Payout-Induced Trading, Asset Demand Elasticities, and Market Feedback Effects*

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Abstract

This paper uses the reinvestment of corporate payouts by financial institutions as a nonfundamental shock to asset prices to estimate the slope of the demand curve for stocks and the real effects of stock returns on corporate financing and investment. Exploiting the separation of announcement and payment at the daily frequency, I find 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. Using dividend-induced trading as a natural experiment for stock returns, I estimate an asset demand elasticity of 1.25 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.

Keywords: Institutional Investors; Price Pressure; Market Feedback Effects; Payout; Dividends; Share Repurchases; Mergers and Acquisitions

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 I examine in Schmickler (2020).

This paper uses an alternative asset demand shock that addresses these drawbacks, allowing me to shine new light on two important questions in empirical finance: what is the slope of the demand curve for stocks? And how does a nonfundamental increase in stock prices affect corporate financing and investment?

I investigate these questions using the mechanism described by Kvamvold and Lindset (2018) and Chen (2020) and based on financial institutions' reinvestment of firms' cash payouts. On average, each year, 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. How do shareholders redeploy these payouts? 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 reinvest payouts, because cash is unproductive. However, since cash payouts do not increase the supply of shares, shareholders cannot reinvest into the payout stock itself on average. Instead, to avoid falling behind their benchmarks, most institutional investors reinvest payouts into their existing portfolios. This *payout-induced trading* is a positive demand shock. Hence, when firms pay out cash, payout-induced trading drives up prices of other stocks held in the same portfolios of financial institutions.

The 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). I construct this shock using US equity portfolio holdings and payout data from 1980 to 2017.

What is the slope of the demand curve for stocks? I address this question by examining the price impact of payout-induced trading. The key challenge in identifying this price pressure effect is that payouts reveal fundamental information which also affects 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. I solve this identification problem by exploiting that demand pressure effects differ from news effects in terms of timing; news changes asset prices around announcements, while demand pressure changes asset prices after the payment, when financial institutions receive cash and reinvest. 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. Further, the absence of pre-payment date trends and the long lag between the announcement and the payment, e.g. an average of 45 days for dividends, ensure that payment date effects are driven by price pressure, not post-announcement drifts.

I test whether payouts generate news and/or price pressure effects in a systematic analysis of returns around payout events. For each type of payout, I show daily abnormal returns of the firm itself, 3-digit SIC code industry peers, and connected stocks around announcements and payment dates. I start with ordinary cash dividends, because they allow for the cleanest identification. Dividend announcements reveal fundamental information. But once announced, dividends are deterministic. Dividends are then paid with a typical delay of one to three months and the payment reveals no fundamental information. Hence, the gap between the announcement and payment dates separates news from demand pressure effects.

I 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, I test for spillovers on industry peers to detect whether dividends generate news spillover effects. I 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. Finally, connected firms do not experience abnormal returns around announcement dates, but they do exhibit persistent, positive abnormal returns after the payment date, with no pre-payment date trend. Specifically, the estimated price impact of dividend-induced trading identifies a demand elasticity of 1.25, consistent with the evidence from Russell 2000 additions and deletions in Chang, Hong, and Liskovich (2015).

While dividends provide the cleanest setting to separate news from price pressure, I also examine other types of payouts. Like dividends, M&A payouts drive up connected 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, M&A-induced trading is not a candidate instrument for returns. Next, for share repurchases, high-frequency payment data are unavailable. Hence, I can only examine announcement returns, not payment date returns. I do not find statistically significant repurchase announcement spillover effects on connected stocks, likely because firms do no begin to repurchase shares immediately after announcements. As I cannot separate news from price pressure effects of share repurchases, share repurchase-induced trading is not a candidate instrument for returns either.

I also investigate heterogeneity across financial institutions and find the results are strongest for mutual funds, followed by investment advisors (e.g. asset managers), consistent with the notion that institutions engage in payout-induced trading because they are benchmarked.

How does a nonfundamental increase in stock prices affect corporate financing and investment? I address this question using dividend-induced trading as an instrument for stock returns. Dividends generate variation in returns of connected stocks that is plausibly exogenous to fundamentals, as evidenced by the high-frequency stock price reactions to payments, but not announcements. Hence, dividend-induced trading is a valid instrument for returns. As the pricing effects are persistent, this allows me to estimate the causal effect of an increase in stock prices on corporate outcomes, i.e. market feedback effects (surveyed in Bond, Edmans, and Goldstein, 2012; Baker and Wurgler, 2013).

Payout-induced trading drives up stock prices. However, this effect occurs in the secondary market, without capital flowing to firms. So why would secondary market prices impact real outcomes? While the literature documents several channels, I find a releveraging market feedback effect on investment. Using dividend-induced trading as an instrumental variable for stock returns, I find firms respond to an exogenous stock price increase by issuing debt, i.e. moving back towards their target leverage ratio, and using the funds to increase investment. I 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. Further, I estimate that a nonfundamental 1% return increases investment by almost 1% relative to its median.

As an additional line of defense against news as a confounding channel, I 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.

From a policy maker's perspective, this mechanism is important because it informs the recurring 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, my paper finds a new spillover effect of payouts: capital investment occurs despite payouts – it just happens at other firms.

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 I 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: payout-induced trading is determined before returns, does not change liquidity, is a strong instrument for returns, and can be used to investigate heterogeneous and/or high-frequency 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 my paper examines returns of connected stocks around announcements and payments to separate news from demand pressure effects. In particular, I find that dividends generate price pressure but no news spillover effects then justifying the use of dividend-induced trading as an instrument for returns allowing me to identify the slope of the demand curve for stocks as well as to identify market feedback 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, allowing me to study the real effects of 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.

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. The empirical tests also require additional institution and firm characteristics.

[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).

2.1 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). I 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 my sample, which spans 1980 to 2017.

In addition, for mutual funds, detailed institution-level data are available. I 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, I infer AuM and returns from portfolio holdings. I measure AuM as the total market value of all observed portfolio positions, and portfolio returns as the portfolio weight-weighted mean of stock returns, assuming that institutions only trade at the end of each quarter. I use AuM and portfolio returns to compute investment flows as a control variable. I 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).

2.2 Firm 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, I 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

[2] I 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.

must infer share repurchases for the pre-2004 period. I 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 me to construct dollar payout flows to each institution

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

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. I 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, I use the market value of the securities paid. I use stock payouts in placebo tests. For share repurchase flows, I do not observe which institutions sell to repurchase programs. Hence, I assume all institutions sell a fraction equal to the fraction of shares repurchased by the firm. Finally, I 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

Stock data are from CRSP, accounting data are from Compustat North America Fundamentals Annual and Quarterly. The main dependent variable is log returns. 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. I 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 I 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 I lag accounting data by 6 months. To construct market beta, I take the 1-month T-bill rate and the market return from Kenneth French's website. I calculate market betas using 60-month rolling window regressions.

[3] I 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. I 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.

