-Induced Trading

As an additional line of defense against news as a confounding channel, we repeat the estimation above using only the *expected* component of dividends because, by definition, only surprise dividends, not expected dividends, convey news. At the end of each year, we predict split-adjusted dividends per share for each quarter of the next year. We take this approach because the market feedback effect regressions are at the annual frequency, but the shock is constructed using quarterly portfolio holdings data. Constructing expected dividends is straightforward. For dividends announced in the previous year, actual and expected dividends are the same. If no dividend is announced yet, we exploit that managers smooth split-adjusted dividends per share. We predict that the last dividend behavior continues in the next year, for each dividend frequency.

This simple prediction rule achieves a striking R^2 of 93% across stocks. We illustrate this in Appendix Figure A4 using a histogram of the relative prediction error. For the vast majority of dividends, the simple prediction is exactly accurate, as evidenced by the large mass at 0. However, the prediction misses dividend initiations and omissions — somewhat not surprinsingly — which correspond to the small bars at -1 and 1. In addition, it misses dividend increases, represented by the small bars between -1 and 0 (dividend decreases are too infrequent to be visible).

The second portion of 5 reports estimates of equation 5 using expected dividend-induced trading as the instrument for returns. The results are close to identical to the estimates using actual dividend payouts, albeit here the instrument is slightly less strong. Looking at the strictest specification in column 6, the F-statistic is 26 instead of 32 and the estimated market feedback effects are slightly larger, but statistically indistinguishable. This stability across point-estimates suggestes that the results are not driven by the potential news component of dividend-induced trading, rather reflecting price pressure.

4.3 Capital Structure Rebalancing Effect

How do firms finance the investment increase? The literature provides a couple of hypotheses in settings without exogenous shocks to stock prices. Baker and Wurgler (2002) find firms issue equity to take advantage of higher stock prices. In contrast, Leary and Roberts (2005), Flannery and Rangan (2006), and Kayhan and Titman (2007) show firms slowly counteract stock price changes to move towards their target debt ratio, consistent with survey evidence in Graham and Harvey (2001) that most CFOs have a target capital structure. In our context, firms do not necessarily experience higher returns when their investment opportunities improve and vice versa — instead, returns are driven by buying pressure. Accordingly, firms may issue equity to raise capital for new investment opportunities or repurchase stock due to a lack thereof.

We test this by estimating equation 5 but substituting the investment rate for the debt or equity issuance rate. We show the results in Table 6 which reports estimates corresponding to columns (3) and (4) in Table 5, i.e. the specification including controls and time fixed effects. We report results for alternative specifications in the Appendix, as described below. We find firms increase debt, not equity. For debt, the 2nd stage coefficient is 0.28 and statistically significant at the 5% level. In response to a 1% return, firms increase debt by 0.28%. This means a firm undoes about one quarter of the stock return's impact on its debt to equity ratio. For equity, we find a positive but statistically insignificant coefficient of 0.06. Together, these findings mean firms partially undo *exogenous* stock returns' effects on leverage. They issue debt to rebalance their capital structure and use the funds for real investment.

4.4 Robustness

We briefly summarize the results of a battery of tests contained in the Appendix. In Table A3, we find the same results qualitatively when excluding payout firms. In fact, the coefficients in the first and second stage increase. We also show that the results are robust to omitting within-industry flows. We implement this by constructing payout flows assuming that investors reinvest into their existing portfolio, but not if a stock is in the same industry. While this is a poor empirical description of investor behavior, it allows us to test whether within-industry flows drive the results, which would be inconsistent with the price pressure channel. We find that this is not the case. The estimated coefficients are virtually unchanged. Next, we show that clustering by time (our baseline specification) is substantially more conservative than clustering by firm and as conservative as double clustering by time and firm. This indicates that error terms are correlated in the cross-section, but less in the time series.

Finally, Appendix Tables A4 and A5 report results for alternative specifications of the regressions that test for market feedback effects on corporate financing. Appendix Table A4 reports results for the stricter specification that includes time×industry fixed effects. Appendix Table A5 also includes time×industry fixed effects, but in addition, it uses *expected* dividend-induced trading as the instrument for returns. The results are robust to either specification.

5 Conclusion

Cash payouts by US public companies are economically large: the average annual cash payout is almost 6% of market equity and dollar total cash payouts peaked in 2018, at almost $2 trillion. This paper uses the reinvestment of cash payouts by financial institutions as a nonfundamental shock to asset prices to estimate the slope of the demand curve for stocks as well as the real effects of stock price increases on corporate financing and investment.

Exploiting the separation of announcement and payment at the daily frequency, we find price pressure spillover effects of firm payouts on other stocks held in the same portfolios of financial institutions which identify an asset demand elasticity of 1.25. Dividends in particular generate payment date price pressure but no announcement date news spillover effects, suggesting that dividend-induced trading is plausibly exogenous to fundamentals.

Therefore, we use dividend-induced trading as an instrument for stock returns and document a releveraging market feedback effect on investment where firms respond to an exogenous stock price increase by issuing debt and use the funds to invest. We estimate that firms undo about a quarter of a nonfundamental stock price increase's impact on their debt to equity ratio by issuing debt over the following year. This informs the recurring policy debate on whether to restrict payouts so firms invest instead. This paper finds a new channel which implies that capital investment occurs despite payouts — it just happens at other firms.

In future work, payout-induced trading could be used to shed light on new feedback effects of financial markets. In particular, payout-induced trading is a shock that hits all stocks almost every day. Therefore, unlike existing natural experiments for stock returns, it could be used to investigate heterogeneity in price pressure and market feedback effects, as well as high-frequency market feedback effects.

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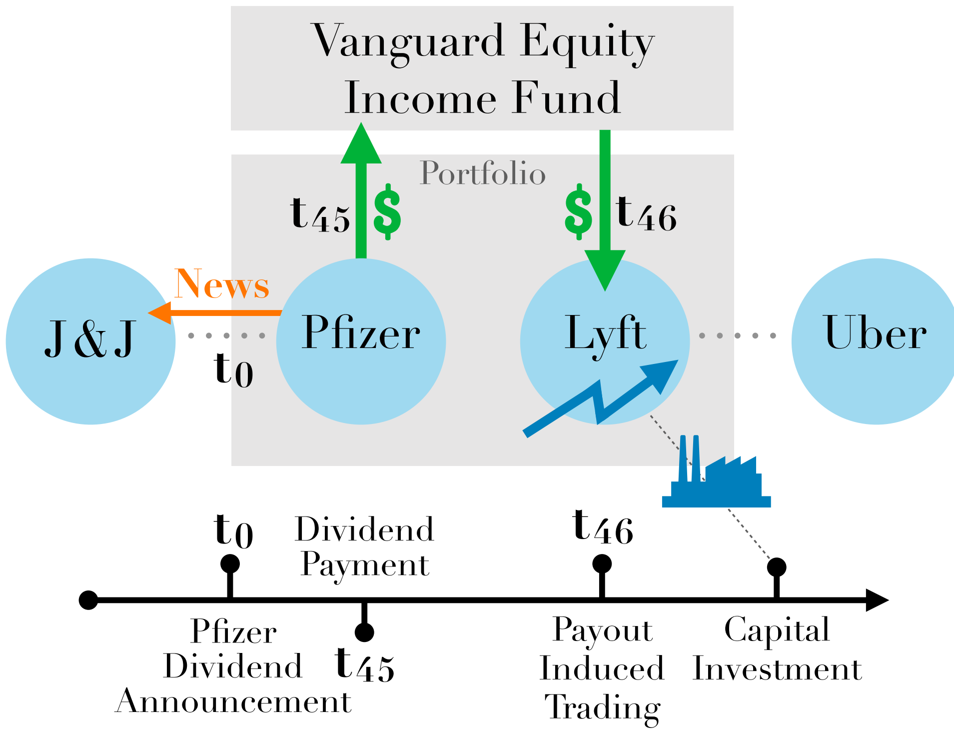
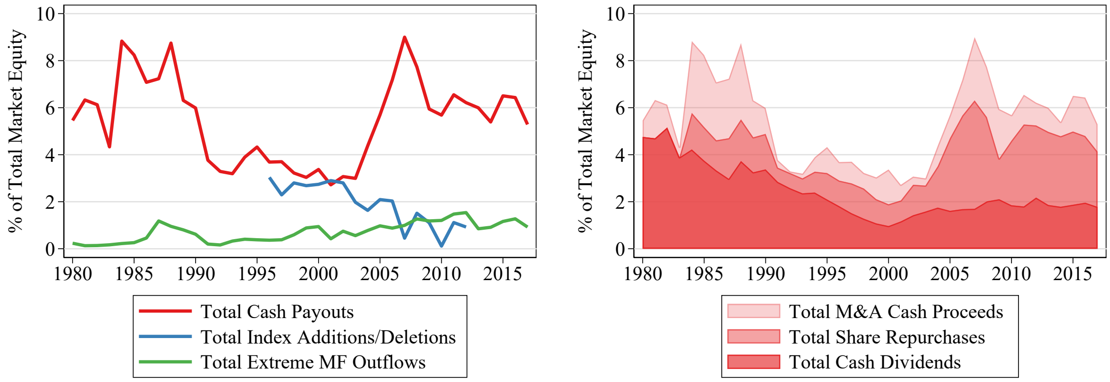
Figure 1: Payout-Induced Trading Mechanism



Payout-induced trading mechanism. At t_0, Pfizer announces a cash dividend. This reveals fundamental information about Pfizer and potentially also about peer firms such as Johnson & Johnson. The dividend is paid with a 45-day lag at t_{45}. Vanguard Equity Income Fund holds Pfizer and hence receives a dividend check. As the fund is benchmarked and/or a closet-indexer, it reinvests the cash into its portfolio. We call these purchases *payout-induced trading*. Payout-induced trading may occur immediately on t_{45} and/or over the following days. Here, Lyft is held in the same portfolio as Pfizer and is thus subject to payout-induced trading. This demand shock pushes up Lyft's price relative to industry peers. Then, in response to the lower cost of capital, Lyft increases investment.

