Payout-Induced Demand Pressure: Spillover Effects on Asset Prices and Corporate Investment[*]

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Abstract

This paper shows that when firms pay dividends, repurchase shares, or are acquired, institutional shareholders preferentially invest the cash proceeds into their existing portfolios, creating price pressure spillover effects of firm payouts on other stocks held in the same portfolios of financial institutions. This price pressure effect identifies an asset demand elasticity of 1.25. Using payout-induced trading as an instrument for stock returns, I 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;

1 Introduction

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. Households largely consume cash payouts (Baker, Nagel, and Wurgler, 2007), but equities are primarily held by financial institutions, which do not consume. Institutions also only pass-through a fraction of firms' payouts to households; for example, mutual funds make most distributions in additional shares, not cash. This means many institutions permanently retain discretion over firms' payouts. As cash is unproductive, institutions reinvest. 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 institutions reinvest payouts into their existing portfolios. I call this behavior *payout-induced trading*. Payout-induced trading is a positive demand shock to other stocks held in the same portfolios of financial institutions. What is the impact of this trillion dollar reallocation of capital on financial markets? And does it have impacts on the real economy?

I investigate these questions using US equity portfolio holdings and payout data from 1980 to 2017. I begin by showing that institutions do indeed engage in payout-induced trading. In particular, I test how institutions trade in response to payout flows, defined as total dollar payouts an institution receives scaled by assets under management. Portfolio position level regressions of the relative change in shares held on payout flows confirm that most institutional investors reinvest the majority of payouts into their existing portfolios.

Among the major institution types, mutual funds have a higher propensity to engage in payout-induced trading than investment advisors (e.g. hedge funds), which have a higher propensity than banks. This supports the notion that institutions engage in payout-induced trading because they are benchmarked; mutual funds are more benchmarked than investment advisors, which are more benchmarked than banks. Specifically, I estimate that mutual funds increase existing positions by at least 0.65% in response to a 1% payout flow. They use the remaining cash to initiate new positions or for net distributions to households.

Next, I examine the price impact of payout-induced trading. I construct the demand shock by aggregating 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).

The key challenge in identifying this price pressure effect is that payouts reveal funda-

mental information which also affects asset prices. Therefore, if firms that are held in the same portfolios ("connected stocks", Anton and Polk, 2014) are similar, abnormal returns of connected stocks may be the result of news, not price pressure. However, 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. In particular, 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 examine 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 positive abnormal returns after the payment date, with no pre-payment date trend. Specifically, a 1% demand shock triggers a persistent return of 0.8%. Interpreting the demand shock as a residual supply shock, this price reaction identifies a demand elasticity of 1.25, similar to Chang, Hong, and Liskovich (2015) who provide evidence from Russell 2000 additions and deletions. Overall, I find dividends generate no news spillover effects but do produce large price pressure spillover effects on connected stocks.

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. Next, for share repurchases, I can only examine announcement returns because high-frequency payment data are unavailable.

I also have to assume that all institutions sell to share repurchase programs in the same proportion because of a lack of data. I find repurchase announcements generate delayed spillover effects on connected stocks. The delay in connected stocks' reaction indicates price pressure effects because firms do not begin repurchases immediately after the announcement. Yet, this finding is merely suggestive due to the lack of payment data.

In addition to cash payouts, firms also make stock distributions. They occasionally pay dividends in additional shares, stock dividends. Also, M&A target stock shareholders are often paid in shares of the acquiror. Stock payouts provide placebo tests, because shareholders reinvest cash but only reinvest stock payouts if they decide to liquidate. Indeed, while stock payouts generate the same announcement date pattern as their corresponding cash payouts, they do not create payment date price pressure spillover effects on connected stocks. Overall, returns of payout stocks, peer stocks, and connected stocks, around announcement and payment dates, and for all types of payouts, show that payout-induced trading drives up stock prices.

I also investigate heterogeneity across financial institutions and across stocks. First, I find the results are strongest for mutual funds, followed by investment advisors (e.g. hedge funds), consistent with the notion that institutions engage in payout-induced trading because they are benchmarked. Second, the granularity of the payout-induced trading shock also allows me to study cross-sectional heterogeneity in asset demand elasticities. I find asset demand is more elastic for large-cap stocks, likely because sellers are more reluctant when trading costs are high. I find an asset demand elasticity of 1 for stocks below the median market value and of 1.5 for stocks above the median market value.

Lastly, I test whether, by driving up stock prices, payout-induced trading affects the real economy. While payout-induced price pressure effects occur in the secondary market, without capital flowing to firms, managers may react to changes in stock prices. Therefore, I investigate how changes in stock prices influence corporate financing and investment. Payouts, particularly 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, payout-induced trading is a valid instrument for returns. 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).

The daily frequency analysis does not find evidence for a reversal of the pricing effect. This is important because managers respond slowly, for example because of financial frictions (e.g. Fazzari et al., 1988; Kaplan and Zingales, 1997) which is why I test for market feedback effects at the annual frequency. Yet, while the pricing effects are persistent, they are not

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permanent. At the annual frequency, I find half of the price impact of payout-induced trading reverts over the following year.

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 presents a new nonfundamental shock to asset prices. 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. 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 (Wardlaw,

2020) of the fund flow-based shock. Overall, in contrast to the two prominent asset demand shocks, index additions/deletions and mutual fund flow-induced fire sales, 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.

Next, this paper contributes to the market feedback effect literature. Since Edmans, Goldstein, and Jiang (2012), the state-of-the-art identification strategy is to instrument returns with the mutual fund outflow-induced fire sale instrument 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 this question. In addition, payout-induced trading provides a new experiment.

Finally, 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 documenting a new spillover effect of all types of payouts generated by one mechanism: payout-induced trading.

Section 2 gives an overview of the data. Next, section 3 documents that institutions reinvest payouts into their existing portfolios. Then, section 4 investigates news and price pressure spillover effects of payout events. Finally, section 5 examines how stock prices affect real managerial decisions.

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

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. hedge funds), 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 pro-

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[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.

vide 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 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

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[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.

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.

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. hedge funds), 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   Trading Response to Payout Flows

This section examines whether institutions reinvest payouts into their existing portfolios. The analysis is at the quarterly frequency because this is the frequency of portfolio holdings data.

3.1   Stylized Facts

While it may seem obvious that institutions reinvest corporate payouts, many institutions are pass-through entities, meaning they have to pass through dividends and capital gains to households to avoid taxation at the institution level. So why would they reinvest corporate payouts? I begin by looking at mutual funds, for which this question can be answered with simple stylized facts. Most importantly, mutual funds reinvest firm payouts because, on average, households reinvest 83% of mutual fund distributions. This means mutual funds make most distributions in additional mutual 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. Appendix Figure 4 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 can therefore reinvest in the meantime.

I show that mutual funds reinvest firm payouts in Figure 4. Panels (a) and (b) plot the time series of aggregate mutual fund dividend and capital gains distributions. The plot does not discriminate between distributions in cash or shares. I highlight the last quarter of each year in blue. Mutual funds distribute significantly more dividends and almost all capital gains during Q4. This is due to two laws. 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 the payout to the time it must distribute. Second, since 1987, Internal Revenue Code section 4982 effectively forces funds to distribute payouts within the same calendar year. While funds held on to corporate payouts before, Internal Revenue Code section 4982 made this fact visible in the time series by coordinating mutual fund distributions to Q4.

Panel (c) shows that firm dividends do not follow a similar pattern. Firm dividends are close to evenly distributed across all quarters and Appendix Figure A1 shows that the same is true for share repurchases and M&A payouts. This difference in timing between corporate payouts and mutual fund distributions makes it simple to check whether mutual funds reinvest corporate payouts. If they did not, their equity portfolio share would steadily decrease by about 1.5% per quarter, the average quarterly cash payout, over the calendar

year as equity is turned into cash by corporate payouts. The equity portfolio share should then recover during Q4 when funds distribute the cash. This is not the case. Panel (d) of Figure 4 plots the equity share time series starting in 1990, when the variable becomes reliably available. The equity share is close to constant over the calendar year, thereby showing that mutual funds reinvest corporate payouts into equities.

3.2 Portfolio Position Level Evidence

Mutual funds use cash payouts they receive from firms to buy stocks. But this behavior can only be exploited to construct an exogenous asset demand shock if funds reinvest into their *existing* portfolio. This is because hypothetical reinvestment in proportion to existing portfolio weights is likely exogenous to firm fundamentals, while actual trades may well be driven by firm fundamentals. Such reasoning follows the literature that constructs asset demand shocks from mutual fund flows (e.g. Edmans, Goldstein, and Jiang, 2012). Luckily, reinvestment into the existing portfolio is likely a good description of actual trading behavior because many funds are benchmarked, closet indexers, or index funds (Cremers and Petajisto, 2009). Here, I test this using portfolio position level data.

Institution i holds $Shares_{i,t}(n)$ split-adjusted shares of stock n at time t. I denote relative payout flows as $payoutflow_{i,t} = PayoutFlow_{i,t}/A_{i,t-1}$. The goal 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

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

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 in proportion to portfolio weights, $\beta = 1$. If they do not reinvest, $\beta = 0$.

I also include institution and stock x time fixed effects. Institution fixed effects address 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 I include fund flows in the vector of control variables, X. The firm x time fixed effects address that firms are the source of payouts. By definition,

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shareholders of payout stocks receive payout flows. So if institutions systematically bought payout stocks, 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. In addition to including firm x time fixed effects, I also address this potential concern by estimating equation 2 on the subsample that excludes payout stocks.

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 4 does not suffer from this attenuation bias because it examines returns at the daily frequency.

Portfolio holdings allow me to examine the reinvestment behavior of institution types beyond mutual funds. I nevertheless start 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 which assets under management, portfolio returns, and investment flows are observable. For other institution types, I have 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. Hence, while the results for other institution types provide additional evidence, the results for mutual funds are most reliable.