When testing for market feedback effects on investment, I follow Dessaint et al. (2018). This means I 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). I also test whether the investment response is financed by debt or equity. I measure debt as total liabilities and equity as common equity. In all exercises, I winsorize characteristics cross-sectionally at the 1 and 99% level, as for example in Green, Hand, and Zhang (2017) and Dessaint et al. (2018). I 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 Firm Payouts

Figure 3 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 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. 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, investments advisors, banks, pension funds, insurance companies, and unclassified institutions hold 25, 21, 12, 3, 2, and 2% of all equities, respectively. The remaining share, about one third, is held by households and foreign investors.

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 type. 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 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 my 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 estimates the price impact of payout-induced trading. The analysis is at the stock level, allowing me to investigate the effects at the daily frequency.

3.1 The Demand Shock

Institution i holds $Shares_{i,t}(n)$ split-adjusted shares of stock n at time t. I denote relative payout flows as $payoutflow_{i,t} = PayoutFlow_{i,t}/A_{i,t-1}$. Chen (2020) shows that institutions reinvest firm payouts into their existing portfolios. In addition, following the reasoning from the literature that constructs asset demand shocks from mutual fund flows (e.g. Edmans, Goldstein, and Jiang, 2012), hypothetical reinvestment in proportion to existing portfolio weights is likely exogenous to firm fundamentals, while actual trades may well be driven by firm fundamentals. If institutions reinvest in proportion to portfolio weights, they buy $payoutflow_{i,t}Shares_{i,t-1}(n)$ shares. This behavior 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

$$PIT_t(n) = \frac{\sum_{i=1}^{I} payoutflow_{i,t}Shares_{i,t-1}(n)}{\sum_{i=1}^{I} Shares_{i,t-1}(n)}. \tag{2}$$

I construct PIT separately for different payout types. This gives cash dividend-induced trading, stock dividend-induced trading, cash M&A-induced trading, stock M&A-induced trading, and 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 the standard mutual fund flow-induced trading instrument as in Edmans, Goldstein, and Jiang (2012), though the exact construction here also takes the Wardlaw (2020) critique into account.

Two sources of variation drive the payout-induced trading shock: first, naturally, payouts; second, investor heterogeneity. 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 each cross-section. In this world, every stock would get the same cross-sectional payout-induced trading shock, making it impossible to identify its price impact in the cross-section.

Figure 4 illustrates the deviation from this world using histograms of 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. With homogeneous investors, all values would be equal to one. I winsorize the ratio at 5. Otherwise, the graph is dominated by whitespace. The left histogram illustrates the cash dividend shock. The distribution is approximately exponential, with most values falling between 0 and 2 and a small mass at 5. This means that on one hand, many stocks are close to untreated, while on the other hand, 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. More stocks are close to untreated, and more stocks receive more than five times the shock they would receive in the homogeneous investor world. This is because on an average day, 30 firms pay a dividend, but zero or one firm is acquired. Overall, Figure 4 shows that investor heterogeneity generates large cross-sectional dispersion in payout-induced trading.

3.2 Identification Strategy

I want to identify the price impact of payout-induced trading. The key challenge 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.

Figure 5 illustrates the setting I exploit. The left plot shows the distribution of the number of days between the dividend payment and announcement date. This gap separates the news from the demand pressure effect. 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 company 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 about one billion dollars. On a few dozen days, companies pay out about five billion dollars. The biggest daily shocks are about ten billion dollars.

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

$$r_t(n) = \alpha_t + \sum_{l=\underline{L}}^{\bar{L}} \gamma_l Z_{t-l}(n) + \beta' X_t(n) + \epsilon_t(n), \tag{3}$$

of stock returns on a vector of shocks Z, which contains the payout to price ratio, the industry payout ratio, and payout-induced trading. I 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. I 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. I 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. I 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 I 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). I 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

I test whether payouts generate news and/or price pressure effects in a systematic analysis of returns around payout events. For each type of payout, I report daily abnormal returns of the firm itself, peer stocks, and connected stocks around announcement and around payment dates. I provide a compact summary of the findings in Figure 2 and go into full detail here.

3.3.1 Ordinary Dividends

I start with ordinary cash dividends, because they allow for the cleanest identification. Figure 6 investigates return patterns around cash dividend events at the daily frequency. On the left, I show returns around the announcement date, which capture the effects of news. On the right, I 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 connected stocks. The plots report estimates of equation 3. I 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, I exploit this behavior in section 4.3 where I 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, I construct hypothetical dividend-induced trading if dividends were paid on the announcement date. I 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 connected stocks. This is expected, as dividends do not even generate spillover effects on industry peers.

Yet, panel (f) documents that connected stocks 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. I 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, I find dividends generate no news spillover effects but do produce large price pressure spillover effects on connected stocks.

I 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, I 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, 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 is not ideal for identification, it does provide additional evidence.

Figure 7 repeats the analysis in Figure 6 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, I 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. Connected 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. The M&A results are less reliable quantitatively but still confirm my finding qualitatively. Overall, M&A deals generate news and price pressure spillover effects. Therefore, while they provide additional evidence for payout-induced trading price pressure effects, they are not ideal to construct instrumental variables for stock returns.

3.3.3 Share Repurchases

For completeness, Figure 8 repeats the analysis in Figure 6 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 me 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 institutions engage in payout-induced trading is likely that 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, I construct payout-induced trading using only one institution type at a time. I then estimate equation 3 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 9 reports the same analysis as panel (f) of Figure 6 but by institution type. Broadly, I 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). This is likely because mutual funds and investment advisors are large and benchmarked, while other institution types are small and/or not 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 fund in particular, may trade more frequently, meaning their quarterly portfolio snapshots mismeasure payout recipients more.

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.

4 Market Feedback Effects

This section examines how payout-induced trading, by driving up stock prices, affects corporate financing and investment. I 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.[4] The only market feedback effect paper investigating the impact of increases in asset prices that I am 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.

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. As an alternative to outflow-induced trading, payout-induced trading provides an experiment for asset price increases and allows me to

[4] 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).

estimate the other side of the market feedback effect. In addition, payout-induced trading is a new experiment for changes in asset prices. This is valuable, because recent work casts doubt on the mechanical construction of the mutual fund flow-induced instrument (Wardlaw, 2020) as well as on the direction of causality (Schmickler, 2020).

4.1 Identification Strategy

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

$$\frac{I_t(n)}{K_{t-1}(n)} = \alpha_t + \alpha(n) + \beta Q_{t-1}(n) + \gamma' X_{t-1}(n) + \xi_t(n), \tag{4}$$

a regression of the investment rate 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 constant returns to scale and perfect competition (Hayashi, 1982).

Payout-induced trading is an instrument for returns, approximately first differences of log Tobin's Q. Hence, I estimate the equation in first differences

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

and instrument returns with payout-induced trading. The control variables are the risk factor characteristics as described in the data section, 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. I also estimate alternative versions of this equation. I show estimates excluding control variables as a simple baseline and estimates replacing time fixed effects with time x industry fixed effects which mitigates the potential concern that payout-induced trading captures news instead of price pressure spillover effects.