Figure 2: Total Cash Payouts vs. Alternative Sources of Asset Demand Shocks

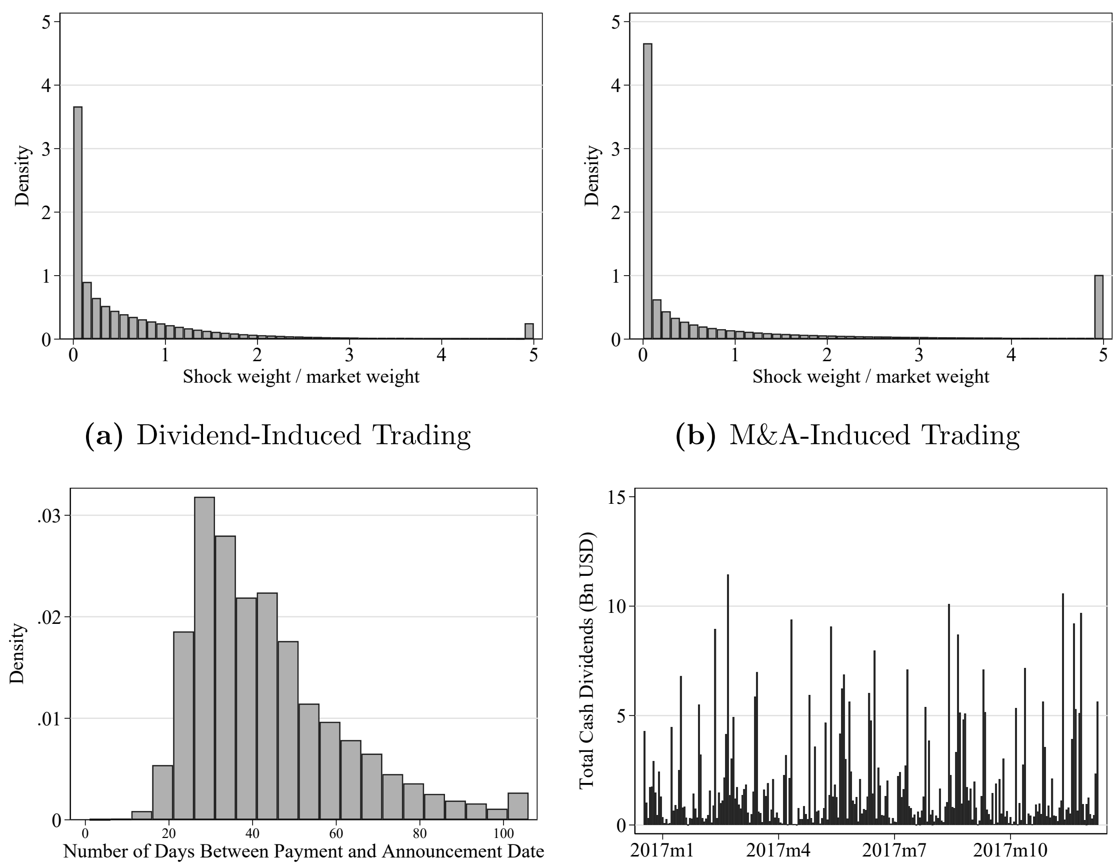


(a) Sources of Asset Demand Shocks

(b) Payout Type Breakup

Firms pay out 6% of market equity in cash. The first plot compares total cash payouts to two prominent, alternative sources for asset demand shocks: total absolute extreme mutual fund outflows and total market value of firms that are added/deleted from the Russell 1000/2000 index. All three are scaled by total market equity. Total cash payouts are the sum of cash dividends, share repurchases, and cash M&A payouts. These range anywhere from 4–9% of market equity (equivalent to $2 trillion in 2017). Total extreme mutual fund (MF) outflows are the absolute value of the sum of quarterly mutual fund level flows less than −5%. These are the source of the current state-of-the-art shock for stock returns (Wardlaw, 2018). Total index additions/deletions are the sum of the market values of all firms that switch from the Russell 2000 to the Russell 1000 or vice versa. This shock is as in Chang, Hong, and Liskovich (2015). The second plot breaks payout into its three components.

Figure 3: Variation in Investor Holdings and Dividend Announcement-Payment Dates



(a) Dividend-Induced Trading

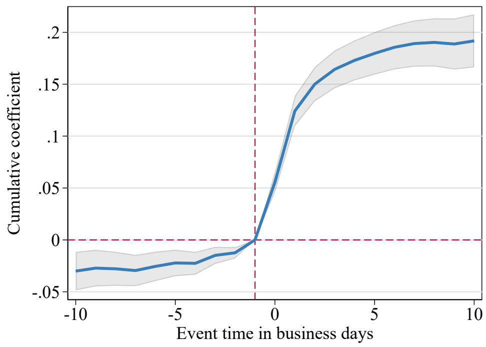
(b) M&A-Induced Trading

(c) Dividend Payment-Announcement Lag

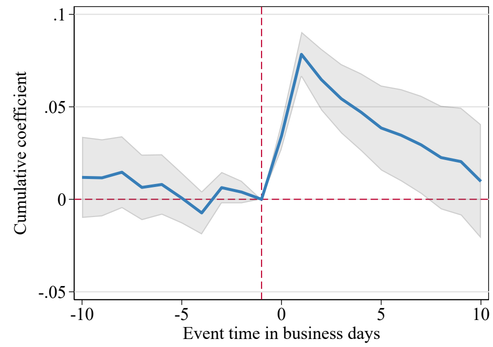
(d) Daily Cash Dividends in 2017

Two sources of variation drive the payout-induced trading shock. The top portion of the figure illustrates investor heterogeneity and cross-sectional variation of payout-induced trading. Plots **(a)** and **(b)** show histograms for the ratio between a stocks' share of dollar payout-induced trading on a given day and its market weight. This construction eliminates time series variation. If all investors held the market portfolio, the ratio would be exactly 1 for all observations. The ratio is not bounded above, so we winsorize it at 5 for this figure. **(a)** shows the histogram for the cash dividend shock. **(b)** shows the histogram for the cash M&A shock. The sample is the daily stock level panel from 1980 to 2017. **The second portion of the figure** illustrates the variation in timing of cash dividend payouts. Plot **(c)** shows the distribution of the number of days between the dividend payment and announcement date. The number of days can be very large, so we winsorize it at the 99th percentile for this figure. We single out dividends because they allow for the cleanest identification of the payout-induced trading effect because of the gap between the announcement and payment date. Plot **(d)** shows total cash dividends for each day in 2017.

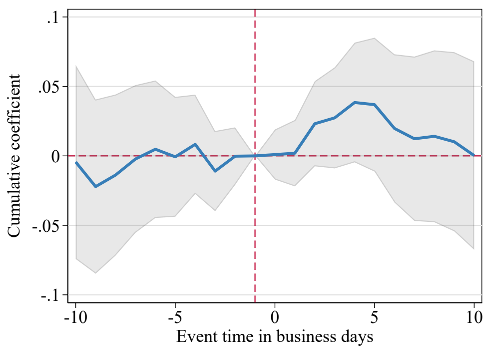
Figure 4: Abnormal Returns Around Dividend Announcement and Payment Dates