Table 2 reports regression estimates of different specifications of equation 2 for mutual funds. On average, payout recipients cannot reinvest into the payout stock itself, because the supply of shares does not increase. Therefore, I report results using the full sample (columns 1 and 2) and the sample that excludes payout stocks (columns 3 and 4). I also report results including and excluding control variables. Using the full sample, I estimate a highly statistically significant coefficient 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 firms in columns 3 and 4, the coefficient decreases to 0.65 but remains highly statistically significant. Overall, mutual funds reinvest the majority of dividend payouts into their existing portfolio.

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, I report results for mutual funds (repeated for comparison), investment advisors, pension funds, banks, insurance companies, and unclassified institutions. I find all institution types except banks reinvest dividends into their existing portfolios. For banks, the coefficient is positive, but close to zero and statistically insignificant. Apart from banks and mutual funds, investment advisors (e.g. hedge funds) 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, I 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 my 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. In Appendix Tables A1 and A2, I report the analogous analysis for M&A instead of dividend payouts. The results are qualitatively the same. For share repurchases, I 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.

4 Price Pressure Effects

This section investigates spillover effects of payout events on returns of other stocks that are held in the same portfolios of financial institutions. The analysis is at the stock level, allowing me to investigate the effects at the daily frequency.

4.1 The Demand Shock

Section 3 shows that the notion that institutions reinvest firm payouts into their existing portfolio in proportion to portfolio weights, i.e. that institutions buy $payoutflow_{i,t}Shares_{i,t-1}(n)$ shares, is a good description for all institution types except banks. I exploit this behavior to construct an asset demand shock by aggregating the hypothetical number of shares non-bank institutions buy in response to payout flows. Scaling by shares outstanding gives the relative demand shock

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

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 5 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 5 shows that investor heterogeneity generates large cross-sectional dispersion in payout-induced trading.

4.2 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 6 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

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

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

4.3   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.

4.3.1   Ordinary Dividends

I start with ordinary cash dividends, because they allow for the cleanest identification. Figure 7 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 4. 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.

The self-effects in panels (a) and (b) are well known (Ogden, 1994; Hartzmark and Solomon, 2013). This paper's contribution is to examine payouts' spillover effects. 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 5.3 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, indicating 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, the main contribution of this paper, which I highlight in red. 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 0.75% return. This implies an asset demand elasticity of 1.5, 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. In the literature, whether price pressure effects are followed by reversal depends on the setting. My finding of no reversal is consistent with the payment date effects documented in Ogden (1994), as well as with newer studies ex-

ploiting 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.

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. Further, in comparison to cash transactions, stock transactions should generate smaller payout-induced trading price pressure effects. Equity investors reinvest cash but only reinvest stock payouts if they decide to liquidate. Therefore, I also evaluate pricing effects around stock dividend and stock M&A events as a placebo test.

4.3.2 Cash M&A Payouts

Figure 8 repeats the analysis in Figure 7 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 coef-

ficients 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.

4.3.3 Share Repurchases

Figure 9 repeats the analysis in Figure 7 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) documents modest and 2-days-delayed spillover effects on connected stocks. It is not clear whether this result 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.

4.3.4 Stock Dividend Payouts (Placebo)

I now turn to placebo tests based on stock instead of cash payouts. The payout-induced trading mechanism predicts that the results for stock transactions should be qualitatively the same as for the corresponding cash transactions, with one key exception: stock payouts

should generate smaller payment date price pressure effects on the stock itself and on connected stocks, because equity investors reinvest cash but may hold the stocks they are paid. The tests confirm this notion.

I begin by examining pricing effects around stock dividend events. 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. Also, unlike cash dividends, stock dividends have no fundamental consequences. Stock dividends do not reallocate cash; they merely act as stock splits. 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.

Figure 10 repeats the analysis in Figure 7 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 same (i.e. no) announcement and peer effects, and in contrast to cash dividends, generate no payout-induced trading price pressure effects.

4.3.5   Stock M&A Payouts (Placebo)

Figure 11 repeats the analysis in Figure 8 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 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.

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4.4   Institutional Heterogeneity

One reason institutions engage in payout-induced trading is likely that many are bench-marked. 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 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 12 reports the same analysis as panel (f) of Figure 7 but by institution type. Broadly, I find price pressure effects for mutual funds and investment advisors (e.g. hedge funds) but not for banks, pension funds, insurance companies, or unclassified institutions (e.g. endowments). This is because, as documented in sections 2 and 3, mutual funds and investment advisors are large and engage in payout-induced trading, banks are large but do not engage in payout-induced trading, and other institution types are small. 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, relatedly, section 3 documents that investment advisors have a lower propensity to reinvest payouts, 0.4 in comparison to 0.65 for mutual funds. Finally, investment advisors are more price elastic than mutual funds; they are less likely to continue buying as prices are rising.

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 4.3.2 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 (i.e. investment advisors) are the

main institution types that run merger arbitrage strategies, the results are consistent with this reasoning. Appendix Figure A3 reports the results for the remaining institution types, which are largely statistically insignificant.

4.5 Cross-Sectional Heterogeneity

Figure 13 investigates whether the price impact of payout-induced trading varies by firm size by repeating the analysis in panel (f) of Figure 7 for below- and above-median-market-cap stocks, separately. Panel (a) shows that a 1% dividend-induced trading demand shock generates a 1% price response among small firms. Panel (b) finds that the same shock generates a 0.75% price response among large firms. While the difference is not statistically significant, it is economically significant as it implies that asset demand elasticities are lower for small than for large stocks, likely because sellers are more reluctant when trading costs are high. Panels (c) and (d) present the analogous analysis for cash M&A payouts. The overall pattern is the same, though the estimated cumulative coefficients are smaller for the reasons discussed in section 4.3.2. The price impact of M&A-induced trading is positive and statistically significant for small and large cap stocks and, again, the price impact is larger for small than large cap stocks.

This exercise also highlights one advantage of payout-induced trading over index additions/deletions as an asset demand shock. Stocks that are added to or deleted from an index are very similar in terms of market cap because the respective indices are largely based on a market cap ranking (e.g. Chang, Hong, and Liskovich, 2015). Therefore, index/additions deletions cannot shed light on heterogeneity in asset demand elasticities across firm size. In contrast, the payout-induced trading shock allows me to document that asset demand is more elastic for large caps.

5 Market Feedback Effects

This section examines how stock prices affect corporate financing and investment. I investigate these effects at the annual frequency because managers make corporate decisions at low frequency and 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 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; 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 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).

5.1   Identification Strategy

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

$$\frac{I_t(n)}{K_{t-1}(n)} = \alpha_t + \alpha(n) + \beta Q_{t-1}(n) + \gamma' X_{t-1}(n) + \xi_t(n), \tag{5}$$

a regression of the investment rate on Tobin's Q, control variables, time, and firm fixed effects at the annual frequency. Estimation at the annual frequency accommodates investment seasonality and that investment may respond slowly because of financial frictions (e.g. Fazzari et al., 1988; Kaplan and Zingales, 1997). The regression is motivated by a Q-theory

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[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).

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

$$\Delta \frac{I_t(n)}{K_{t-1}(n)} = \eta_t + \beta r_{t-1}(n) + \tilde{\gamma}' \tilde{X}_{t-1}(n) + \epsilon_t(n) \tag{6}$$

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 4 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 4, 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.

5.2 The Investment Response

Table 4 reports estimates of equation 6. I show the first and second stage of 2SLS estimates of equation 6, 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 A5 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 0.07, meaning a 1% return is associated with a 7 basis point increase in investment. This corresponds to about 0.4% 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 48. In the second stage, I estimate a highly statistically significant coefficient of 0.1. This estimate is higher than the OLS coefficient of 0.07, 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 42. In the second stage, the coefficient remains statistically significant and increases from 0.1 to 0.16. 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 29, and the coefficient in the second stage decreases slightly to 0.13. The investment-return elasticity estimates range from 0.1 to 0.16, but their difference is not statistically significant. Overall, a 1% return translates into a 0.1 to 0.16% increase in investment, almost 1% of the median annual investment rate. Returns translate into investment almost one-for-one.

5.3   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 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, 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 5 reports estimates of equation 6 using expected dividend-induced trading as the instrument for returns. The results are close to identical to the estimates in table 4. The instrument is slightly weaker. Looking at the strictest specification in column 5, the F-statistic is 28 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 0.16 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.

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5.4   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 6 but substituting the investment rate for the debt or equity issuance rate. I show the results in Table 6 which reports results corresponding to columns 3 and 4 in Table 4, 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 0.25 and statistically significant at the 5% level. In response to a 1% return, firms increase debt by 0.25%. 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 0.02. 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.

5.5   Robustness

The Appendix contains further robustness results. In Table A4, I find the same results qualitatively when excluding payout firms. In fact, the coefficients in the first and second stage increase by about 50%. 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 A5 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, Table A6 reports evidence for partial long-run return reversal after the dividend-induced trading shock. While I do not find reversal at the daily frequency in section 4, annual frequency regressions of returns on dividend-induced trading and four lags of dividend-induced trading suggest that about half of the price impact reverses over the following year. The coefficients on the deeper lags are statistically insignificant. In the literature, as discussed in section 4, whether price pressure effects are followed by reversal depends on the setting, with findings ranging from no reversal (e.g. Ogden, 1994; Kaul, Mehrotra, and Morck, 2000; Chang, Hong, and Liskovich, 2015) to partial reversal (e.g. Greenwood, 2005) to full reversal (e.g. Edmans, Goldstein, and Jiang, 2012). In comparison, I find no short-term reversal but partial long-term reversal.

Finally, Appendix Tables A7 and A8 report results for alternative specifications of the regressions that test for market feedback effects on corporate financing. Appendix Table A7 reports results for the stricter specification that includes time x industry fixed effects. Appendix Table A8 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.

6   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 shows three consequences of firm payouts. First, when firms pay dividends, repurchase shares, or are acquired, financial institutions preferentially invest the cash proceeds into their existing portfolio. I call this reinvestment *payout-induced trading*. Second, payout-induced trading drives up asset prices. The estimated price pressure effects imply an asset demand elasticity of 1.25. While the literature already documents that firms experience abnormal returns around their own payout events, this paper documents that payouts generate spillover effects on other firms held in the same portfolios of financial institutions. In doing so, my analysis reveals a new channel through which financial institutions impact asset prices namely the reinvestment of payouts.