I illustrate this identification strategy in Figure 1. Pfizer pays a dividend; Pfizer is held in the same portfolio as Lyft, but not as Uber. Hence, Lyft experiences payout-induced trading, driving up its stock price relative to Uber. This section tests whether Lyft increases its investment rate over the following year relative to Uber and attributes this investment response to the price increase. The threat to identification is that since they are held in the same portfolio, Lyft may be more similar to Pfizer than Uber. Accordingly, Pfizer and Lyft may be hit by common shocks that do not hit Uber. Further, market feedback effect regressions require a low-frequency instrument because managers make corporate decisions at low frequency. Therefore, I cannot use the same, daily frequency empirical strategy that identified the payout-induced trading price pressure effect. However, section 3 shows that dividends only generate spillover effects on asset prices of connected stocks after the payment date, not after the announcement date. This suggests that payout-induced trading only affects asset prices of connected stocks via a demand pressure channel and not via a news channel. Therefore, it is a plausibly exogenous shock to asset prices of connected stocks.

As discussed in section 3, PIT 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. At low frequency, however, data are not abundant. Therefore, while I examine the subsample that excludes payout firms in a robustness check, I begin with an alternative approach to address this concern. I define $PayoutFlowEx_{i,t}(n) = \sum_{m \neq n} Payout_t(m) Shares_{i,t-1}(m)$ and substitute $PayoutFlow$ with $PayoutFlowEx$ in the definition of PIT. Now PIT is trading induced by other firms' payouts and is thus free of the known self-effect. In addition, I control for the payout ratios corresponding to all payout types.

4.2 The Investment Response

Table 2 reports estimates of equation 5. I show the first and second stage of 2SLS estimates of equation 5, as well as the OLS estimate as a baseline. In all specifications, I cluster standard errors by time because returns are highly correlated in the cross-section. Appendix Table A3 demonstrates that clustering by time is more conservative than clustering by firm and as conservative as double clustering by time and firm. The first column reports the results of the OLS regression. There is a strong positive relationship between returns and 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 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 dividend-induced trading is a strong instrument, with a Kleibergen and Paap (2006) F-statistic of 50. In the second stage, I estimate a highly statistically significant coefficient of 0.1. This estimate is higher than the OLS coefficient, consistent with the common finding that instrumenting returns addresses the mismeasurement problem and mitigates the attenuation bias (e.g. Erickson and Whited, 2000; Almeida, Campello, and Galvao, 2010; Bakke and Whited, 2010).

In columns 4 and 5, I add control variables. Again, dividend-induced trading is a strong instrument with an F-statistic of over 50. In the second stage, the coefficient remains statistically significant and increases from 0.1 to 0.17. In the last two columns, I include time x 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 the inclusion of time x industry fixed effects. This is not the case. The instrument remains strong with an F-statistic of 30, and the coefficient in the second stage decreases slightly to 0.15. The investment-return elasticity estimates range from 0.1 to 0.17, but their difference is not statistically significant. Overall, a 1% return translates into a 0.1 to 0.17% increase in investment, up to 1% of the median annual investment rate. Returns translate into investment almost one-for-one.

4.3 Expected payout-induced trading

As an additional line of defense against news as a confounding channel, I repeat the exercise using only the expected component of dividends because, by definition, only surprise dividends, not expected dividends, convey news. At the end of each year, I predict split-adjusted dividends per share for each quarter of the next year. I 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 simple. For dividends announced in the previous year, actual and expected dividends are the same. If no dividend is announced yet, I exploit that managers smooth split-adjusted dividends per share. I predict that the last dividend behavior continues in the next year, for each dividend frequency. This simple dividend prediction achieves an R^2 of 93%. I illustrate this in Appendix Figure A1 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, which correspond to the small bars at -1 and 1. In addition, it misses dividend increases, which correspond to the small bars between -1 and 0. Finally, dividend decreases are too infrequent to be visible.

Table 3 reports estimates of equation 5 using expected dividend-induced trading as the instrument for returns. The results are close to identical to the estimates in table 2. The instrument is slightly weaker. Looking at the strictest specification in column 5, the F-statistic is 26 instead of 32 and the estimated market feedback effects are slightly larger, but statistically indistinguishable. Again, considering the strictest specification in column 6, the coefficient on returns is 0.17 instead of 0.15. This shows that the results are not driven by the news component of dividend-induced trading and therefore suggests that the increase in investment is driven by price pressure.

4.4 Capital Structure Rebalancing Effect

How do firms finance the investment increases? Taking a step back, how do stock prices affect corporate financing? 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. However, these studies examine how firms' financing decisions respond to changes in stock prices, whereas I ask how firms react to *exogenous* changes in stock prices. The difference may be large, because firms experience great returns when their investment opportunities improve and vice versa. Accordingly, firms may issue equity to raise capital for new investment opportunities or repurchase stock due to a lack of investment opportunities. In contrast, I explore how firms react to returns that are unrelated to fundamentals.

I test this by estimating equation 5 but substituting the investment rate for the debt or equity issuance rate. I show the results in Table 4 which reports results corresponding to columns 3 and 4 in Table 2, i.e. the specification including controls and time fixed effects. I report results for alternative specifications in the Appendix. I 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, I 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.