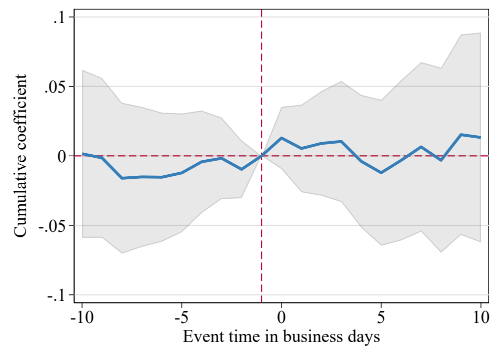
(a) Announcement, payout stock



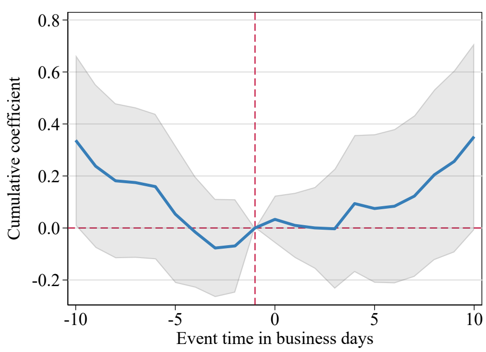
(b) Payment, payout stock



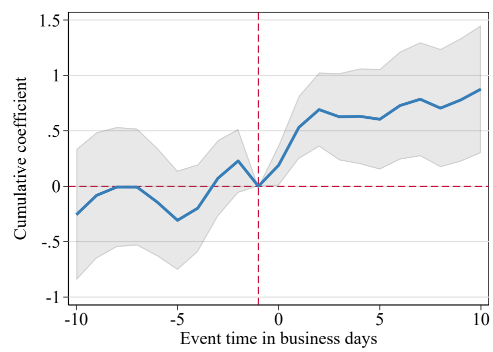
(c) Announcement, industry peers



(d) Payment, industry peers



(e) Announcement, connected stocks



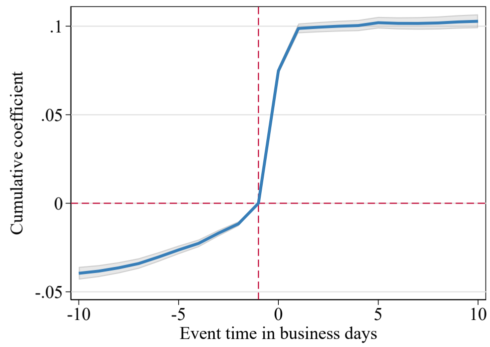
(f) Payment, connected stocks

Dividend payouts increase returns of stocks held in the same portfolio. The plots show percent cumulative abnormal returns in response to a 1% shock around dividend announcement and payment dates of the firm itself (**(a)** and **(b)**), 3-digit SIC code industry peers (**(c)** and **(d)**), and other stocks held in the same portfolios of financial institutions (**(e)** and **(f)**). Coefficients are comparable across columns but not across rows, because different rows report coefficients on different variables. The plots are based on estimates of γ_l from equation 4:

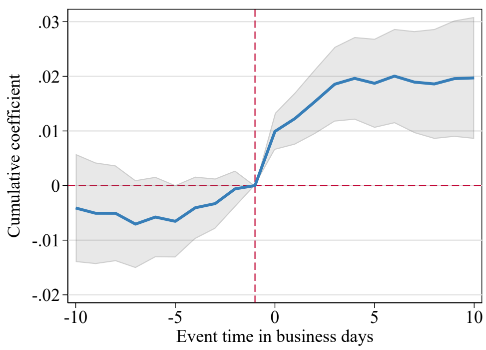
$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n)$$

with standard errors clustered by time. The vector of shocks Z contains the payout to price ratio, the industry payout ratio, and payout-induced trading. We show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), we sum the lead and lag coefficients separately. The regressions in graph **(a)** and **(b)** use the full sample. Graphs **(c)** to **(f)** use the sample that excludes payout firms to isolate spillover effects. Controls include risk factor characteristics (beta, log market equity, profitability, investment, and momentum) and dividend to book equity. The sample is daily from 1980 to 2017.

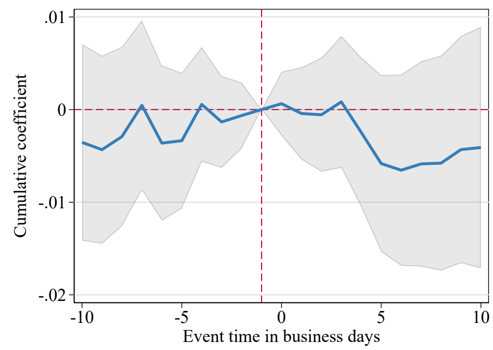
Figure 5: Abnormal Returns Around M&A Announcement and Payment Dates



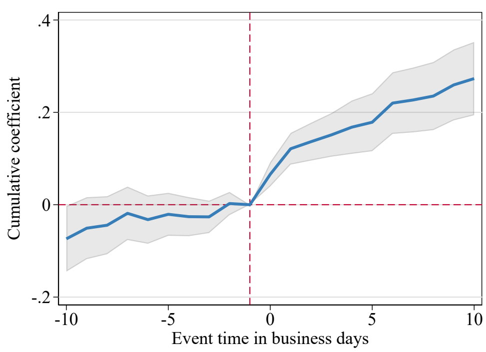
(a) Announcement, payout stock



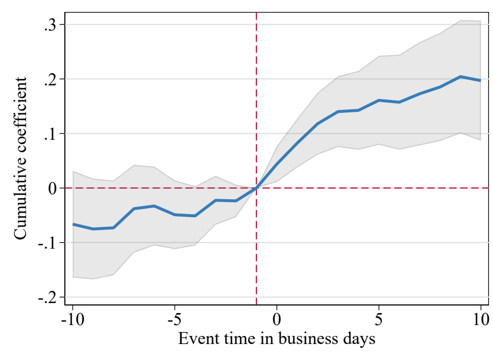
(b) Announcement, industry peers



(c) Payment, industry peers



(d) Announcement, connected stocks



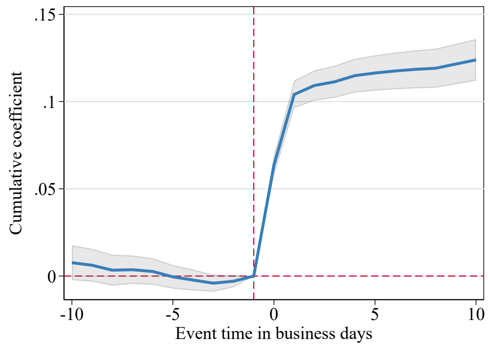
(e) Payment, connected stocks

M&A payouts increase returns of stocks held in the same portfolio. The plots show percent cumulative abnormal returns in response to a 1% shock around cash M&A announcement and payment dates of the firm itself **(a)**, 3-digit SIC code industry peers (**(b)** and **(c)**), and other stocks held in the same portfolios of financial institutions (**(d)** and **(e)**). Coefficients are comparable across columns but not across rows, because different rows report coefficients on different variables. The plots are based on estimates of γ_l from equation 4:

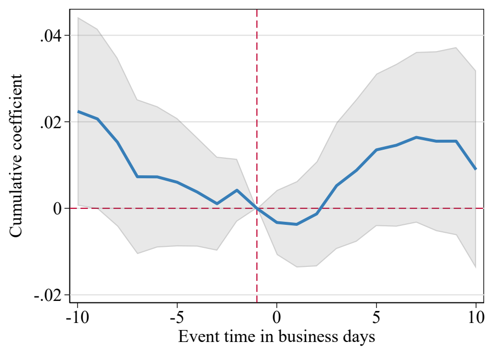
$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n)$$

with standard errors clustered by time. The vector of shocks Z contains the payout to price ratio, the industry payout ratio, and payout-induced trading. We show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), we sum the lead and lag coefficients separately. The regression in graph **(a)** uses the full sample. Graphs **(b)** to **(e)** use the sample that excludes payout firms to isolate spillover effects. Controls include risk factor characteristics (beta, log market equity, profitability, investment, and momentum) and dividend to book equity. The sample is daily from 1980 to 2017.

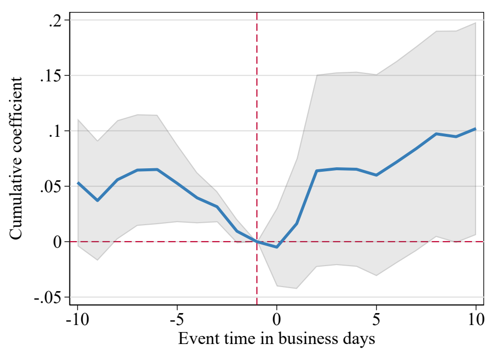
Figure 6: Abnormal Returns Around Share Repurchase Announcement Dates