Third, payout-induced trading has real effects. Using payout-induced trading as an instrument for stock returns, I 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 shows 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
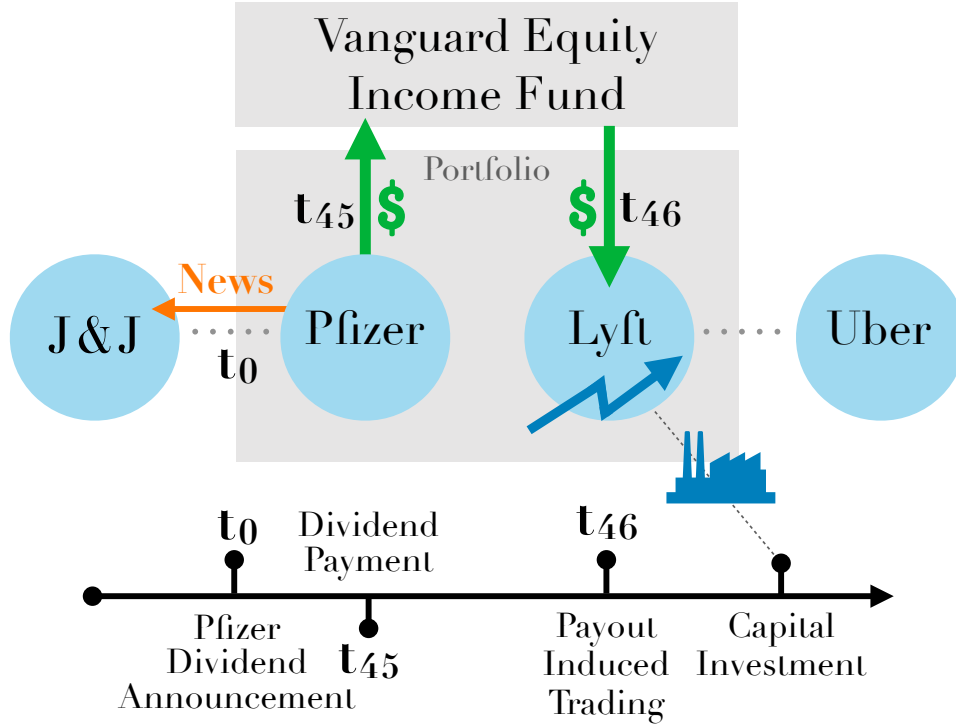
This figure illustrates the payout-induced trading effect. At t_0, Pfizer announces a cash dividend. This reveals fundamental information about Pfizer and potentially also about peer firms such as Johnson & Johnson. The dividend is paid with a 45-day lag at t_{45}. Vanguard Equity Income Fund holds Pfizer 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 Findings
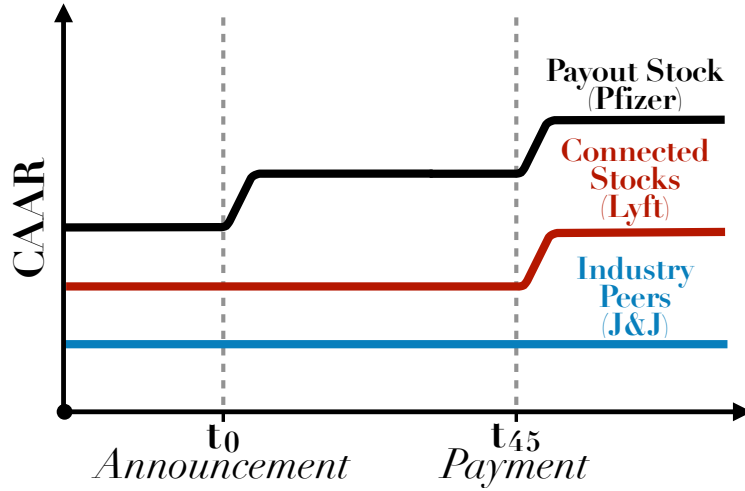
(a) Returns around Dividend Events
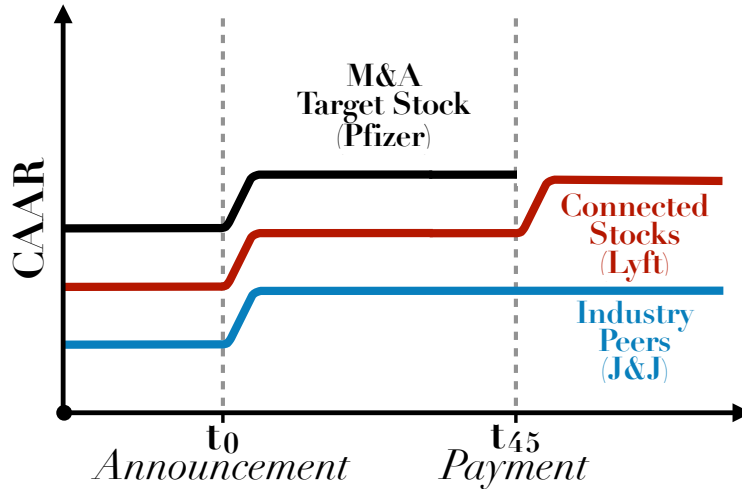
(b) Returns around M&A Events

This figure illustrates the main identification strategy and findings. 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 carry out a systematic analysis of returns around payout events. For each type of payout, I show returns of the firm itself, peer stocks, and connected stocks around announcement and payment dates. Two patterns emerge, illustrated in panels (a) and (b). While connected stocks always show payment date price pressure effects, and industry peers never show payment date effects, the empirical analysis demonstrates that dividend announcements do not reveal significant fundamental information about other stocks (a), but M&A announcements do (b). In addition, as is widely known, the payout firm itself always experiences positive abnormal returns, though for M&A events, targets cease to exist after the payment date. Overall, I find dividends and M&A payouts both generate large price pressure spillover effects on connected stocks, but only M&A payouts generate news spillover effects.

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

(a) Sources of Asset Demand Shocks

(b) Payout Type Breakup
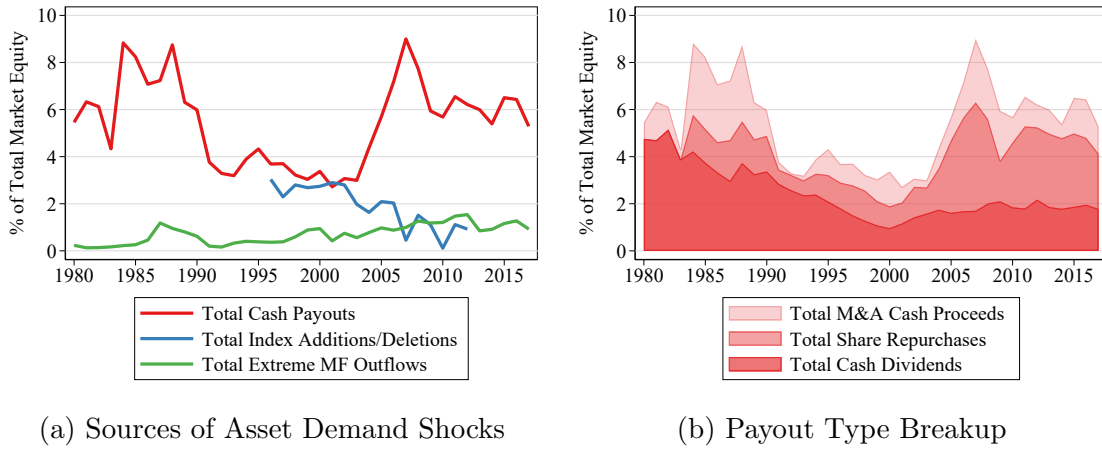
This figure shows summary statistics. The first plot compares total cash payouts to two prominent, alternative sources for asset demand shocks: total absolute extreme mutual fund outflows and total market value of firms that are added/deleted from the Russell 1000/2000 index. All three are scaled by 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: Quarterly Mutual Fund Distributions vs. Firm Dividends

(a) Mutual fund dividend distribution

(b) Mutual fund capital gains distribution

(c) Firm dividends

(d) Mutual fund equity holdings
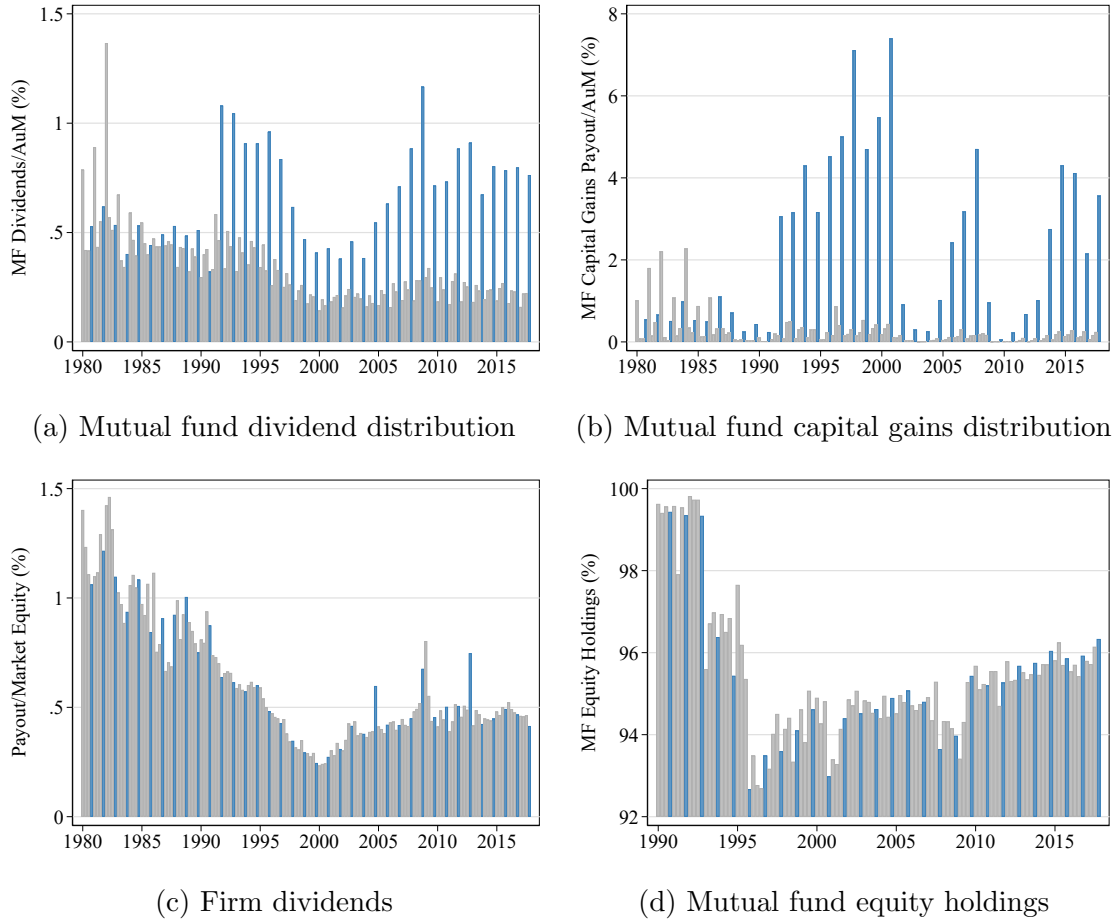
This figure contains time series plots, with the last quarter of each year highlighted in blue. Panels (a) and (b) plot the time series of aggregate mutual fund dividend and capital gains distributions as a fraction of total assets under management. For comparison, Panel (c) shows firm dividends as a 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 5: The Payout-Induced Trading Shock