4.5 Robustness

The Appendix contains further robustness results. In Table A2, I find the same results qualitatively when excluding payout firms. In fact, the coefficients in the first and second stage increase. I also show that the results are robust to omitting within-industry flows. I 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 bad description of investor behavior, it allows me to test whether within-industry flows drive the results, which would be inconsistent with the price pressure channel. I find that this is not the case. The estimated coefficients are virtually unchanged. Next, Appendix Table A3 shows that clustering by time 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 x industry fixed effects. Appendix Table A5 also includes time x 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, I 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, I 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. I 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. Further, I estimate that a nonfundamental 1% return increases investment by almost 1% relative to its median. This informs the recurring policy debate on whether to restrict payouts so firms invest instead. My 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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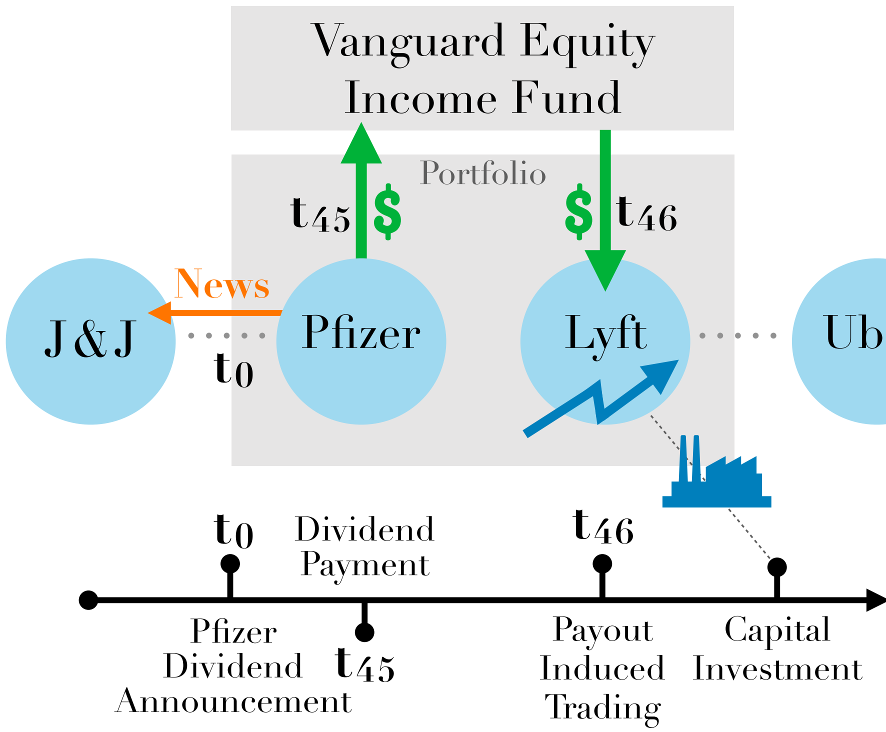
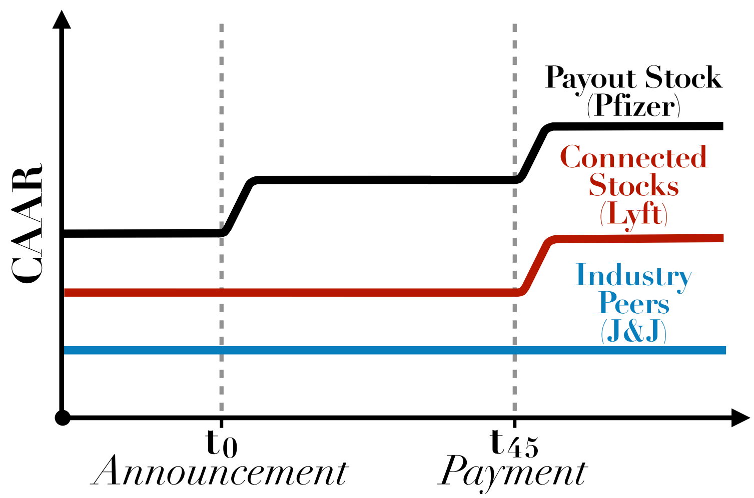
Figure 1: Illustration of Payout-Induced Trading Mechanism



This figure illustrates the payout-induced trading effect. 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. I 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.

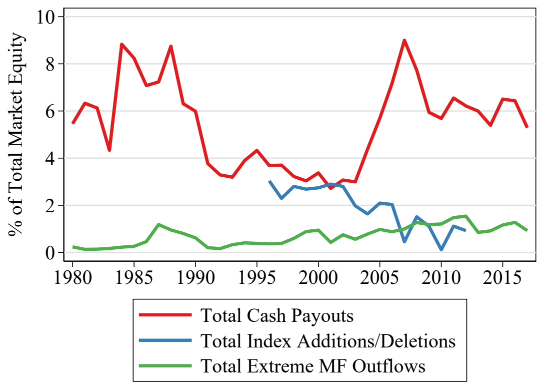
Figure 2: Illustration of Identification Strategy



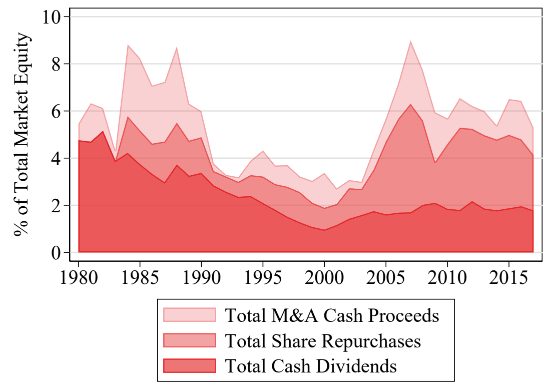
(a) Returns around Dividend Events

This figure illustrates the main identification strategy. Demand pressure effects differ from news effects in terms of timing; news change asset prices at announcement, while demand pressure changes asset prices at payment, when financial institutions receive cash and reinvest. I examine returns of the firm itself, peer stocks, and connected stocks around announcement and payment dates. As is widely known, the payout firm itself always experiences positive abnormal returns after dividend announcements and payments (Ogden, 1994; Hartzmark and Solomon, 2013). Next, I 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. Finally, connected firms do not experience abnormal returns around announcement dates, but they do exhibit persistent, positive abnormal returns after the payment date, suggesting that dividend-induced trading is plausibly exogenous to fundamentals and motivating its use as an instrument for stock returns.

Figure 3: Total Cash Payouts vs. Alternative Sources of Asset Demand Shocks



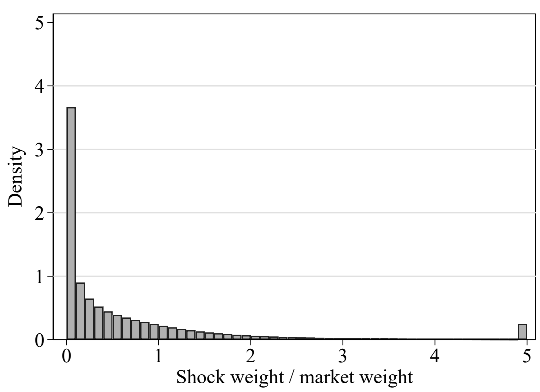
(a) Sources of Asset Demand Shocks



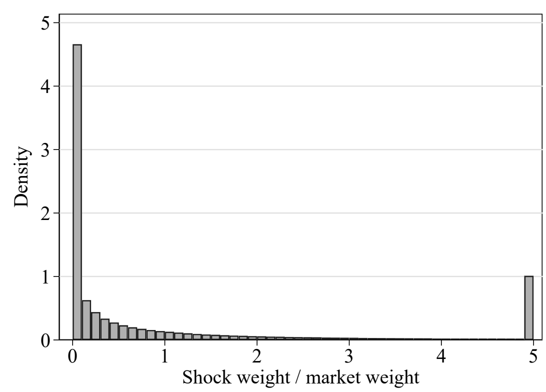
(b) Payout Type Breakup

This figure shows summary statistics. 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. Total extreme mutual fund 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 4: The Payout-Induced Trading Shock



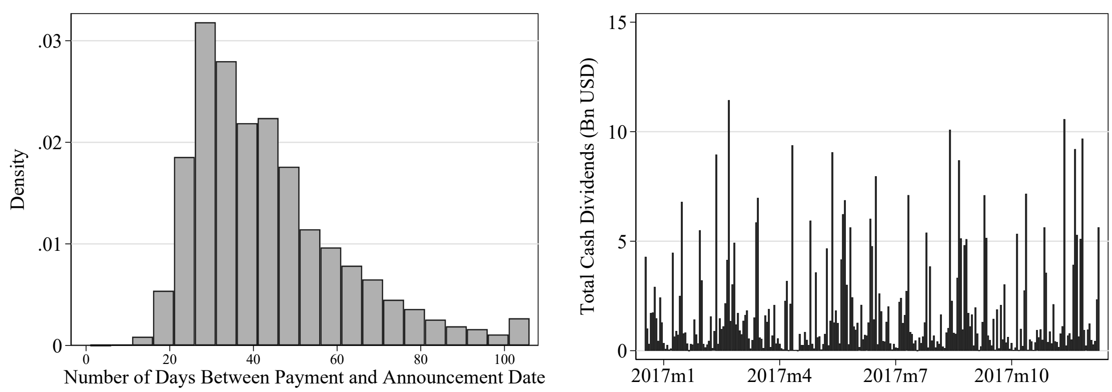
(a) Dividend-Induced Trading



(b) M&A-Induced Trading

The two plots 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 I 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.