(a) Announcement, payout stock



(b) Announcement, industry peers



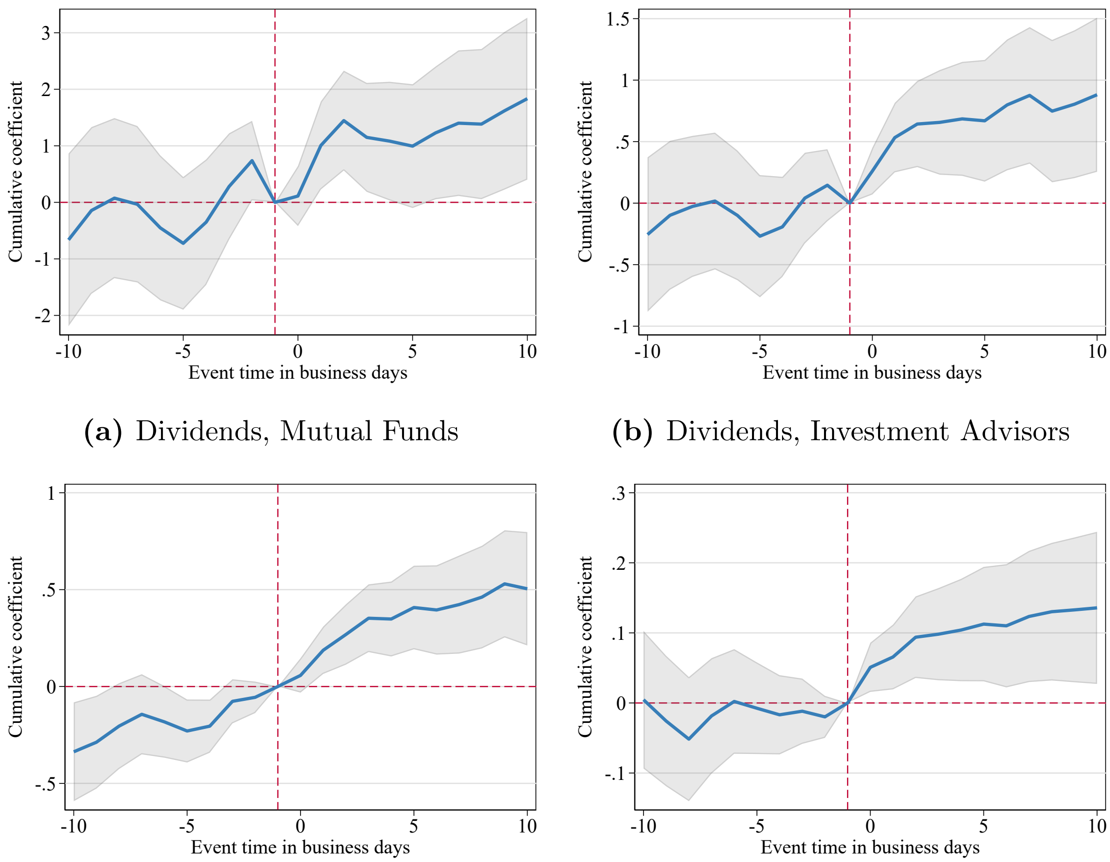
(c) Announcement, connected stocks

Share repurchase announcements do not reveal information on stocks held in the same portfolio. The plots show percent cumulative abnormal returns in response to a 1% shock around share repurchase announcement and payment dates of the firm itself **(a)**, 3-digit SIC code industry peers **(b)**, and other stocks held in the same portfolios of financial institutions **(c)**. Coefficients are comparable across columns but not across rows, because different rows report coefficients on different variables. The plots are based on estimates of γ_l from equation 4:

$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n)$$

with standard errors clustered by time. The vector of shocks Z contains the payout to price ratio, the industry payout ratio, and payout-induced trading. We show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), we sum the lead and lag coefficients separately. The regression in graph **(a)** uses the full sample. Graphs **(b)** and **(c)** use the sample that excludes payout firms to isolate spillover effects. Controls include risk factor characteristics (beta, log market equity, profitability, investment, and momentum) and dividend to book equity. The sample is daily from 1980 to 2017.

Figure 7: Price Impact of Payout-Induced Trading — Institutional Heterogeneity



(a) Dividends, Mutual Funds

(b) Dividends, Investment Advisors

(c) M&A, Mutual Funds

(d) M&A, Investment Advisors

Benchmarked institutions generate larger price responses. The plots show percent cumulative abnormal returns in response to a 1% shock around dividend and M&A payment dates of other stocks held in the same portfolios of financial institutions. Coefficients are comparable across columns but not across rows, because different rows report coefficients on different variables. The plots are based on estimates of γ_l from equation 4:

$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n)$$

with standard errors clustered by time. Z contains the payout-induced trading shock. We show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), we sum the lead and lag coefficients separately. The estimating sample excludes payout firms to isolate spillover effects. Plots **(a)** and **(b)** show results for cash dividend payouts using holdings by mutual funds and investment advisors (e.g. hedge funds). Plots **(c)** and **(d)** use cash M&A payments. Controls include risk factor characteristics (beta, log market equity, profitability, investment, and momentum) and dividend to book equity. The sample is daily from 1980 to 2017.

Table 1: Summary Statistics for Financial Institutions

Period	# Inst.	Mkt share (%)	AuM ($million)		# stocks held		Payout flows (%)	
			50th	90th	50th	90th	50th	90th
Panel A: Mutual Funds								
1980-1989	401	4	63	486	46	103	4.5	9.6
1990-1999	1366	10	85	1008	56	170	3.0	6.0
2000-2009	3167	20	114	1525	64	312	4.0	7.8
2010-2017	3426	25	168	2353	59	368	5.1	8.7
Panel B: Investment Advisors								
1980-1989	224	7	244	1154	81	240	4.8	11.3
1990-1999	588	9	217	1255	73	221	3.2	6.8
2000-2009	1621	13	227	1762	66	247	3.9	9.1
2010-2017	2857	21	204	2295	53	270	5.1	9.1
Panel C: Banks								
1980-1989	220	15	386	3393	185	580	5.8	7.9
1990-1999	204	13	493	10250	213	960	3.7	4.6
2000-2009	167	12	396	15308	205	1322	4.5	5.9
2010-2017	160	12	404	18676	187	1232	5.7	6.8
Panel D: Pension Funds								
1980-1989	32	3	871	5613	154	549	5.8	8.3
1990-1999	36	4	1376	23765	367	1291	3.7	4.9
2000-2009	39	3	3536	37969	633	2229	4.9	5.7
2010-2017	53	3	4032	25819	512	1581	5.9	7.6
Panel E: Insurance companies								
1980-1989	67	3	389	2293	101	412	5.3	9.6
1990-1999	75	4	827	5307	136	800	3.4	4.8
2000-2009	58	4	1126	15097	203	1780	4.5	6.3
2010-2017	49	2	903	35447	185	2068	5.7	8.6
Panel F: Other financial institutions								
1980-1989	37	1	213	1116	55	192	4.9	10.0
1990-1999	32	1	251	2379	61	144	3.5	5.0
2000-2009	144	1	137	1401	37	236	3.7	11.7
2010-2017	179	2	168	3225	27	441	4.8	9.2

Do institutions hold concentrated portfolios? This table summarizes financial institution information by institution type and decade, reporting time-series means within the given period. We report the number of institutions, the market share of the institution type in %, the median and 90th percentile of assets under management in $ million, the number of stocks held in a portfolio, and payout flows in %. Payout flows are constructed at the institution level following equation 1 in the main text, then aggregated for each institutional type: $PayoutFlow_{i,t} = \sum_{n=1}^{N} Payout_t(n) \times Shares_{i,t-1}(n)$, where institution i holds $Shares_{i,t-1}(n)$ shares of stock n, which pays out $Payout_t(n)$ per share, and dollar payout flows are the sum of the payouts from all N stocks. The investment advisor type includes e.g. hedge funds. The "other" type includes e.g. endowments. The sample is 1980 to 2017.

Table 2: Trading Response to Dividend Payouts — Mutual Funds

	Including payout stocks		Excluding payout stocks	
	(1)	(2)	(3)	(4)
Payout Flow	0.794***	0.829***	0.572***	0.637***
	(0.056)	(0.056)	(0.069)	(0.071)
Controls		✓		✓
Institution FE	✓	✓	✓	✓
Time×stock FE	✓	✓		
R-squared	0.108	0.103	0.141	0.134
Observations	17,677,791	16,080,367	687,0472	610,0798

Do mutual funds reinvest payouts into their existing portfolio? Reported estimates come from the regression:

$$\frac{\Delta Shares_{i,t}(n)}{Shares_{i,t-1}(n)} = \alpha_i + \alpha_t(n) + \beta PayoutFlow_{i,t} + \gamma' X_{i,t}(n) + \epsilon_{i,t}(n)$$

which measures the relative change in shares held by mutual funds given payout flows, defined as $PayoutFlow_{i,t} = \sum_{n=1}^{N} Payout_t(n) \times Shares_{i,t-1}(n)$. Columns (3) and (4) are estimated off a sample of mutual funds that excludes payout stocks. Specifications (2) and (4) control for fund flows. (1) and (2) contain institution and stock×time fixed effects. The regression is at the portfolio position level. For small portfolio positions, even small trades imply large relative changes in shares held. To prevent these minor adjustments from driving the results, we exclude small positions with portfolio weights less than 0.1%. As the relative change in shares held is not bounded above, we winsorize it at 100%. The frequency is quarterly and payouts use cash dividend payments. We report standard errors clustered by institution×time in parentheses. The sample is 1980 to 2017. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table 3: Trading Response to Dividend Payouts — All Institutions

	(1) MF	(2) IA	(3) Bank	(4) PF	(5) IC	(6) Other
Payout Flow	0.637***	0.387***	0.00955	1.776***	0.402**	0.610***
	(0.0713)	(0.0612)	(0.184)	(0.475)	(0.169)	(0.181)
R-squared	0.134	0.150	0.274	0.382	0.310	0.330
Observations	610,0798	3,426,851	343,519	128,482	161,486	148,889

Do institutions vary in how they reinvest payouts into their existing portfolio? Reported estimates come from the regression:

$$\frac{\Delta Shares_{i,t}(n)}{Shares_{i,t-1}(n)} = \alpha_i + \alpha_t(n) + \beta PayoutFlow_{i,t} + \gamma' X_{i,t}(n) + \epsilon_{i,t}(n)$$

which measures the relative change in shares held by mutual funds given payout flows, defined as $PayoutFlow_{i,t} = \sum_{n=1}^{N} Payout_t(n) \times Shares_{i,t-1}(n)$. Columns (1) to (6) report results for **mutual funds (MF)**, **investment advisors** (e.g. asset managers) **(IA)**, **banks**, **pension funds (PF)**, **insurance companies (IC)**, and **unclassified institutions** (e.g. endowments) **(Other)**. We report results for the strictest specification corresponding to column (4) of table 2, i.e. including controls and excluding payout stocks. As the relative change in shares held is not bounded above, we winsorize it at 100%. The regression contains institution and stock×time fixed effects. The regression is at the portfolio position level. For small portfolio positions, even small trades imply large relative changes in shares held. To prevent these minor adjustments from driving the results, we exclude small positions with portfolio weights less than 0.1%. The frequency is quarterly. We report standard errors clustered by institution×time in parentheses. The sample is 1980 to 2017 and cash payouts use dividend payments. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table 4: Correlation Between Firm Characteristics and PIT Exposure and Dividend Yield