(a) Dividend-Induced Trading
(b) M&A-Induced Trading
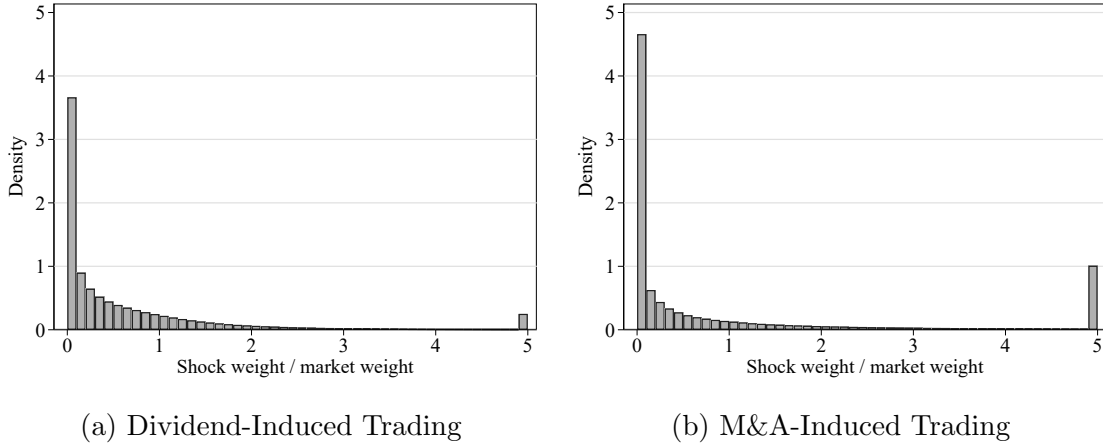
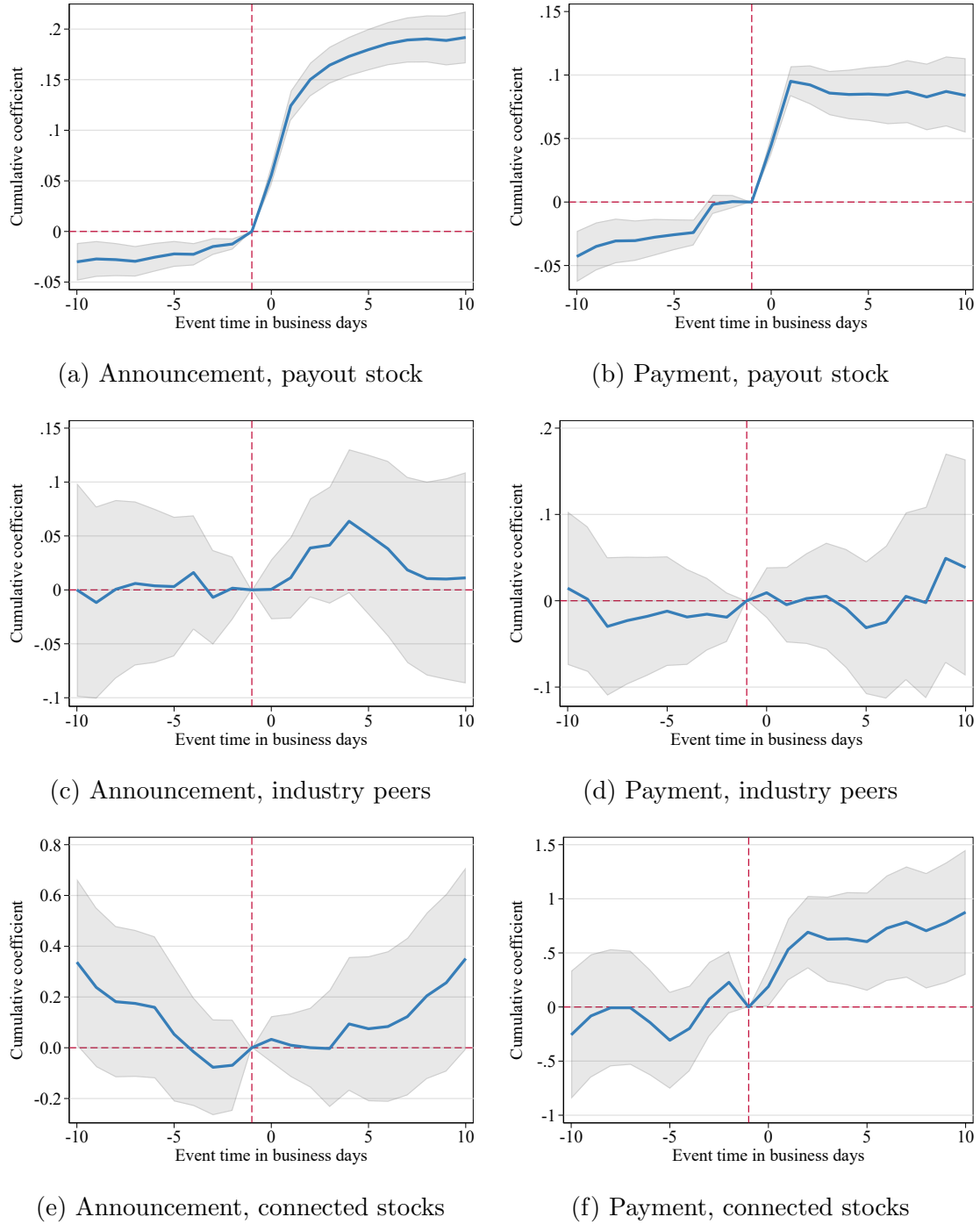The two plots show histograms for the ratio between a stocks' share of dollar payout-induced trading on a given day and its market weight. This construction eliminates time series variation. If all investors held the market portfolio, the ratio would be exactly 1 for all observations. The ratio is not 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 6: Ordinary Dividends

(a) Payment - Announcement Date Lag
(b) Daily Cash Dividends in 2017
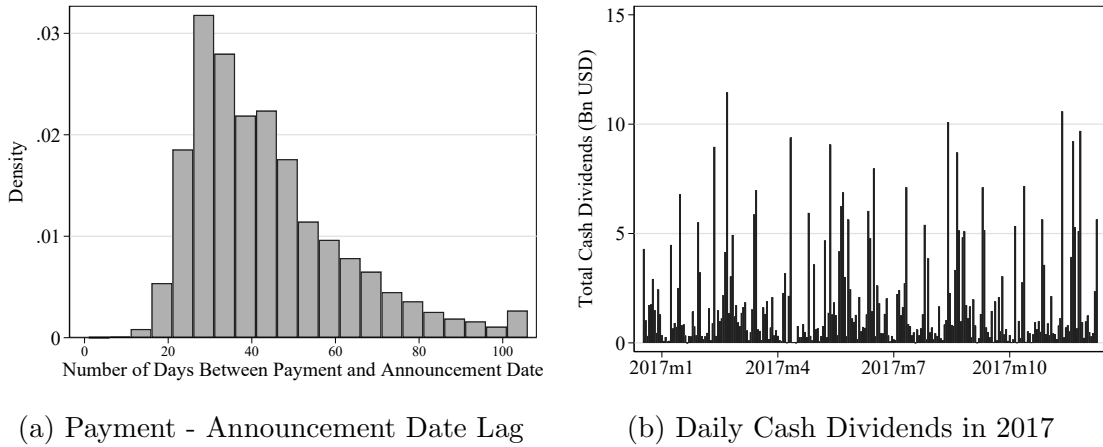
This figure illustrates cash dividend payouts. The first plot shows the distribution of the number of days between the dividend payment and announcement date. The number of days can be very large, so I winsorize it at the 99th percentile for this figure. I single out dividends because they allow for 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 7: 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 4, 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 8: Abnormal Returns Around M&A Announcement and Payment Dates
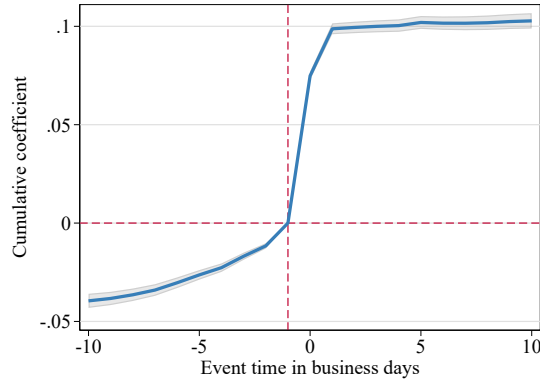
(a) Announcement, payout stock