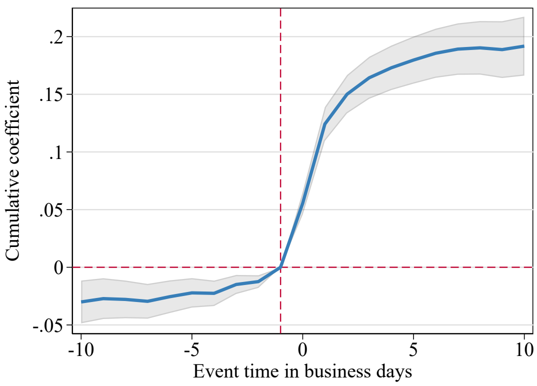
Figure 5: Ordinary Dividends



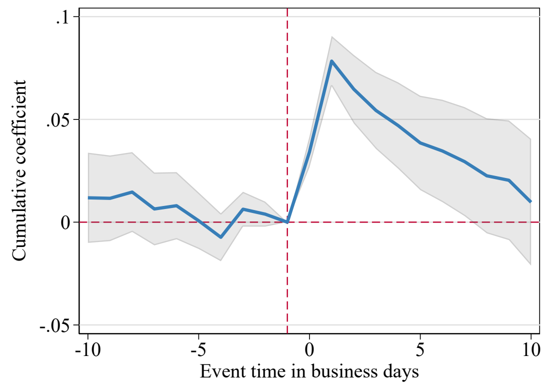
(a) Payment - Announcement Date Lag (b) Daily Cash Dividends in 2017

This figure illustrates cash dividend payouts. The first plot shows the distribution of the number of days between the dividend payment and announcement date. The number of days can be very large, so I winsorize it at the 99th percentile for this figure. I 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 (b) shows total cash dividends for each day in 2017.

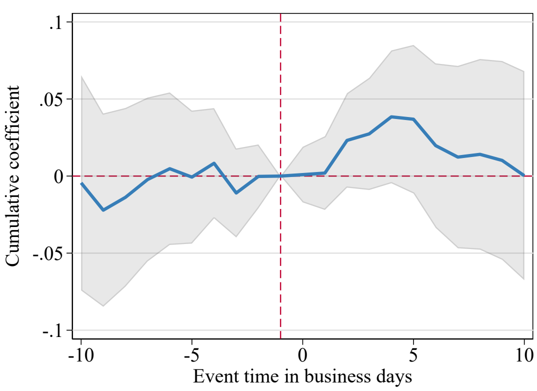
Figure 6: Abnormal Returns Around Dividend Announcement and Payment Dates



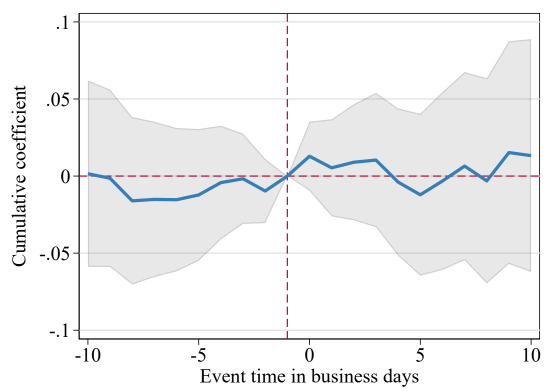
(a) Announcement, payout stock



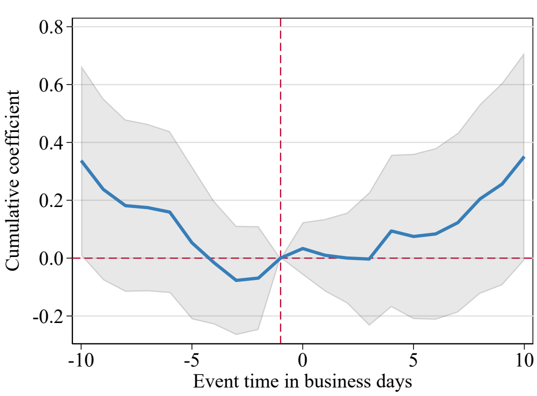
(b) Payment, payout stock



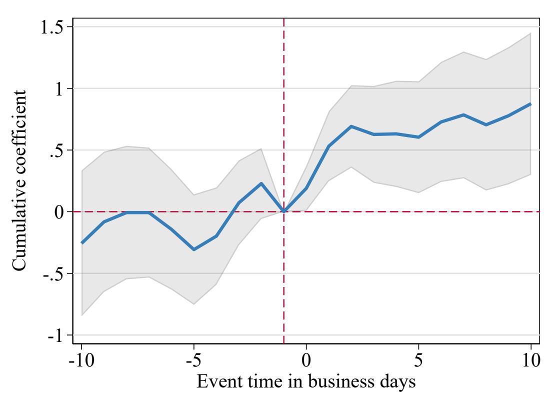
(c) Announcement, industry peers



(d) Payment, industry peers



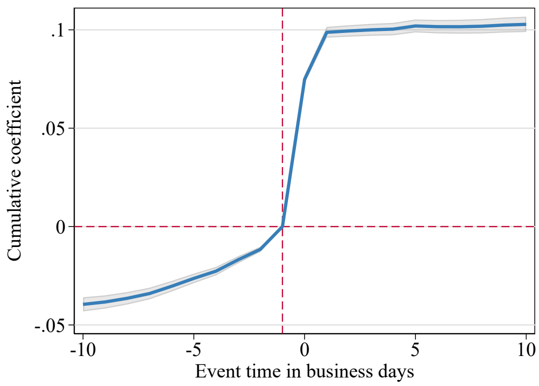
(e) Announcement, connected stocks



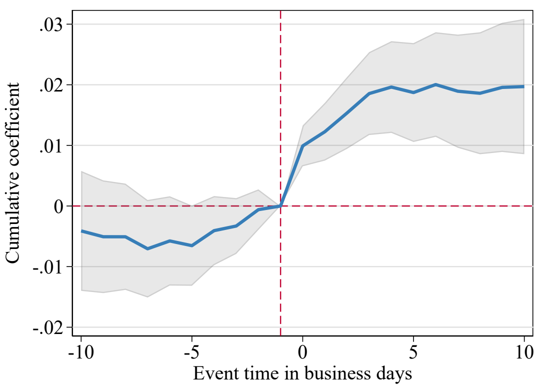
(f) Payment, connected stocks

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 equation 3, with standard errors clustered by time. I show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), I 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. The controls are described in the data section. The sample is daily from 1980 to 2017.