	PIT	Dividend/Price
Market beta	−0.104**	−0.290***
	(0.044)	(0.034)
Log market equity	0.990***	0.242***
	(0.077)	(0.023)
Log Tobin's Q	−0.649***	−0.259***
	(0.053)	(0.034)
Profitability	−0.673***	0.046
	(0.240)	(0.120)
Investment	−1.570***	−0.855***
	(0.122)	(0.076)
R-squared	0.467	0.233
Observations	149,313	149,313

Are firms more exposed to payouts different from those paying? This table shows estimates of regressions of dividend-induced trading, derived in the paper as:

$$PIT_t(n) = \frac{\sum_{i=1}^{I} \widetilde{PayoutFlow}_{i,t} \times Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}$$

where $\widetilde{PayoutFlow}_{i,t}(n) = \sum_{m \neq n} Payout_t(m) \times Shares_{i,t-1}(m)$, and of dividend yield on firm characteristics and time fixed effects. The frequency is annual. The sample is 1980 to 2017. We report standard errors clustered by time in parentheses. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table 5: The Market Feedback Effect on Investment

	(1) OLS	(2) FS	(3) 2SLS	(4) FS	(5) 2SLS	(6) FS	(7) 2SLS
Return	0.083***		0.100***		0.171***		0.151***
	(0.006)		(0.026)		(0.041)		(0.041)
PIT		0.021***		0.014***		0.012***	
		(0.003)		(0.002)		(0.002)	
		53.07^F		56.06^F		31.75^F	
Return			0.107***		0.192***		0.172***
			(0.027)		(0.046)		(0.045)
PIT(Expected)		0.019***		0.012***		0.010***	
		(0.003)		(0.002)		(0.002)	
		46.73^F		46.21^F		26.53^F	
Controls				✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓		
Time×industry FE						✓	✓
Observations	112,444	112,444	112,444	112,076	112,076	109,725	109,725

How does a firm's investment rate change given its previous annual stock return? The estimates come from the instrumental variable regression:

$$\Delta \frac{I_t(n)}{K_{t-1}(n)} = \eta_t + \beta r_{t-1}(n) + \tilde{\gamma}' \tilde{X}_{t-1}(n) + \epsilon_t(n),$$

which instruments returns (r) with the payout-induced trading variable derived in the paper:

$$PIT_t(n) = \frac{\sum_{i=1}^{I} \widetilde{PayoutFlow}_{i,t} \times Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}$$

where $\widetilde{PayoutFlow}_{i,t}(n) = \sum_{m \neq n} Payout_t(m) \times Shares_{i,t-1}(m)$. The regression includes as controls risk factor characteristics (beta, log market equity, profitability, investment, and momentum), dividend to book equity, payout ratios for all payout types (cash dividends, stock dividends, cash M&A, stock M&A, and share repurchases), and fixed effects depending on the specification. Column (1) shows the results of an OLS regression without controls and only year dummies. Columns (2) and (3) show first and second stages of 2SLS regressions without controls. Columns (4) and (5) replicate columns (2) and (3) with the addition of controls and the last two columns do the same, but with alternative fixed effects. **The top portion of the table** uses the instrument PIT constructed with actual cash dividend payouts. **The second portion of the table** reports regressions instrumented with *expected* dividend payouts, which we construct following a simple predictive rule. At the end of each year, we predict split-adjusted dividends per share for each quarter of the next year. For dividends announced in the previous year, actual and expected dividends are the same. If no dividend is announced yet, we exploit that managers smooth split-adjusted dividends per share. The rule for prediction assumes that the last dividend behavior continues in the next year, for each dividend frequency. The frequency of all regressions is annual and the sample span 1980 to 2017. We report standard errors clustered by time in parentheses. ^{F} denotes Kleibergen and Paap (2006) F-statistics. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table 6: Capital Structure Rebalancing Effect

	Debt			Equity		
	(1) OLS	(2) FS	(3) 2SLS	(4) OLS	(5) FS	(6) 2SLS
Return	0.049***		0.282**	−0.040**		0.063
	(0.012)		(0.107)	(0.017)		(0.083)
PIT		0.014***			0.015***	
		(0.002)			(0.002)	
Controls	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
F-statistic		59.78			49.11	
Observations	100,845	100,845	100,845	94,264	94,264	94,264

How do firms finance investment increases? The estimates come from the instrumental variable regression:

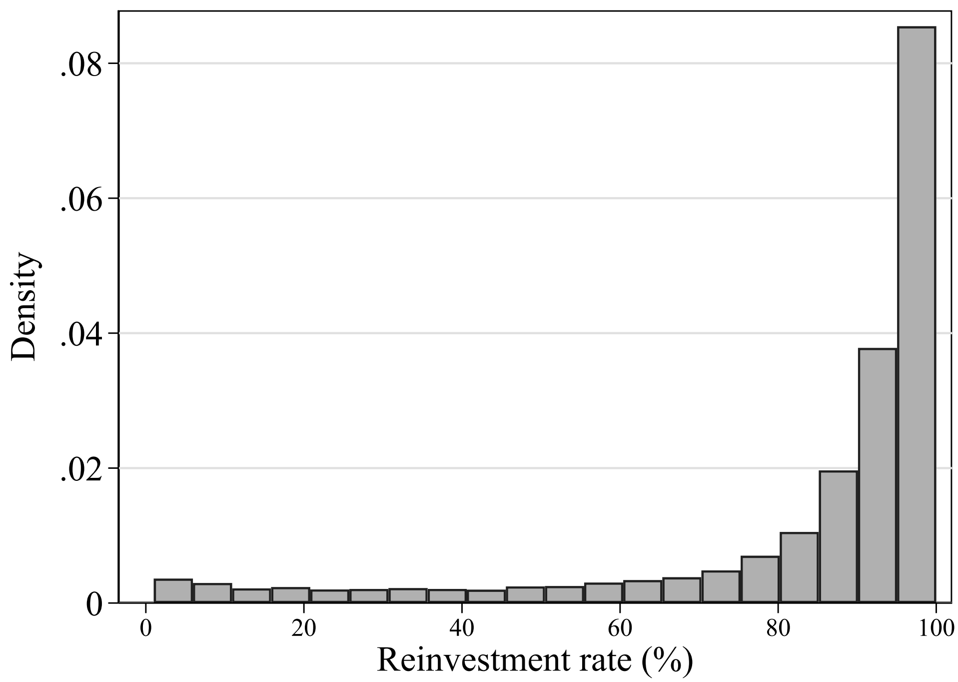
$$\text{Outcome}_t(n) = \eta_t + \beta r_{t-1}(n) + \tilde{\gamma}' \tilde{X}_{t-1}(n) + \epsilon_t(n),$$

which instruments returns (r) with the payout-induced trading variable derived in the paper:

$$PIT_t(n) = \frac{\sum_{i=1}^{I} \widetilde{PayoutFlow}_{i,t} \times Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}$$

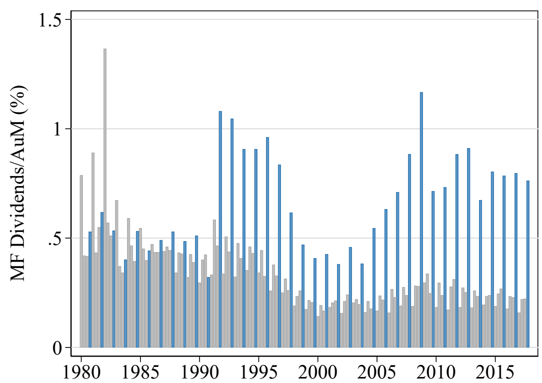
where $\widetilde{PayoutFlow}_{i,t}(n) = \sum_{m \neq n} Payout_t(m) \times Shares_{i,t-1}(m)$. Outcomes are the change in total liabilities in columns (1) to (3) and the change in common equity in columns (4) to (6). The regression includes as controls risk factor characteristics (beta, log market equity, profitability, investment, and momentum), dividend to book equity, payout ratios for all payout types (cash dividends, stock dividends, cash M&A, stock M&A, and share repurchases), except that we drop Tobin's Q from the equity issuance regression because it contains book equity which is part of the dependent variable. The frequency of all regressions is annual and the sample span 1980 to 2017. We report standard errors clustered by time in parentheses and Kleibergen and Paap (2006) F-statistics in the table footer. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Figure A1: Mutual Fund Distribution Reinvestment Rate

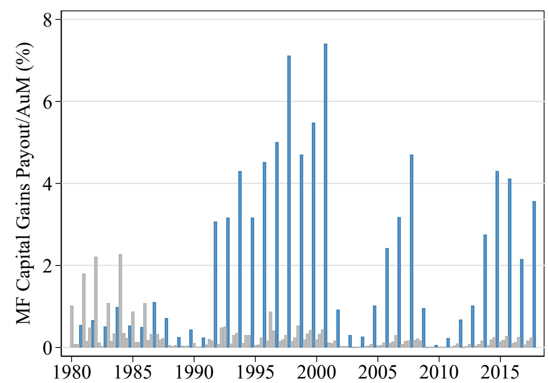


Households reinvest on average 83% of mutual fund distributions. This figure shows a histogram of the mutual fund distribution reinvestment rate. The sample is the quarterly mutual fund level panel from 1995 to 2017. The sample starts in 1995 because this is when Morningstar reinvestment data become reliably available. Over half of all funds face reinvestment rates above 90% and the vast majority of funds faces reinvestment rates above 50%.

