Price and Liquidity Spillovers during Fire Sale Episodes

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ABSTRACT

We study price and liquidity spillovers in U.S. stock markets around mutual fund fire sales. We find that the well-documented impact-reversal pattern for the returns of fire sale stocks (e.g., Coval and Stafford, 2007) spills over onto the stock returns of economic peers, with a magnitude that is around one fifth of the original effect. These spillovers extend to liquidity and are not explained by common funding shocks or the hedging activity of liquidity providers. We conclude that they represent information spillovers due to learning from prices, thus identifying cross-asset learning as an important driver for the commonality in returns and liquidity.

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How much do investors learn from stock prices? Do they learn across assets, that is, extract information about one stock from the prices of other (economically related) stocks? These questions are important as they pertain to the information efficiency of stock prices as well as to the comovement in returns and liquidity. Standard theory on these questions assumes rational expectations, i.e., builds on the premise that investors rationally condition their expectations on *all relevant* price signals.[1] There are, however, reasons to believe that learning from prices will not be perfect. First, rational expectation models leave little room for disagreement and thus have a hard time explaining observed levels of trading activity.[2] Second, given the sheer number of potentially relevant price signals, perfect learning from prices amounts to solving a tremendous filtering problem that is likely to overwhelm even the most sophisticated investors. The extent of learning from prices thus remains an open question.

In this paper, we shed light on this question by studying whether investors learn from the price changes of economically related peer stocks. This task is challenging because the econometrician does not observe all the information that reaches investors, whether it stems from newswires, analyst reports, internet chat rooms or even word-of-mouth. Thus, if two stocks drop at the same time, the econometrician cannot tell whether this happened because (a) investors learnt about one stock from the price of the other, or (b) because investors in both stocks responded to a common piece of (unobserved) information. We overcome this problem by isolating stock price movements where it becomes clear *ex post* that they occurred without fundamental reason. Specifically, we consider price movements that turn out to be price pressure effects triggered by mutual fund fire sales (Coval and Stafford, 2007), and which therefore revert over time—proving that the fundamentals of affected firms

[1] Rational learning from prices has been first formalized in the seminal models of Grossman (1976), Hellwig (1980), Grossman and Stiglitz (1980), and Admati (1985). These models have seen hundreds, if not thousands, of adaptations over time—commonly referred to as the noisy rational expectations equilibrium (NREE) literature.

[2] See, for instance, Harris and Raviv (1993), Kandel and Pearson (1995), and Hong and Stein (2007).

remain unchanged on average. Hence, there is no fundamental news that can explain a potential spillover.[3]

We then ask whether the price pressure effects for fire sale stocks spill over to their close economic peers, which we identify from the text-based network industry classification (TNIC) developed by Hoberg and Phillips (2010a; 2015). Indeed, when investors learn from prices and are not able to see through the non-fundamental reason of the price drop in the fire sale stock,[4] they should downgrade their expectations about its peers.[5] Over time, investors become aware of the mispricing implied by the fire sale, and the prices of both the fire sale stock and its economic peers should revert. Hence, we expect to find a similar impact-reversal pattern for the peers of fire sale stocks. Put differently, models with rational learning entail that, occasionally, investors make mistakes and update on noise. We argue that, due to the omitted variable problem described above, it is exactly in this case that we can hope to identify learning from prices.

Figure 2 illustrates the main finding of our paper: the impact-reversal patterns from fire sales spill over to peer firms that do not experience fire sales themselves. In the quarter where a mutual fund fire sale hits a firm (Panel A), its economic peers experience a stock price drop that is approximately one fifth of the fire sale effect (Panel B). Both the fire sale and the peer effect reverse over subsequent quarters, confirming the non-fundamental nature of the fire sale shocks. We consider several explanations for these findings—including common

[3] There is a large accounting literature documenting that stock prices respond to the arrival of public news about peer firms (e.g., Firth, 1976; Foster, 1981; Ramnath, 2002; Thomas and Zhang, 2008; Brochet et al., 2016). While showing that investors learn across stocks, these papers cannot distinguish between learning from news and learning from prices.

[4] Given that fund flow and holdings data comes online with a reporting lag of up to two quarters (e.g., Coval and Stafford, 2007; Frazzini and Lamont, 2008), most investors are only able to observe the fire sale with a delay. Indeed, if this were not the case, one would expect more liquidity providers to enter so that the price drop upon the fire sale would be significantly reduced in the first place.