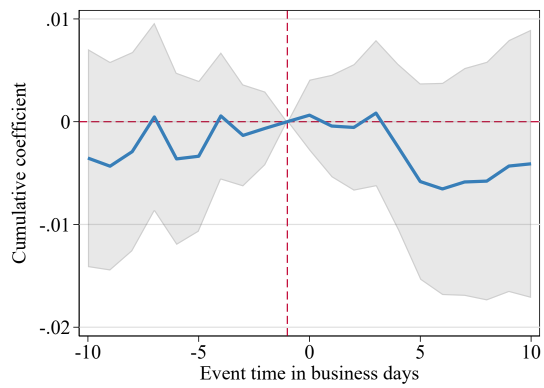
Figure 7: Abnormal Returns Around M&A Announcement and Payment Dates



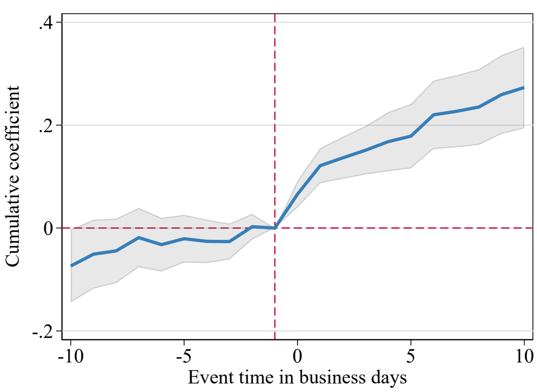
(a) Announcement, payout stock



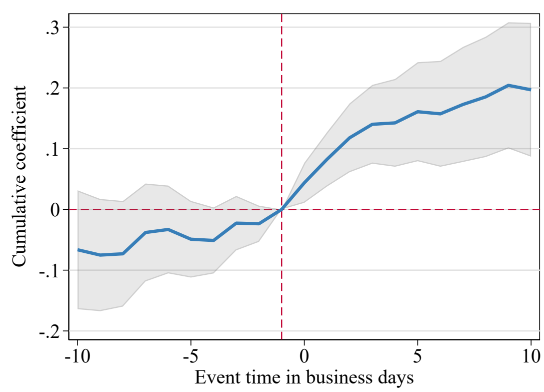
(b) Announcement, industry peers



(c) Payment, industry peers



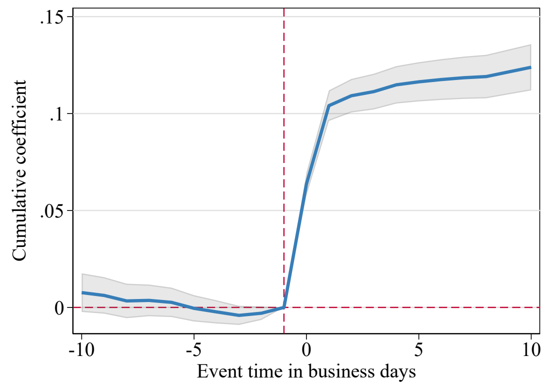
(d) Announcement, connected stocks



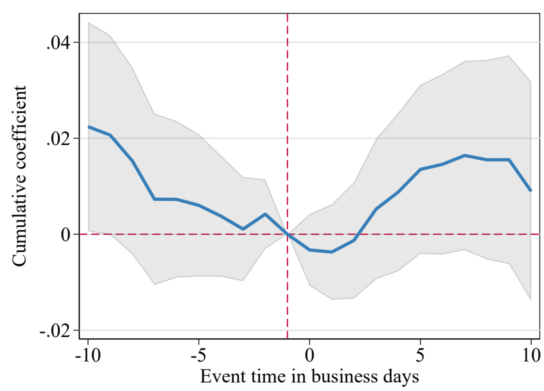
(e) Payment, connected stocks

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 equation 3, with standard errors clustered by time. I show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), I 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. The controls are described in the data section. The sample is daily from 1980 to 2017.

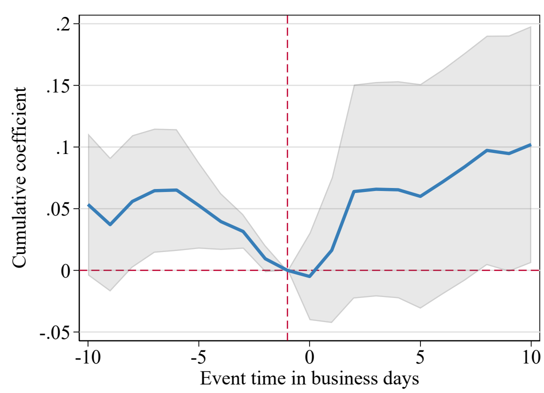
Figure 8: Abnormal Returns Around Share Repurchase Announcement Dates