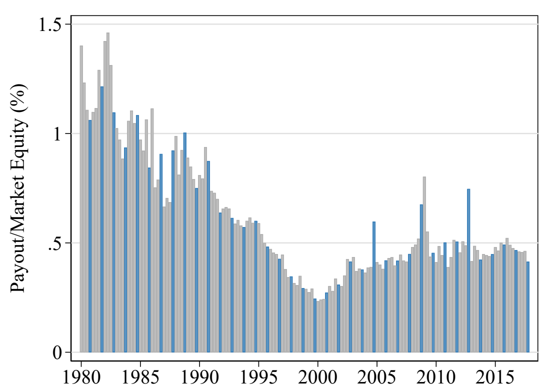
Figure A2: Quarterly Mutual Fund Distributions and Corporate Dividends



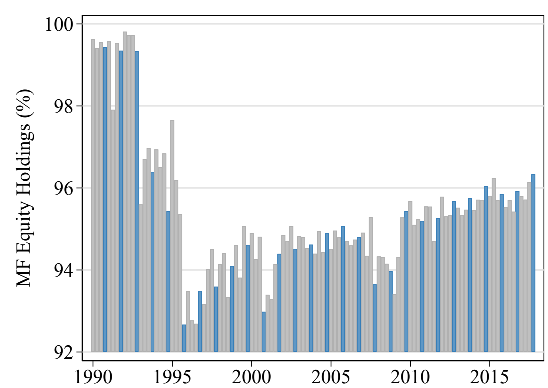
(a) Mutual fund dividend distribution



(b) Mutual fund capital gains distribution



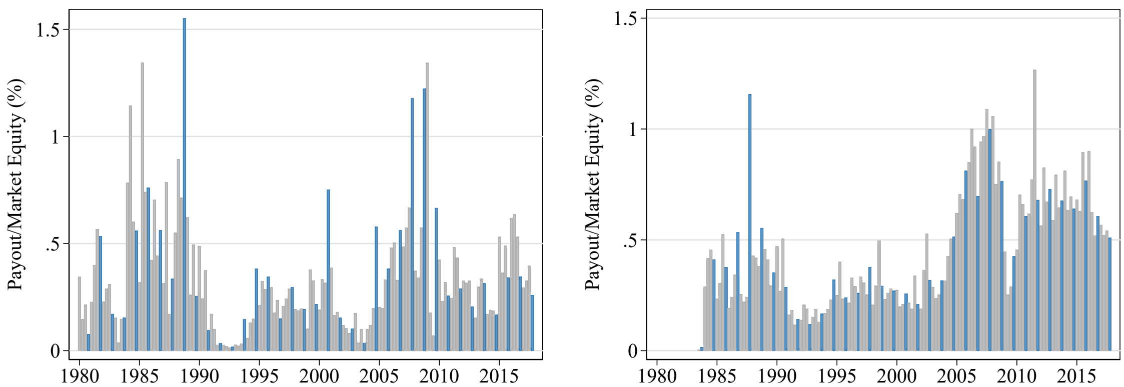
(c) Firm dividends



(d) Mutual fund equity holdings

Stock holdings pay dividends uniformly, but mutual funds distribute at the end of year. This figure contains time series plots, with the last quarter of each year highlighted in blue. Panels **(a)** and **(b)** plot the time series of aggregate mutual fund dividend and capital gains distributions as a fraction of total assets under management. For comparison, Panel **(c)** shows firm dividends as a fraction of total market equity. Panel **(d)** plots the aggregate mutual fund equity position as a fraction of assets under management, starting in 1990, when the Morningstar equity share variable becomes reliably available. The sample is from 1980 to 2017.

Figure A3: Quarterly Firm Payouts — M&A and Share Repurchases

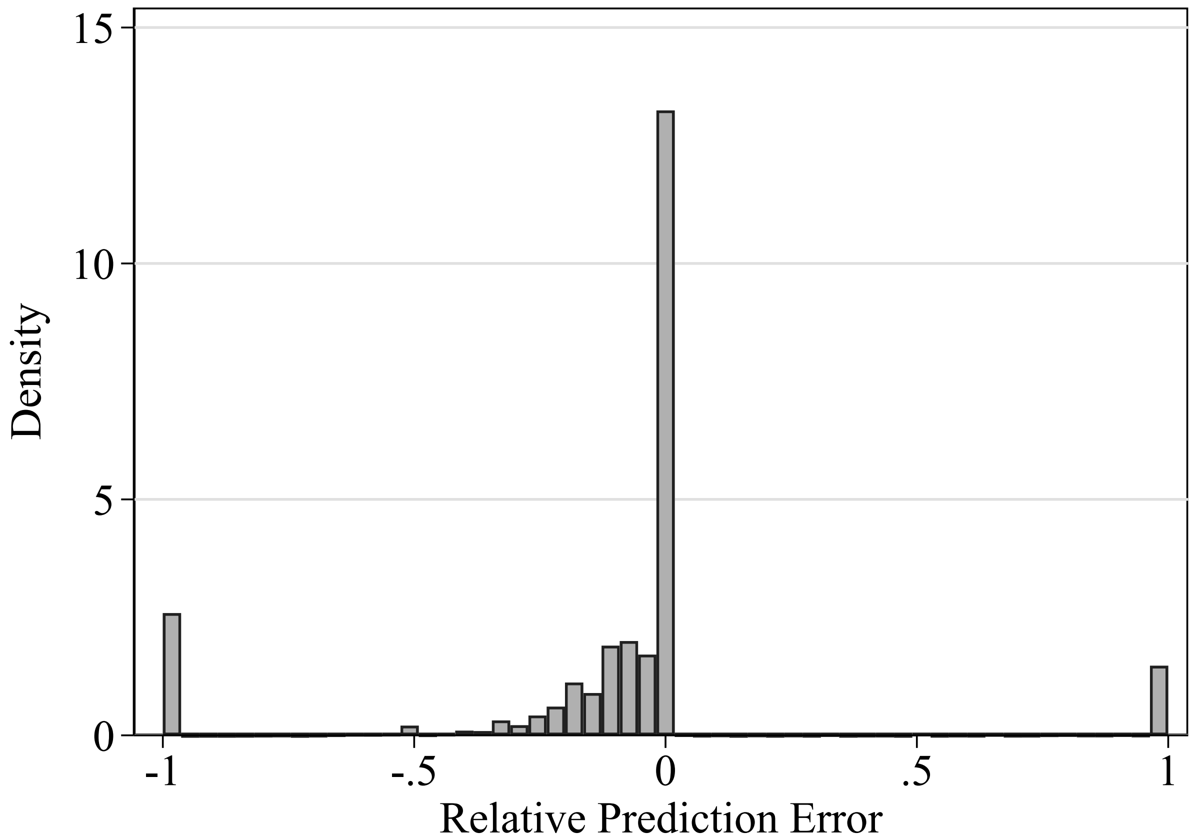


(a) Cash M&A payouts

(b) Share repurchases

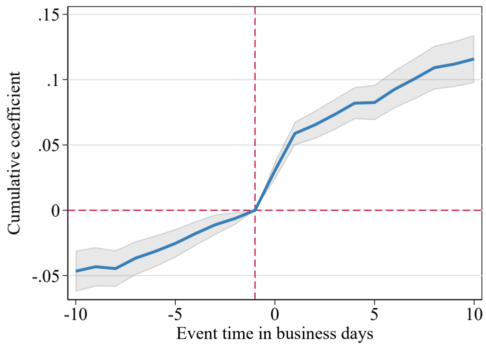
M&A share repurchase payouts happen uniformly during the year. This figure complements Figure A2. It contains time series plots, with the last quarter of each year highlighted in blue. The plots show firm payouts as a fraction of total market equity. Panel **(a)** shows cash M&A payouts, Panel **(b)** shows share repurchases. The sample is from 1980 to 2017.

Figure A4: Relative Dividend Prediction Error

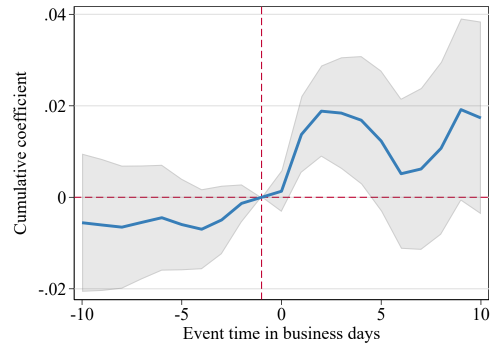


Can dividend payouts be accurely predicted? This histogram shows the distribution of prediction errors from a simple rule to predict dividend payouts ($ paid). At the end of each year, we predict split-adjusted dividends per share for each quarter of the next year. For dividends announced in the previous year, actual and expected dividends are the same. If no dividend is announced yet, we exploit that managers smooth split-adjusted dividends per share. The rule for prediction assumes that the last dividend behavior continues in the next year, for each dividend frequency. This simple prediction rule achieves a R^2 of 93% across stocks. Bars reported are relative prediction errors, i.e. the prediction error scaled by the prediction target. -1 and 1 are dividend initiations and dividend discontinuations, respectively. The sample is the daily stock level panel from 1980 to 2017.