[5] We expect a downgrade if negative news for one firm constitutes negative news for the other firm. For firms competing in the same product market (which will be affected by the same demand shocks), this should be true on average. Below, we also present cross-sectional results in which we differentiate between peers for which this is more or less likely to be the case.

funding shocks, reverse causality and cross-asset hedging by liquidity-providing arbitrageurs—and conclude that they are most consistent with the "learning channel" posited by multi-asset rational expectation models. Consistent with this interpretation, we find that the price spillover effect is stronger when cross-asset learning is more important—i.e., when public information about peer stocks is scarce (e.g., small firms, firms with low analyst coverage or high analyst forecast errors) and for peers that are more closely related to the fire sale firm.

One crucial element for the learning story is the *lack of public information* regarding the non-fundamental nature of the fire sale stock. This observation leads us to conduct a placebo experiment by testing for spillover effects of another well-known price pressure effect for which such information should be available—S&P 500 index additions (Harris and Gurel, 1986; Shleifer, 1986; Beneish and Whaley, 1996; Lynch and Mendenhall, 1997). Though the literature doesn't quite agree on whether the run-up in prices of newly added stocks reflects pure price pressure or also direct benefits of the index inclusion (such as increased investor awareness; see, e.g., Wurgler and Zhuravskaya, 2002; Denis et al., 2003; Chen et al., 2004; Patel and Welch, 2016), the fact that additions are publicly observed means there should be less uncertainty about the value implications for peer stocks. We indeed find that the peers of added stocks exhibit an economically weaker and mostly insignificant spillover effect. This confirms that the lack of public information surrounding fire sales is key to understanding the return spillover effect that we document.

We go on to test two auxiliary predictions of a cross-asset learning channel. First, we find that the characteristics of peer firms not only affect the strength of the price spillover but also the severity of the original fire sale effect: when their peers are more informative, firms suffer less from mutual funds' selling pressure. This provides indirect evidence for the existence of a feedback effect as hypothesized by cross-asset learning models (e.g., Cespa and Foucault, 2014). Second, we document that fire sale firms see a strong dry-up in liquidity, which similarly spills over to peer firms. As we clarify with a standard NREE model based on Admati (1985), this liquidity spillover effect is a unique prediction of a cross-asset learning channel. The intuition is that the fire sale, by reducing the informativeness of the price signal, increases the uncertainty about peer firms, making investors more reluctant to provide liquidity.

Our identification rests on the assumption that mutual fund fire sales are exogenous to affected stocks. While ours is not the only paper making this assumption, we acknowledge that the endogeneity of fire sales is of particular concern in the context of identifying spillover effects. To be precise, there are two layers of endogeneity. First, distressed funds may selectively sell stocks about which they have negative information (see Huang et al., 2016). To the extent that this information also pertains to industry peers, we may see a simultaneous price drop for fire sale stocks and their peers. Second, we may face a reverse causality when industry distress triggers outflows from funds heavily invested in that industry. To immunize our approach against the first concern, we follow Edmans et al. (2015) and identify fire sales based on "hypothetical sales" imputed from a proportional downscaling of a fund's previous portfolio holdings (rather than using their actual sales). To deal with the second concern, we verify in numerous robustness checks that our results are not driven by broad industry trends or funds whose outflows are likely to be caused by industry distress. As we discuss in detail below, the observed return reversal within 1-2 years is further evidence against the reverse causality argument as industry cycles evolve at a more glacial pace (Hoberg and Phillips, 2010b).

Our paper contributes to several strands of research. First, we speak to the literature on comovement and spillovers in asset markets. There is strong evidence for commonality in returns and liquidity (Pindyck and Rotemberg, 1993; Chordia et al., 2000; Hartford and Kaul, 2005; Hasbrouck and Seppi, 2001; Korajczyk and Sadka, 2008). Since these comovements seem excessive relative to the comovement in fundamentals, subsequent research has explored both behavioral explanations (Lee et al., 1991; Bodurtha et al, 1995; Barberis and Shleifer, 2003; Barberis et al., 2005) and financial friction-based explanations (Greenwood, 2005; Andrade et al., 2008; Greenwood and Thesmar, 2011; Anton and Polk, 2014). Another, more closely related branch of the literature focuses on informed order flow as a source of return comovement (Boulatov et al., 2013; Pasquariello and Vega, 2015). While these papers make inroads into establishing cross-asset information flows as an important driver of comovement, they are unable to control for *all* information arrivals that may explain the joint dynamics in order flow and returns. As such, there remains the possibility that returns comove not because investors learn from order flow, but because of information shocks that drive both returns and order flows. We sidestep this problem by considering fire sales as a quasi-natural experiment to cleanly identify a purely learning-based channel for stock price spillovers.