(a) Announcement, payout stock



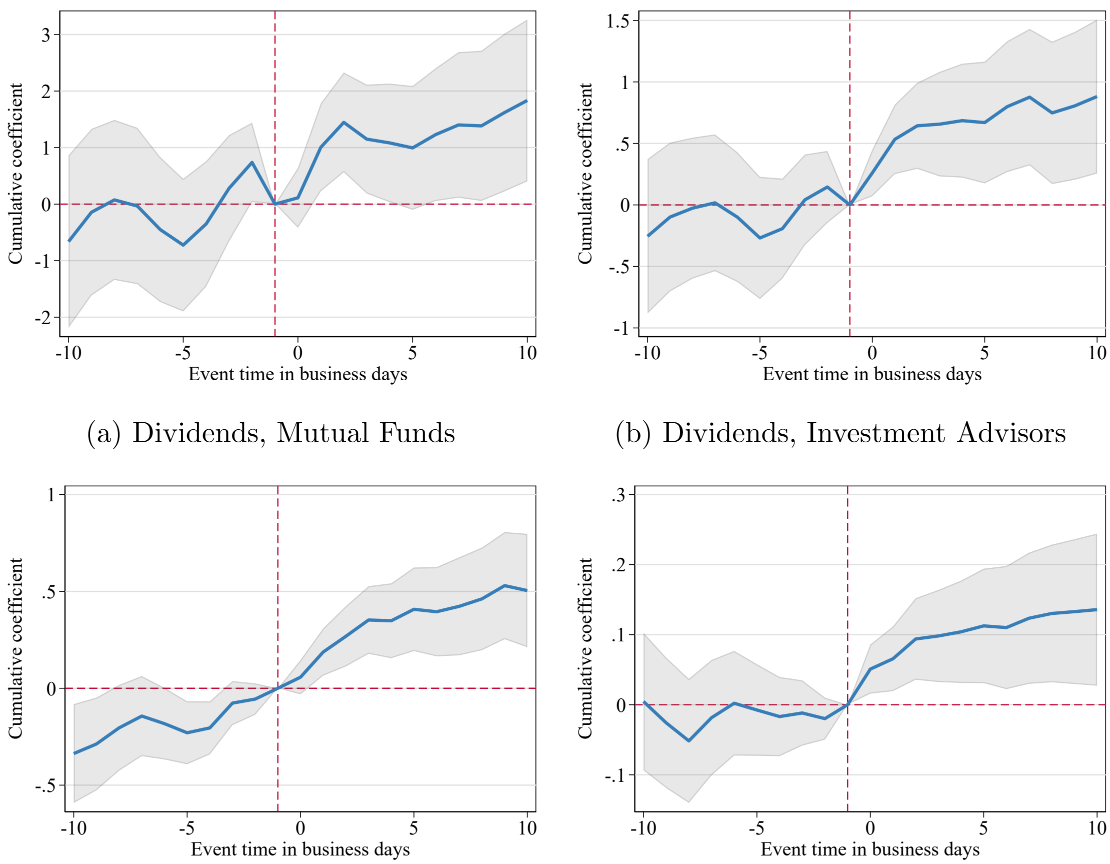
(b) Announcement, industry peers



(c) Announcement, connected stocks

The plots show percent cumulative abnormal returns in response to a 1% shock around share repurchase announcement 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 not comparable across plots, because different plots report coefficients on different variables. The plots are based on estimates of equation 3, with standard errors clustered by time. I show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), I 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. The controls are described in the data section. The sample is daily from 1980 to 2017.

Figure 9: 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

The plots show percent cumulative abnormal returns in response to a 1% payout-induced trading shock, for different institutions and payout types. The top row reports results for cash dividend-induced trading, the bottom row for cash M&A-induced trading. The left column reports results for mutual funds, the right column for investment advisors (e.g. hedge funds). The plots are based on estimates of equation 3, with standard errors clustered by time. I show cumulative coefficients and 95% confidence intervals from a Wald test. Following Schmidheiny and Siegloch (2020), I sum the lead and lag coefficients separately. The underlying regressions use the sample that excludes payout firms to isolate spillover effects. The controls are described in the data section. The sample is daily from 1980 to 2017.

Table 1: Summary Statistics for Financial Institutions by Type

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

This table summarizes financial institution information by institution type and decade, reporting time-series means by institution type and within the given period. I 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 %. The investment advisor type includes e.g. hedge funds. The "other" type includes e.g. endowments. The sample is 1980 to 2017.

Table 2: The Market Feedback Effect on Investment

	(1) OLS	(2) FS	(3) 2SLS	(4) FS	(5) 2SLS	(6) FS	(7) 2SLS
return	0.0826***		0.100***		0.171***		0.151***
	(0.00605)		(0.0257)		(0.0409)		(0.0407)
DIT		0.0210***		0.0140***		0.0118***	
		(0.00288)		(0.00187)		(0.00209)	
Controls				✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓		
Time x ind. FE						✓	✓
F-statistic		53.07		56.06		31.75	
N	112444	112444	112444	112076	112076	109725	109725

This table shows estimates of equation 5, the firm-level regression of the change in the investment rate on returns and controls and fixed effects depending on the specification. The control variables are stock characteristics as described in the data section. The first column shows the results of an OLS regression. The next columns show first and second stages of different versions of 2SLS regressions. The instrument is normalized cash dividend-induced trading (DIT). The frequency is annual. I 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 3: The Market Feedback Effect on Investment – Instrument with *Expected* DIT

	(1) FS	(2) 2SLS	(3) FS	(4) 2SLS	(5) FS	(6) 2SLS
return		0.107***		0.192***		0.172***
		(0.0269)		(0.0457)		(0.0452)
DIT	0.0186***		0.0119***		0.00994***	
	(0.00271)		(0.00175)		(0.00193)	
Controls			✓	✓	✓	✓
Time FE	✓	✓	✓	✓		
Time x ind. FE					✓	✓
F-statistic	46.73		46.21		26.53	
N	112444	112444	112076	112076	109725	109725

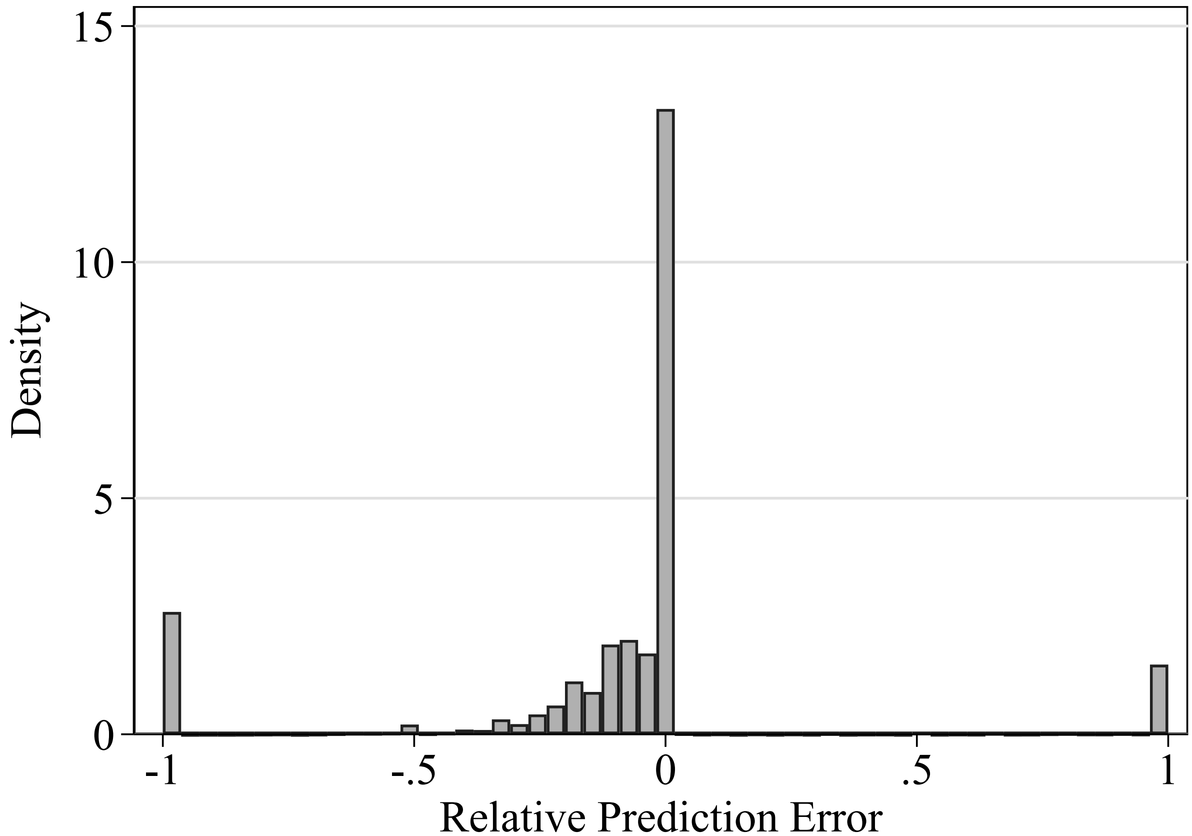
This table is analogous to table 2, except that the instrumental variable is *expected* dividend-induced trading. It shows estimates of equation 5, the firm-level regression of the change in the investment rate on returns and controls and fixed effects depending on the specification. The control variables are stock characteristics as described in the data section. The columns show first and second stages of different versions of 2SLS regressions. The instrument is normalized for ease of comparison. The frequency is annual. I 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 4: Capital Structure Rebalancing Effect