(b) Announcement, industry peers

(c) Payment, industry peers
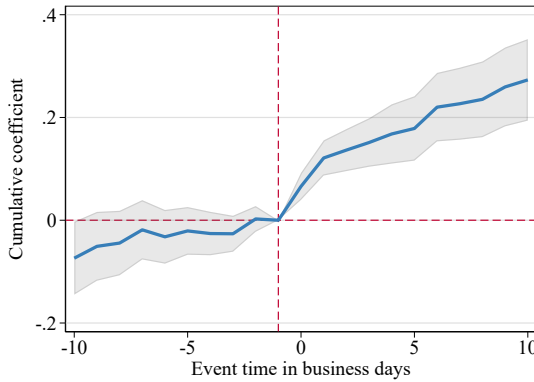
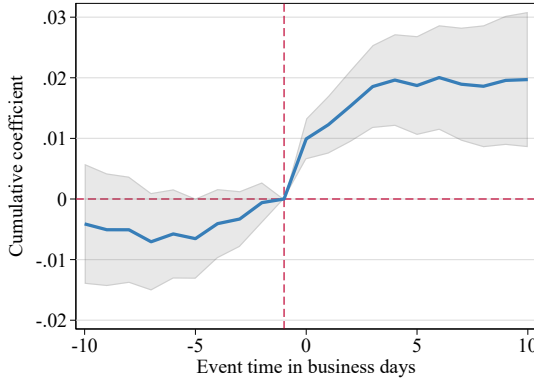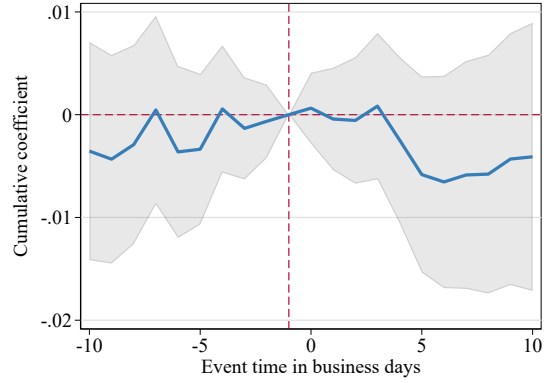(d) Announcement, connected stocks

(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 4, 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 9: Abnormal Returns Around Share Repurchase Announcement Dates

(a) Announcement, payout stock

(b) Announcement, industry peers
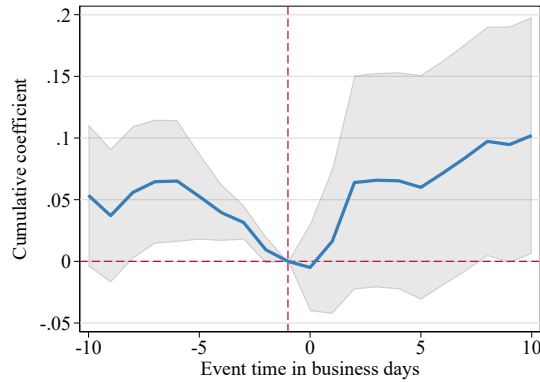
(c) Announcement, connected stocks

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 4, 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 10: Returns Around Stock Dividend Announcement and Payment Dates (Placebo)

(a) Announcement, payout stock
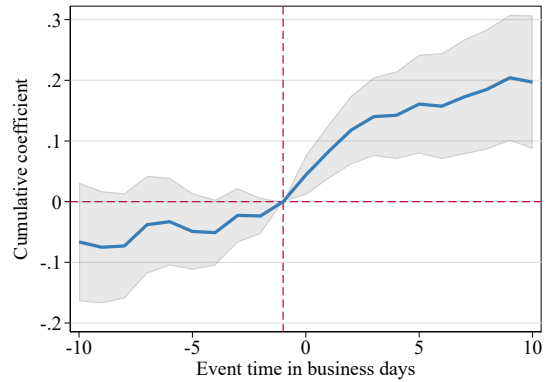
(b) Payment, payout stock

(c) Announcement, industry peers
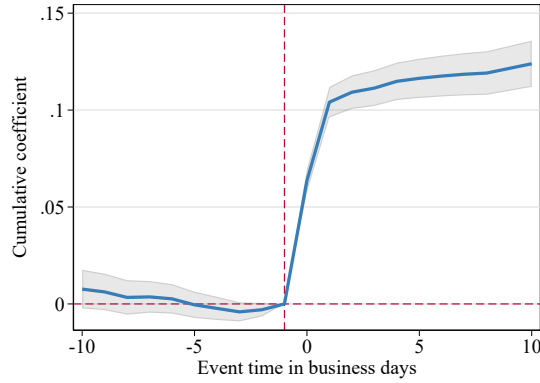
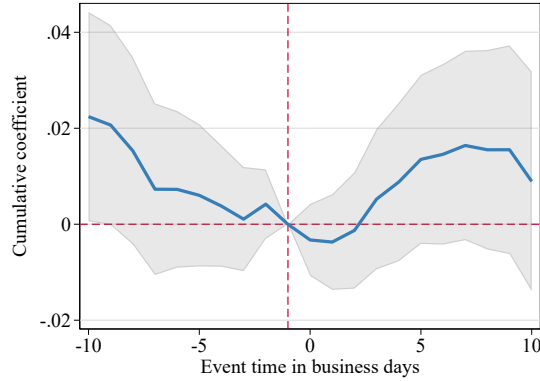(d) Payment, industry peers

(e) Announcement, connected stocks

(f) Payment, connected stocks

The plots show percent cumulative abnormal returns in response to a 1% shock around stock 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 4, 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.

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Figure 11: Returns Around Stock M&A Announcement and Payment Dates (Placebo)