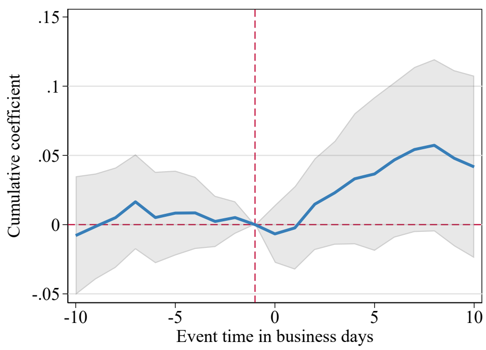
Figure A5: Returns Around Stock Dividend Announcement and Payment Dates — Placebo



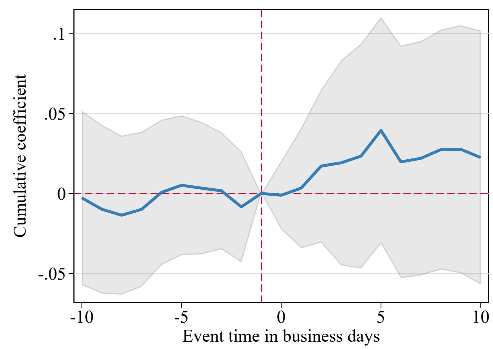
(a) Announcement, payout stock



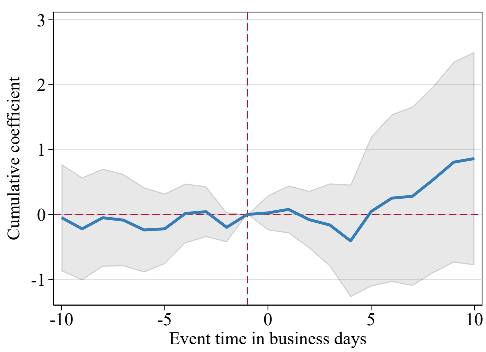
(b) Payment, payout stock



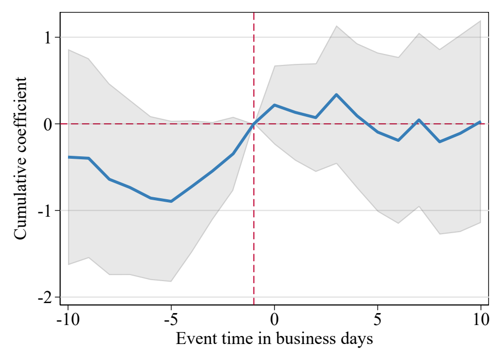
(c) Announcement, industry peers



(d) Payment, industry peers



(e) Announcement, connected stocks



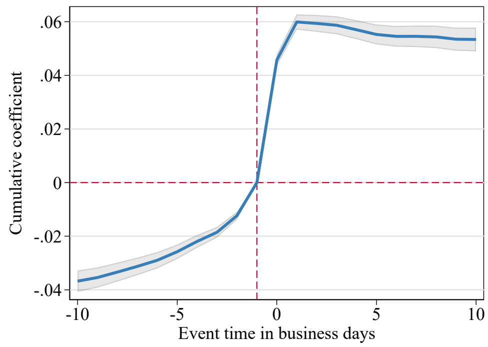
(f) Payment, connected stocks

Dividend payouts in stock do not affect returns of stocks held in the same portfolio. The plots show percent cumulative abnormal returns in response to a 1% shock around dividend announcement and payment dates of the firm itself (**(a)** and **(b)**), 3-digit SIC code industry peers (**(c)** and **(d)**), and other stocks held in the same portfolios of financial institutions (**(e)** and **(f)**). Coefficients are comparable across columns but not across rows, because different rows report coefficients on different variables. The plots are based on estimates of γ_l from equation 4:

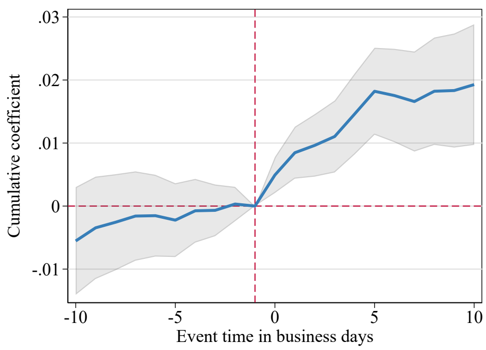
$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n)$$

with standard errors clustered by time. The vector of shocks Z contains the payout to price ratio, the industry payout ratio, and payout-induced trading. Payouts in this specification are constructed using stock rather than cash events. We show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), we sum the lead and lag coefficients separately. The regressions in graph **(a)** and **(b)** use the full sample. Graphs **(c)** to **(f)** use the sample that excludes payout firms to isolate spillover effects. Controls include risk factor characteristics (beta, log market equity, profitability, investment, and momentum) and dividend to book equity. The sample is daily from 1980 to 2017.

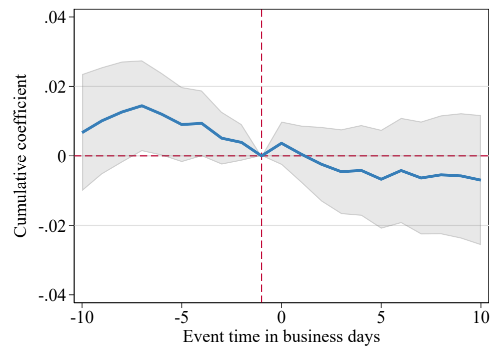
Figure A6: Returns Around Stock M&A Announcement and Payment Dates — Placebo



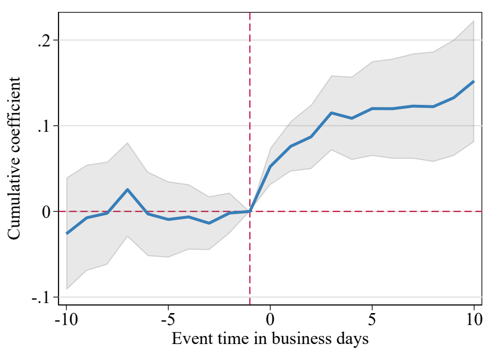
(a) Announcement, payout stock



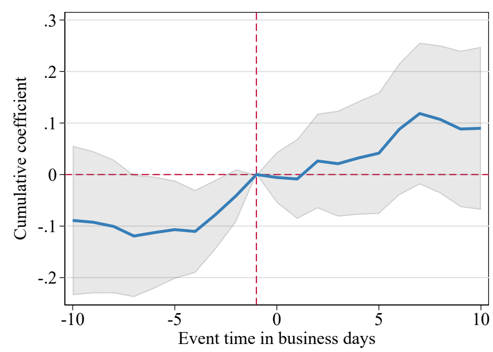
(b) Announcement, industry peers



(c) Payment, industry peers



(d) Announcement, connected stocks



(e) Payment, connected stocks

M&A payouts in stock do not affect returns of stocks held in the same portfolio. The plots show percent cumulative abnormal returns in response to a 1% shock around stock M&A announcement and payment dates of the firm itself **(a)**, 3-digit SIC code industry peers (**(b)** and **(c)**), and other stocks held in the same portfolios of financial institutions (**(d)** and **(e)**). Coefficients are comparable across columns but not across rows, because different rows report coefficients on different variables. The plots are based on estimates of γ_l from equation 4:

$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n)$$

with standard errors clustered by time. The vector of shocks Z contains the payout to price ratio, the industry payout ratio, and payout-induced trading. Payouts in this specification are constructed using stock rather than cash events. We show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), we sum the lead and lag coefficients separately. The regression in graph **(a)** uses the full sample. Graphs **(b)** to **(e)** use the sample that excludes payout firms to isolate spillover effects. Controls include risk factor characteristics (beta, log market equity, profitability, investment, and momentum) and dividend to book equity. The sample is daily from 1980 to 2017.