	Debt			Equity		
	(1) OLS	(2) FS	(3) 2SLS	(4) OLS	(5) FS	(6) 2SLS
return	0.0487***		0.282**	-0.0401**		0.0625
	(0.0123)		(0.107)	(0.0170)		(0.0831)
DIT		0.0140***			0.0154***	
		(0.00181)			(0.00219)	
F-statistic		59.78			49.11	
N	100845	100845	100845	94264	94264	94264

This table is analogous to table 2, though with different dependent variables. I replace investment (CAPX, the change in property plant and equipment) with the change in total liabilities in columns 1 to 3 and the change in common equity in columns 4 to 6. The table shows estimates of equation 5, the firm-level regression of the respective dependent variable on returns, controls, and time fixed effects. The control variables are stock characteristics as described in the data section, except that I drop Tobin's Q from the equity issuance regression because it contains book equity which is part of the dependent variable. The columns show first and second stages of different versions of 2SLS regressions. The instrument is normalized cash dividend-induced trading (DIT). The frequency is annual. I 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: Relative Dividend Prediction Error



This histogram shows the distribution of the relative dividend prediction error, i.e. the prediction error scaled by the prediction target. -1 and 1 are dividend initiations and dividend discontinuations, respectively. At the end of each year, I predict split-adjusted dividends per share for each quarter of the next year. This is because the market feedback effect regressions are at the annual frequency, but the shock is constructed using quarterly frequency portfolio holdings data. For dividends announced in the previous year, actual and expected dividends are the same. If no dividend is announced yet, I exploit that managers smooth split-adjusted dividends per share. I predict that the last dividend behavior continues in the next year, for each dividend frequency. The sample is the daily stock level panel from 1980 to 2017.

Table A1: Firms Subject to Dividend-Induced Trading are Similar to Dividend-Paying Firms

	DIT	Div/P
Market beta	-0.104**	-0.290***
	(0.0444)	(0.0335)
Log market equity	0.990***	0.242***
	(0.0768)	(0.0230)
Log Tobin's Q	-0.649***	-0.259***
	(0.0525)	(0.0335)
Profitability	-0.673***	0.0457
	(0.240)	(0.120)
Investment	-1.570***	-0.855***
	(0.122)	(0.0763)
R-squared	0.467	0.233
N	149313	149313

This table shows estimates of regressions of dividend-induced trading and of the dividend yield on firm characteristics and time fixed effects. The frequency is annual. The sample is 1980 to 2017. I report standard errors clustered by time in parentheses. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A2: The Market Feedback Effect on Investment – Robustness Tests

	Exclude payout firms		No within-industry flows	
	(1) FS	(2) 2SLS	(3) FS	(4) 2SLS
DIT	0.0245***			
	(0.00382)			
return		0.199***		0.187***
		(0.0415)		(0.0425)
DIT (ex ind.)			0.0128***	
			(0.00171)	
F-statistic	41.18		55.94	
N	72637	72637	112076	112076

This table is analogous to table 2, except for an alternative sample or instrument. 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. The table shows estimates of equation 5, the firm-level regression of the change in the investment rate on returns and controls and time fixed effects. The control variables are stock characteristics as described in the data section. The columns show first and second stages of different versions of 2SLS regressions. The instruments are normalized for ease of comparison. The frequency is annual. I 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 A3: The Market Feedback Effect on Investment – Alternative Clustering

	Time		Firm		Firm & time	
	(1) FS	(2) 2SLS	(3) FS	(4) 2SLS	(5) FS	(6) 2SLS
DIT	0.0140***		0.0140***		0.0140***	
	(0.00187)		(0.000643)		(0.00189)	
return		0.171***		0.171***		0.171***
		(0.0409)		(0.0188)		(0.0369)
F-statistic	56.06		474.9		54.96	
N	112076	112076	112076	112076	112076	112076

This table is analogous to table 2, except for alternative assumptions about the distribution of the residual. In columns 1 and 2, I cluster standard errors by time (repeated for comparison). In columns 3 and 4, I cluster standard errors by firm. Lastly, in columns 5 and 6, I cluster standard errors by firm and time. The table shows estimates of equation 5, the firm-level regression of the change in the investment rate on returns, controls, and time fixed effects. The control variables are stock characteristics as described in the data section. The columns show first and second stages of different versions of 2SLS regressions. The instrument is normalized cash dividend-induced trading (DIT). The frequency is annual. I report standard errors in parentheses and Kleibergen and Paap (2006) F-statistics in the table footer. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A4: Capital Structure Rebalancing Effect – Time x Industry Fixed Effects

	Debt			Equity		
	(1) OLS	(2) FS	(3) 2SLS	(4) OLS	(5) FS	(6) 2SLS
return	0.0464***		0.257*	-0.0464***		0.0205
	(0.0121)		(0.134)	(0.0161)		(0.0955)
DIT		0.0118***			0.0128***	
		(0.00204)			(0.00219)	
F-statistic		33.16			34.42	
N	98574	98574	98574	91953	91953	91953

This table is analogous to table 4, except that I control for time x industry fixed effects instead of time fixed effects. The table shows estimates of equation 5, the firm-level regression of the respective dependent variable on returns, controls, and time x industry fixed effects. The control variables are stock characteristics as described in the data section, except that I drop Tobin's Q from the equity issuance regression because it contains book equity which is part of the dependent variable. The columns show first and second stages of different versions of 2SLS regressions. The instrument is normalized cash dividend-induced trading (DIT). The frequency is annual. I 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 – Instrument with *Expected* DIT

	Debt			Equity		
	(1) OLS	(2) FS	(3) 2SLS	(4) OLS	(5) FS	(6) 2SLS
return	0.0475***		0.274**	-0.0464***		0.00991
	(0.0120)		(0.115)	(0.0161)		(0.105)
DIT		0.0124***			0.0111***	
		(0.00211)			(0.00199)	
F-statistic		34.72			31.02	
N	98574	98574	98574	91953	91953	91953

This table is analogous to table A4, except that the instrumental variable is *expected* dividend-induced trading. The table shows estimates of equation 5, the firm-level regression of the respective dependent variable on returns, controls, and time x industry fixed effects. The control variables are stock characteristics as described in the data section, except that I drop Tobin's Q from the equity issuance regression because it contains book equity which is part of the dependent variable. The columns show first and second stages of different versions of 2SLS regressions. The instrument is normalized expected cash dividend-induced trading (DIT). The frequency is annual. I 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.