(a) Announcement, payout stock

(b) Announcement, industry peers

(c) Payment, industry peers
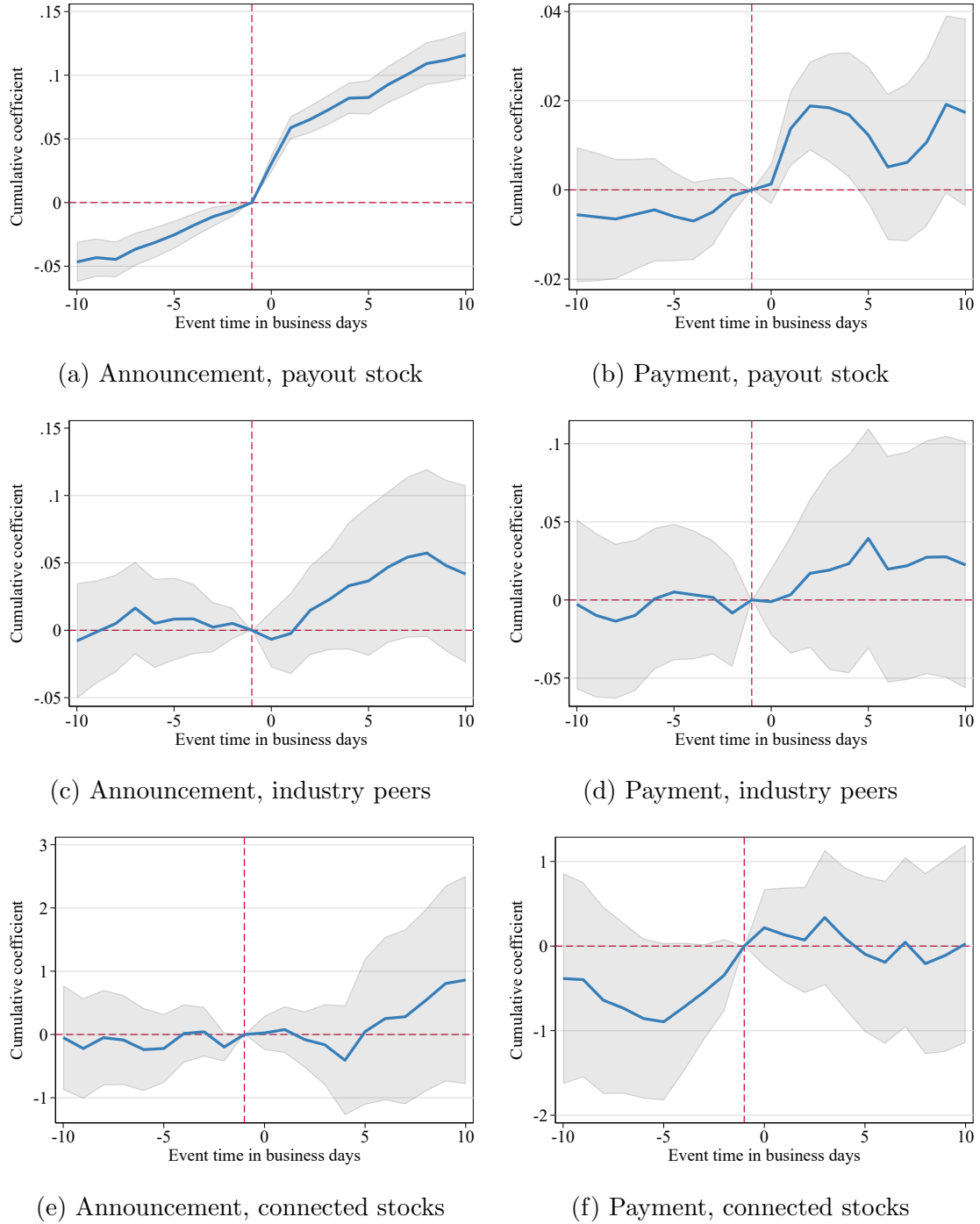

(d) Announcement, connected stocks
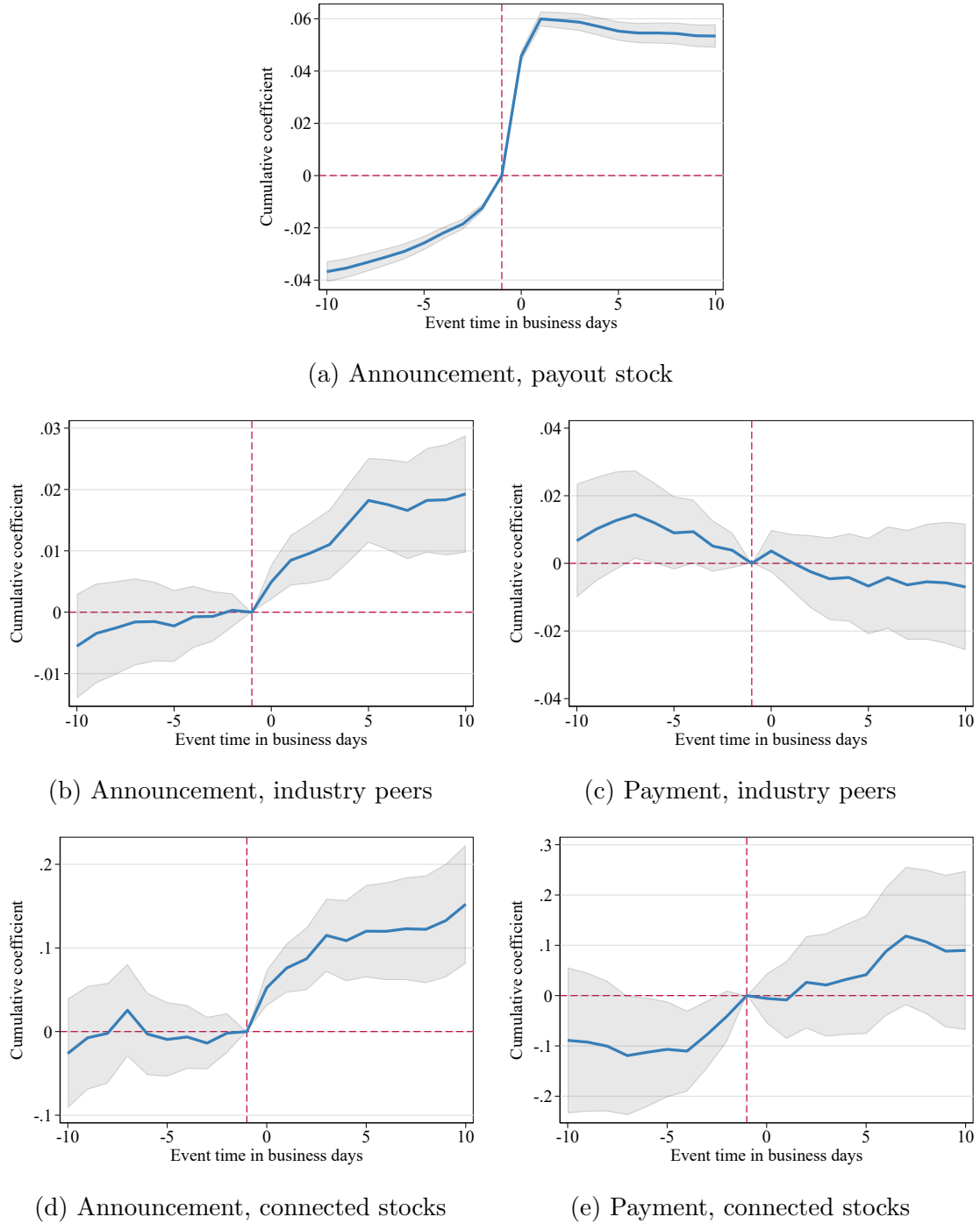

(e) Payment, connected stocks

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 equation 4, 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.

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Figure 12: Price Impact of Payout-Induced Trading – Institutional Heterogeneity
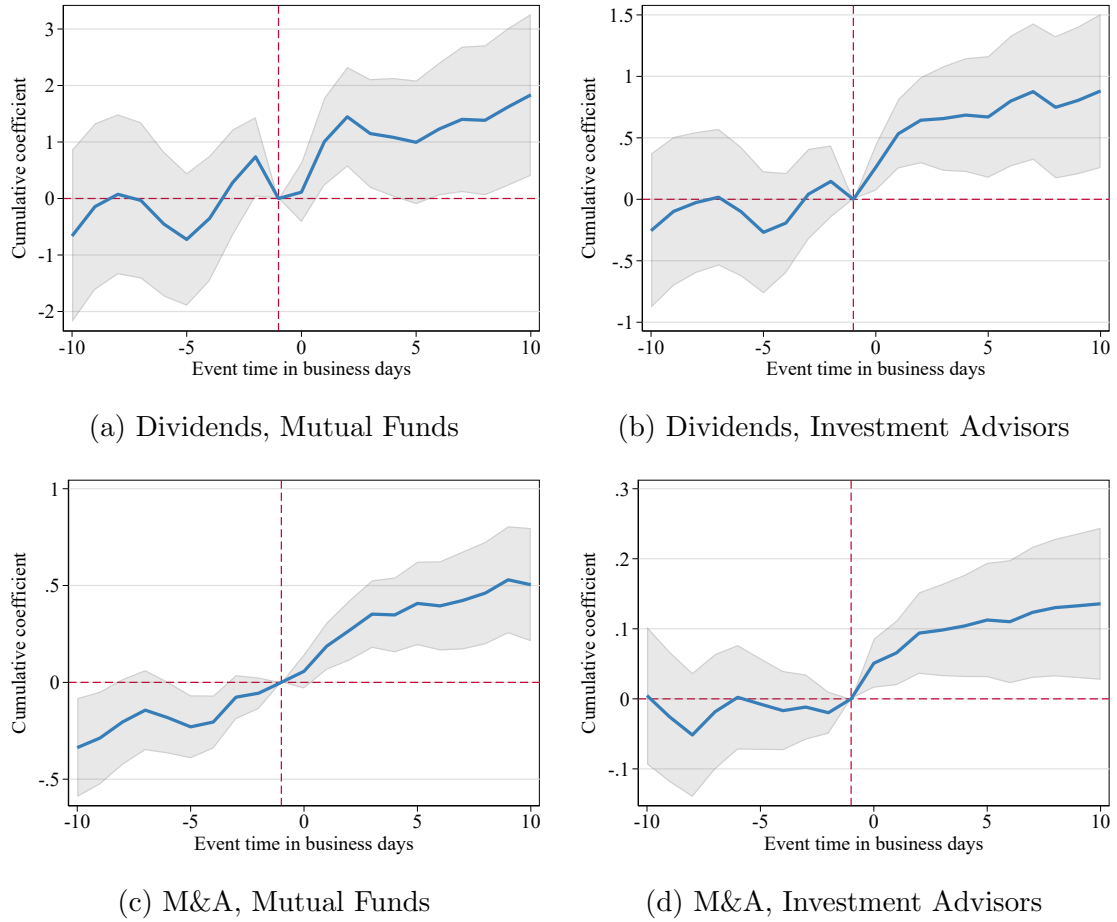
(a) Dividends, Mutual Funds

(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 4, 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.

Figure 13: Price Impact of Dividend-Induced Trading by Size

(a) Dividends, Small caps

(b) Dividends, Large caps

(c) M&A, Small caps

(d) M&A, Large caps
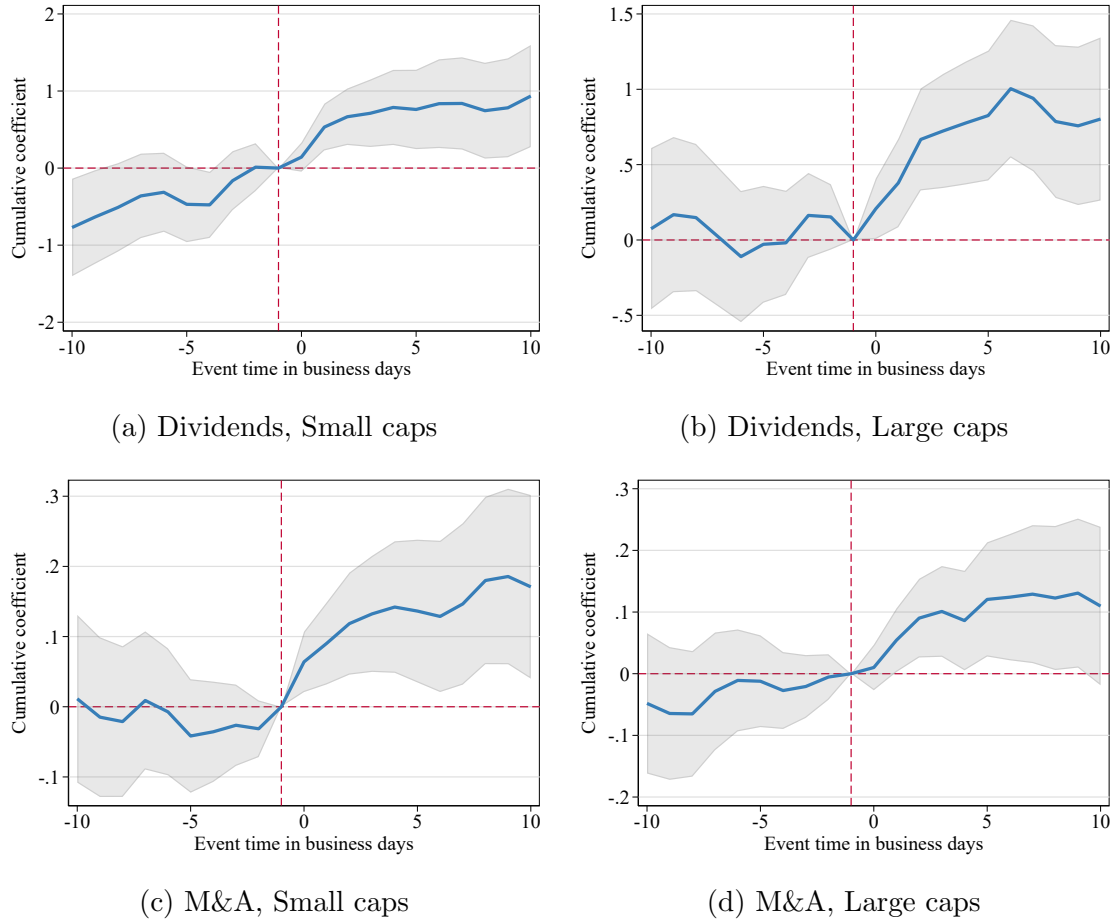
The plots show percent cumulative abnormal returns in response to a 1% dividend-induced trading shock, estimated separately for below- and above-median-market-capitalization stocks, and different payout types. The top row reports results for cash dividend-induced trading, the bottom row for cash M&A-induced trading. The plots are based on estimates of equation 4, 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: Do Institutions Reinvest Payouts Into Their Existing Portfolio?