Table A1: Mutual Fund Trading Response to Dividend Payouts — Cash M&A

	Including payout firms		Excluding payout firms	
	(1)	(2)	(3)	(4)
M&A Flow	0.557***	0.569***	0.588***	0.602***
	(0.012)	(0.012)	(0.012)	(0.013)
Controls		✓		✓
R-squared	0.157	0.152	0.129	0.124
Observations	17,677,773	16,080,338	17,546,880	15,963,488

Do mutual funds reinvest payouts into their existing portfolio? Reported estimates come from the regression:

$$\frac{\Delta Shares_{i,t}(n)}{Shares_{i,t-1}(n)} = \alpha_i + \alpha_t(n) + \beta PayoutFlow_{i,t} + \gamma' X_{i,t}(n) + \epsilon_{i,t}(n)$$

which measures the relative change in shares held by mutual funds given payout flows, defined as $PayoutFlow_{i,t} = \sum_{n=1}^{N} Payout_t(n) \times Shares_{i,t-1}(n)$. This table is analogous to table 2, except that we examine cash M&A instead of dividend flows. Columns (3) and (4) are estimated off a sample of mutual funds that excludes payout stocks. Specifications (2) and (4) control for fund flows. (1) and (2) contain institution and stock×time fixed effects. The regression is at the portfolio position level. For small portfolio positions, even small trades imply large relative changes in shares held. To prevent these minor adjustments from driving the results, we exclude small positions with portfolio weights less than 0.1%. As the relative change in shares held is not bounded above, we winsorize it at 100%. The frequency is quarterly. We report standard errors clustered by institution×time in parentheses. The sample is 1980 to 2017. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A2: Institutional Trading Response to Dividend Payouts — Cash M&A

	(1) MF	(2) IA	(3) Bank	(4) PF	(5) IC	(6) Other
M&A Flow	0.602***	0.0878***	0.147***	0.462***	0.217***	0.0665*
	(0.0128)	(0.00718)	(0.0232)	(0.0557)	(0.0407)	(0.0341)
R-squared	0.124	0.168	0.139	0.236	0.208	0.321
Observations	15,963,488	10,852,070	293,2246	761,949	880,874	557,560

Do institutions vary in how they reinvest payouts into their existing portfolio? Reported estimates come from the regression:

$$\frac{\Delta Shares_{i,t}(n)}{Shares_{i,t-1}(n)} = \alpha_i + \alpha_t(n) + \beta PayoutFlow_{i,t} + \gamma' X_{i,t}(n) + \epsilon_{i,t}(n)$$

which measures the relative change in shares held by mutual funds given payout flows, defined as $PayoutFlow_{i,t} = \sum_{n=1}^{N} Payout_t(n) \times Shares_{i,t-1}(n)$. This table is analogous to table 3, except that we examine cash M&A instead of dividend flows. Columns (1) to (6) report results for **mutual funds (MF)**, **investment advisors** (e.g. asset managers) **(IA)**, **banks**, **pension funds (PF)**, **insurance companies (IC)**, and **unclassified institutions** (e.g. endowments) **(Other)**. We report results for the strictest specification corresponding to column (4) of table 2, i.e. including controls and excluding payout stocks. As the relative change in shares held is not bounded above, we winsorize it at 100%. The regression contains institution and stock×time fixed effects. The regression is at the portfolio position level. For small portfolio positions, even small trades imply large relative changes in shares held. To prevent these minor adjustments from driving the results, we exclude small positions with portfolio weights less than 0.1%. The frequency is quarterly. We report standard errors clustered by institution×time in parentheses. The sample is 1980 to 2017. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A3: The Market Feedback Effect on Investment – Robustness & Alternative Specifications

	(1) FS	(2) 2SLS	(3) FS	(4) 2SLS	(5) FS	(6) 2SLS	(7) FS	(8) 2SLS	(9) FS	(10) 2SLS
PIT	0.025***		0.013***		0.014***		0.014***		0.014***	
	(0.004)		(0.002)		(0.002)		(0.001)		(0.002)	
Return		0.199***		0.187***		0.171***		0.171***		0.171***
		(0.042)		(0.043)		(0.041)		(0.019)		(0.037)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Exclude...										
Payout firms	✓	✓								
Within industry flows			✓	✓						
Clustering...										
Time	✓	✓	✓	✓	✓	✓				
Firm							✓	✓		
Firm×time									✓	✓
F-statistic	41.18		55.94		56.06		474.9		54.96	
Observations	72,637	72,637	112,076	112,076	112,076	112,076	112,076	112,076	112,076	112,076

How does a firm's investment rate change given its previous annual stock return? The estimates come from the instrumental variable regression:

$$\Delta \frac{I_t(n)}{K_{t-1}(n)} = \eta_t + \beta r_{t-1}(n) + \tilde{\gamma}' \tilde{X}_{t-1}(n) + \epsilon_t(n),$$

which instruments returns (r) with the payout-induced trading variable derived in the paper:

$$PIT_t(n) = \frac{\sum_{i=1}^{I} PayoutFlow_{i,t} \times Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}$$

where $PayoutFlow_{i,t}(n) = \sum_{n=1}^{N} Payout_t(n) \times Shares_{i,t-1}(n)$ (or the alternative $\widetilde{PayoutFlow}_{i,t}(n)$ when indicated). The regression includes as controls risk factor characteristics (beta, log market equity, profitability, investment, and momentum), dividend to book equity, payout ratios for all payout types (cash dividends, stock dividends, cash M&A, stock M&A, and share repurchases), and fixed effects depending on the specification. This table is analogous to table 5, except for an alternative sample, instrument construction, or assumptions about the distribution of the residual. Columns (1) and (2) exclude dividend-paying firms. Columns (3) and (4) use a version of the dividend-induced trading instrument that assumes investors do not reinvest into stocks with the same 3-digit SIC code. In columns (5) and (6), we cluster standard errors by time (repeats baseline specification for comparison). In columns (7) and (8), we cluster standard errors by firm. Lastly, in columns (9) and (10), we cluster standard errors by firm and time. The frequency of all regressions is annual and the sample span 1980 to 2017. Cash payouts use dividend payments. We report standard errors clustered by time in parentheses and Kleibergen and Paap (2006) F-statistics. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A4: Capital Structure Rebalancing Effect — Time×Industry Fixed Effects

	Debt			Equity		
	(1) OLS	(2) FS	(3) 2SLS	(4) OLS	(5) FS	(6) 2SLS
Return	0.046***		0.257*	−0.045***		0.021
	(0.012)		(0.134)	(0.016)		(0.096)
PIT		0.012***			0.013***	
		(0.002)			(0.002)	
F-statistic		33.16			34.42	
Observations	98,574	98,574	98,574	91,953	91,953	91,953

How do firms finance investment increases? The estimates come from the instrumental variable regression:

$$\text{Outcome}_t(n) = \eta_t + \beta r_{t-1}(n) + \tilde{\gamma}' \tilde{X}_{t-1}(n) + \epsilon_t(n),$$

which instruments returns (r) with the payout-induced trading variable derived in the paper:

$$PIT_t(n) = \frac{\sum_{i=1}^{I} \widetilde{PayoutFlow}_{i,t} \times Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}$$

where $\widetilde{PayoutFlow}_{i,t}(n) = \sum_{m \neq n} Payout_t(m) \times Shares_{i,t-1}(m)$. Outcomes are the change in total liabilities in columns (1) to (3) and the change in common equity in columns (4) to (6). The regression includes as controls risk factor characteristics (beta, log market equity, profitability, investment, and momentum), dividend to book equity, payout ratios for all payout types (cash dividends, stock dividends, cash M&A, stock M&A, and share repurchases), except that we drop Tobin's Q from the equity issuance regression because it contains book equity which is part of the dependent variable. This table is analogous to table 6, except that we control for time×industry fixed effects instead of time fixed effects. The frequency of all regressions is annual and the sample span 1980 to 2017. We report standard errors clustered by time in parentheses and Kleibergen and Paap (2006) F-statistics in the table footer. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A5: Capital Structure Rebalancing Effect — Instrumenting with Expected PIT

	Debt			Equity		
	(1) OLS	(2) FS	(3) 2SLS	(4) OLS	(5) FS	(6) 2SLS
Return	0.048***		0.274**	−0.045***		0.010
	(0.012)		(0.115)	(0.016)		(0.105)
PIT(Expected)		0.012***			0.011***	
		(0.002)			(0.002)	
F-statistic		34.72			31.02	
Observations	98,574	98,574	98,574	91,953	91,953	91,953

How do firms finance investment increases? The estimates come from the instrumental variable regression:

$$\text{Outcome}_t(n) = \eta_t + \beta r_{t-1}(n) + \tilde{\gamma}' \tilde{X}_{t-1}(n) + \epsilon_t(n),$$

which instruments returns (r) with the payout-induced trading variable derived in the paper:

$$PIT_t(n) = \frac{\sum_{i=1}^{I} \widetilde{PayoutFlow}_{i,t} \times Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}$$

where $\widetilde{PayoutFlow}_{i,t}(n) = \sum_{m \neq n} Payout_t(m) \times Shares_{i,t-1}(m)$. Outcomes are the change in total liabilities in columns (1) to (3) and the change in common equity in columns (4) to (6). The regression includes as controls risk factor characteristics (beta, log market equity, profitability, investment, and momentum), dividend to book equity, payout ratios for all payout types (cash dividends, stock dividends, cash M&A, stock M&A, and share repurchases), except that we drop Tobin's Q from the equity issuance regression because it contains book equity which is part of the dependent variable. This table is analogous to table A4, except that the instrumental variable uses *expected* dividend payouts, which we construct following a simple predictive rule. At the end of each year, we predict split-adjusted dividends per share for each quarter of the next year. For dividends announced in the previous year, actual and expected dividends are the same. If no dividend is announced yet, we exploit that managers smooth split-adjusted dividends per share. The rule for prediction assumes that the last dividend behavior continues in the next year, for each dividend frequency. The frequency of all regressions is annual and the sample span 1980 to 2017. We report standard errors clustered by time in parentheses and Kleibergen and Paap (2006) F-statistics in the table footer. ***, **, and * denote significance at the 10%, 5%, and 1% levels.