	Including payout firms		Excluding payout firms	
	(1) Trading	(2) Trading	(3) Trading	(4) Trading
Dividend Flow	0.794***	0.829***	0.572***	0.637***
	(0.0555)	(0.0562)	(0.0687)	(0.0713)
Controls		✓		✓
R-squared	0.108	0.103	0.141	0.134
N	17677791	16080367	6870472	6100798

This table reports estimates of equation 2, regressions of the relative change in shares held on payout flows. As the relative change in shares held is not bounded above, I winsorize it at 100%. Depending on the specification, I control for investment flows. The regression also contains institution and stock x 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, I exclude small positions with portfolio weights less than 0.001. The frequency is quarterly. I report standard errors clustered by institution x time in parentheses. The sample is 1980 to 2017. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table 3: Do Institutions Reinvest Payouts Into Their Existing Portfolio?

	(1) MF	(2) IA	(3) Bank	(4) PF	(5) IC	(6) Other
Dividend 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
N	6100798	3426851	343519	128482	161486	148889

This table reports estimates of equation 2 by institution type, regressions of the relative change in shares held on payout flows. Columns 1 to 6 report results for mutual funds (MF), investment advisors (e.g. hedge funds) (IA), banks, pension funds (PF), insurance companies (IC) and unclassified institutions (e.g. endowments) (Other). I 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, I winsorize it at 100%. The regression contains institution and stock x 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, I exclude small positions with portfolio weights less than 0.001. The frequency is quarterly. I report standard errors clustered by institution x time in parentheses. The sample is 1980 to 2017. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table 4: 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 6, 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 5: 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 4, except that the instrumental variable is *expected* dividend-induced trading. It shows estimates of equation 6, 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 6: Capital Structure Rebalancing Effect

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

This table is analogous to table 4, 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 6, 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: Quarterly Firm Payouts

(a) Cash M&A payouts

(b) Share repurchases
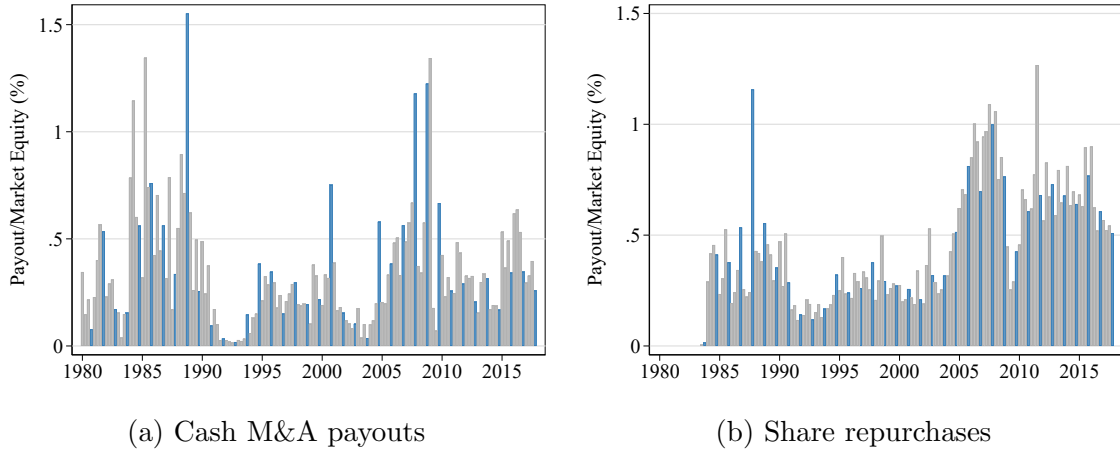
This figure complements Figure 4. It contains time series plots, with the last quarter of each year highlighted in blue. The plots show firm payouts as a fraction of total market equity. Panel (a) shows cash M&A payouts, panel (b) shows share repurchases. The sample is from 1980 to 2017.

Figure A2: Mutual Fund Distribution Reinvestment Rate
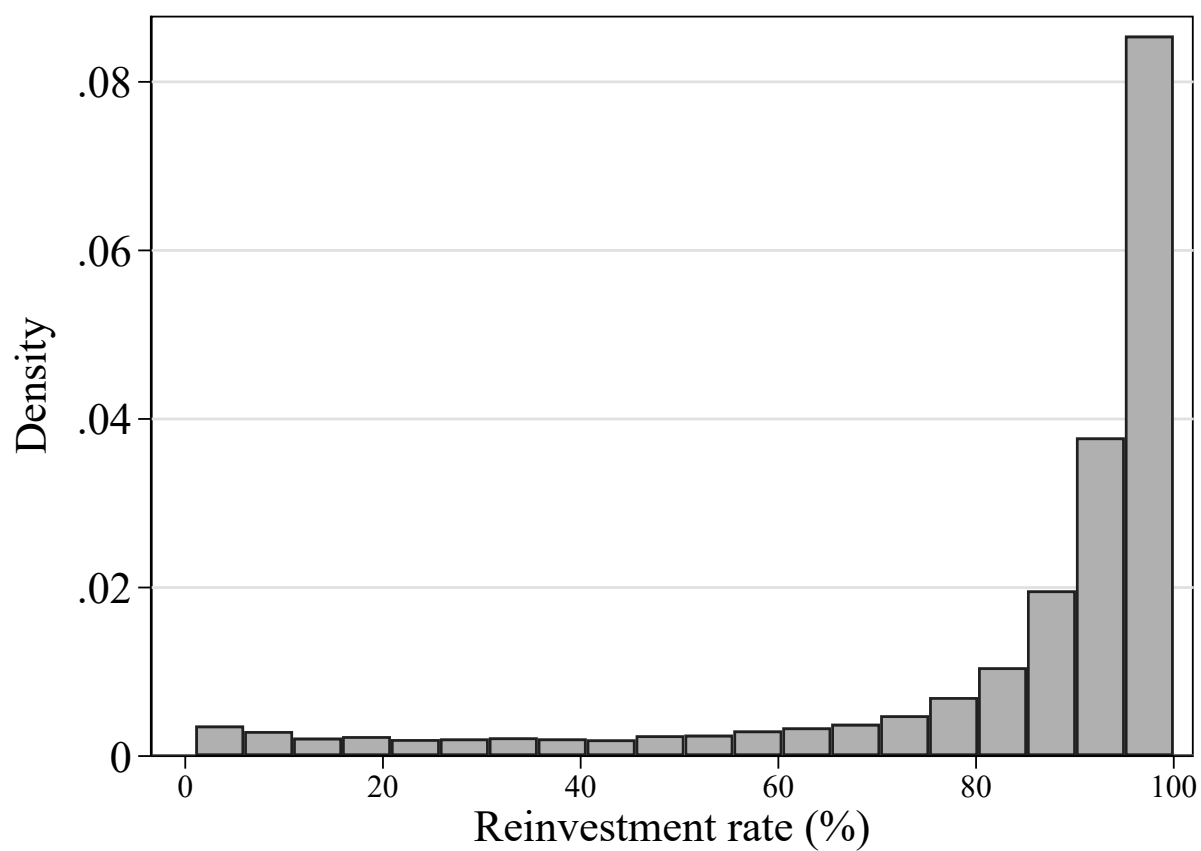
This figure shows a histogram of the mutual fund distribution reinvestment rate. The sample is the quarterly mutual fund level panel from 1995 to 2017. The sample starts in 1995 because this is when Morningstar reinvestment data become reliably available.

Figure A3: Price Impact of Payout-Induced Trading – Other Institution Types
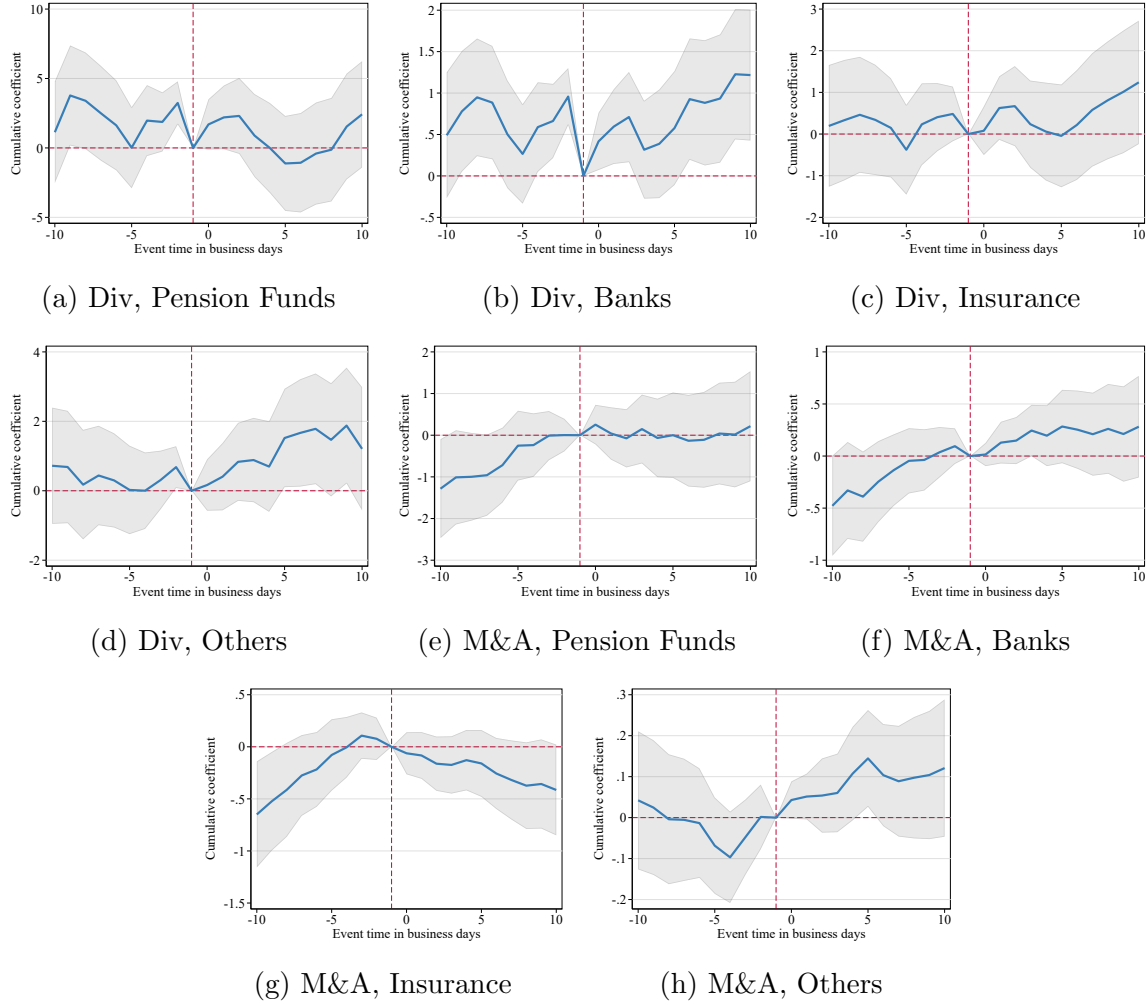
(a) Div, Pension Funds

(b) Div, Banks

(c) Div, Insurance

(d) Div, Others

(e) M&A, Pension Funds

(f) M&A, Banks

(g) M&A, Insurance

(h) M&A, Others

The plots show percent cumulative abnormal returns in response to a 1% payout-induced trading shock for different institution and payout types. The institution types are pension funds, banks, insurance companies, and unclassified institutions; the shock types are cash dividend-induced trading (indicated by "Div") and cash M&A-induced trading. I specify the relevant combination of institution and payout type in the caption. The plots are based on estimates of equation 4, 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 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 A4: Relative Dividend Prediction Error
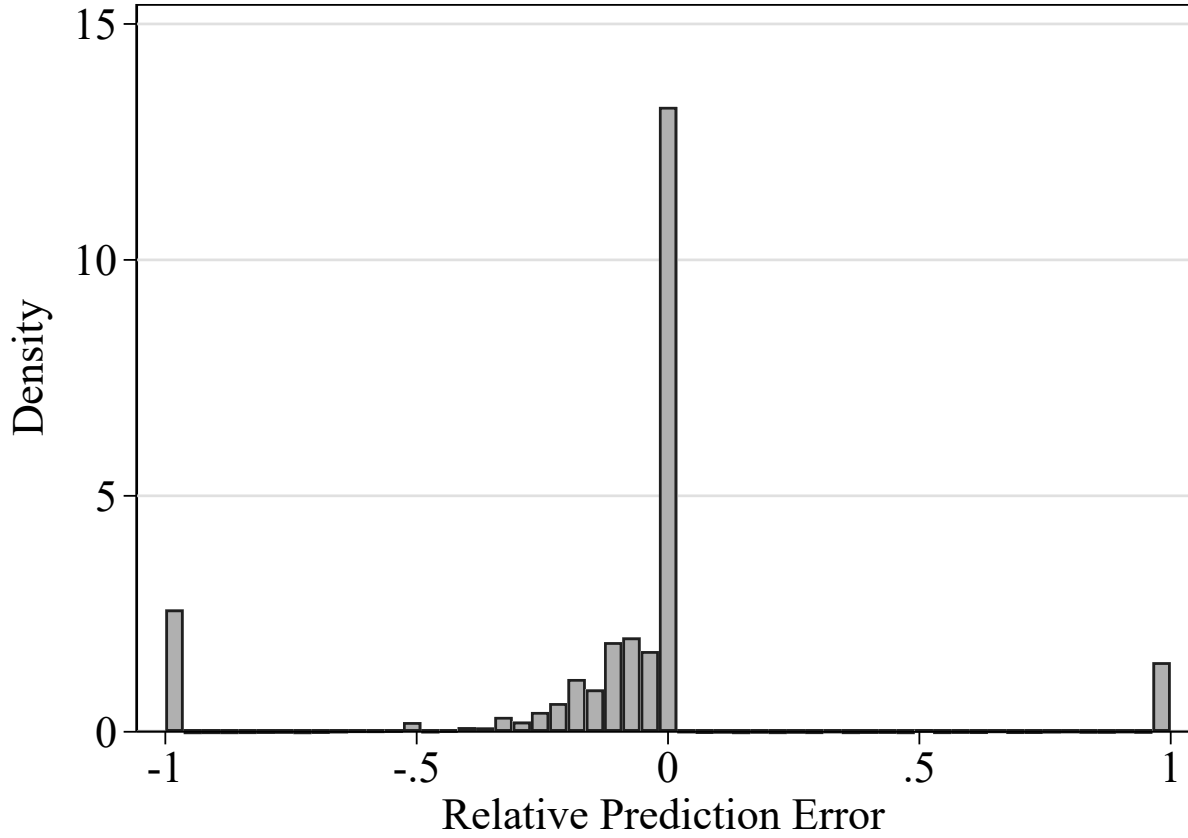
This histogram shows the distribution of the relative dividend prediction error, i.e. the prediction error scaled by the prediction target. -1 and 1 are dividend initiations and dividend discontinuations, respectively. At the end of each year, I predict split-adjusted dividends per share for each quarter 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: Do Institutions Reinvest Payouts Into Their Existing Portfolio?

	Including payout firms		Excluding payout firms	
	(1) Trading	(2) Trading	(3) Trading	(4) Trading
M&A Flow	0.557***	0.569***	0.588***	0.602***
	(0.0117)	(0.0124)	(0.0122)	(0.0128)
Controls		✓		✓
R-squared	0.157	0.152	0.129	0.124
N	17677773	16080338	17546880	15963488

This table is analogous to table 2, except that I examine cash M&A instead of dividend flows. I report estimates of equation 2, regressions of the relative change in shares held on payout flows. As the relative change in shares held is not bounded above, I winsorize it at 100%. Depending on the specification, I control for investment flows. The regression also contains institution and stock x 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, I exclude small positions with portfolio weights less than 0.001. The frequency is quarterly. I report standard errors clustered by institution x time in parentheses. The sample is 1980 to 2017. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A2: Do Institutions Reinvest Payouts Into Their Existing Portfolio?

	(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
N	15963488	10852070	2932246	761949	880874	557560

This table is analogous to table 3, except that I examine cash M&A instead of dividend flows. I report estimates of equation 2 by institution type, regressions of the relative change in shares held on payout flows. Columns 1 to 6 report results for mutual funds (MF), investment advisors (e.g. hedge funds) (IA), banks, pension funds (PF), insurance companies (IC) and unclassified institutions (e.g. endowments) (Other). I report results for the strictest specification corresponding to column (4) of table A1, i.e. including controls and excluding payout stocks. As the relative change in shares held is not bounded above, I winsorize it at 100%. The regression contains institution and stock x 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, I exclude small positions with portfolio weights less than 0.001. The frequency is quarterly. I report standard errors clustered by institution x time in parentheses. The sample is 1980 to 2017. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A3: 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 A4: 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 4, 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 6, 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 A5: 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*** (0.00187)		0.0140*** (0.000643)		0.0140*** (0.00189)	
return		0.171*** (0.0409)		0.171*** (0.0188)		0.171*** (0.0369)
F-statistic	56.06		474.9		54.96	
N	112076	112076	112076	112076	112076	112076

This table is analogous to table 4, 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 6, 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 A6: Partial Reversal After Payout-Induced Trading Shock

	(1) return	(2) return	(3) return	(4) return	(5) return	(6) return
DIT	0.0241*** (0.00298)	0.0186*** (0.00531)	0.0168*** (0.00252)	0.0173*** (0.00467)	0.0135*** (0.00254)	0.0165*** (0.00471)
L.DIT		-0.00556 (0.00470)		-0.0101** (0.00477)		-0.00941** (0.00454)
L2.DIT		0.00286 (0.00306)		0.00319 (0.00242)		0.00274 (0.00244)
L3.DIT		0.00200 (0.00338)		0.00114 (0.00300)		0.000816 (0.00306)
L4.DIT		0.00274 (0.00230)		0.00187 (0.00204)		0.00119 (0.00175)
Controls			✓	✓	✓	✓
Time FE	✓	✓	✓	✓		
Time x ind. FE					✓	✓
N	149313	92416	132472	92416	128600	89754

This table reports results from cross-sectional regressions of returns on lags of dividend-induced trading. Depending on the specification, I include time fixed effects, time x industry fixed effects, and control variables. The control variables are stock characteristics as described in the data section. I report standard errors clustered by time in parentheses. ***, **, and * denote significance at the 10%, 5%, and 1% levels.

Table A7: 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*** (0.0121)		0.257* (0.134)	-0.0464*** (0.0161)		0.0205 (0.0955)
DIT		0.0118*** (0.00204)			0.0128*** (0.00219)	
F-statistic		33.16			34.42	
N	98574	98574	98574	91953	91953	91953

This table is analogous to table 6, except that I control for time x industry fixed effects instead of time fixed effects. The table shows estimates of equation 6, 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 A8: 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*** (0.0120)		0.274** (0.115)	-0.0464*** (0.0161)		0.00991 (0.105)
DIT		0.0124*** (0.00211)			0.0111*** (0.00199)	
F-statistic		34.72			31.02	
N	98574	98574	98574	91953	91953	91953

This table is analogous to table A7, except that the instrumental variable is *expected* dividend-induced trading. The table shows estimates of equation 6, 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.