Second, we add to the vast literature on learning in financial markets. While there is a large body of theory on information asymmetry and learning from prices (e.g., Hellwig, 1980; Grossman and Stiglitz, 1980; Wang, 1993), clean empirical tests of primitive predictions from these models remain rare, because investors' information sets are difficult to observe and highly endogenous. One exception is Kelly and Ljungqvist (2012) who exploit exogenous variation in analyst coverage to study how shocks to information asymmetry affect firm valuations. Another, more closely related paper is Banerjee (2011) who solves a dynamic noisy rational expectations model to derive predictions about investors' use of information contained in prices, which he then goes on to test in the data. While the evidence is more consistent with learning from prices, his approach is based on studying correlations and does not allow to quantify the extent of investors' learning activity. We contribute by applying an empirical design that allows to identify whether and how much investors learn from the stock prices of peer firms. This enables us to directly confirm a basic assumption from rational expectation models with multiple price signals (Admati, 1985; Caballé and Krishnan, 1994; Kodres and Pritsker, 2002; Veldkamp 2006; Bernardt and Taub, 2008).

Third, we contribute to the literature on mutual fund trading pressure. Coval and Stafford (2007) show that the trading behavior of mutual funds with extreme outflows lead to price pressure effects for affected stocks.[6] Since mutual fund flows can be treated as largely exogenous from the perspective of affected stocks,[7] subsequent research has exploited fire sales to shed light on the real effects of stock price changes on corporate outcomes such as takeover activity (Edmans et al., 2012), investment and employment (Hau and Lai, 2013), opportunistic option grant timing and insider purchases (Ali et al., 2011), and seasoned equity offerings (Khan et al., 2012).[8] Related to our work, Dessaint et al. (2016) show that peer firms of fire sale stocks cut investment, consistent with these managers learning from stock prices but failing to filter out the noise induced by fund selling pressure. Instead of looking at corporate outcomes, we study *price* and *liquidity spillovers* between fire sale stocks and their economic peers. In our view, documenting these spillovers is important as it provides clean evidence for the importance of the learning channel—the bedrock of the rational expectations literature—in driving the commonality in returns and liquidity.

Finally, we contribute to an old literature on the variation in stock returns. Starting with Roll (1988) and Cutler et al. (1989), researchers have concluded time and again that firm-specific or market-wide news explain a surprisingly low fraction of the variation in stock return.[9] Our results suggest a new way for understanding this apparent puzzle. Specifically, we show that

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[6] In the international context, Jotikasthira et al. (2012) find that flow shocks to funds domiciled in developed markets affect their asset allocations abroad and thereby transmit to emerging markets. Falato et al. (2016) provide evidence for fire sale spillovers in fixed income markets.

[7] This identifying assumption is supported by the fact that the price pressure effect reverses over subsequent quarters, proving that the fundamentals of affected stocks are unchanged on average. See the robustness section for more discussion on this point.

[8] The evidence on option grant timing, insider trading and SEOs suggests that at least some managers of fire sale firms are aware of the temporary mispricing induced by the fire sale. This finding is in line with the vast literature on the market timing ability of firm managers (e.g., Baker and Wurgler, 2002).

[9] See, e.g., Boudoukh et al. (2015) for a recent analysis of the relation between the stock return variation and news arrivals. The low explanatory power of fundamental news for stock returns is further echoed by a large literature trying to understand the causes for the excessive volatility of stock returns and attributing it mostly to discount rate shocks (Shiller, 1981; Campbell and Shiller, 1988a, 1988b).

stock prices co-move due to cross-asset learning among close economic peers, and that this co-movement may be triggered by noise. As such, future investigations into the drivers of the stock return variation may want to consider the rich network structure and implied cross-asset learning effects that naturally arise when investors cannot perfectly tell apart fundamentals from noise.

The remainder of this paper is organized as follows. Section I lays out the hypotheses tested in this paper. Section II describes the data and methodology. Section III presents the main results on return spillovers, including a cross-sectional analysis and numerous robustness checks. Section IV provides additional evidence in favor of the cross-asset learning channel. Section V concludes.

I. Hypotheses

A. Cross-asset Learning

We draw on multi-asset models with learning from prices to develop our predictions about the *informational spillover* effects of mutual fund fire sales. These predictions are derived in Internet Appendix A using a standard NREE model in the spirit of Admati (1985); here we focus on providing their intuitions. The first prediction concerns a *price spillover effect*: under asymmetric information, the price of the fire sale stock serves as a signal about fundamentally-related peer firms. Rational learning then entails that, unless investors perfectly understand that a price drop in the fire sale stock is caused by noise, they interpret the price drop as representing bad news for peer stocks, causing peers' stock prices to weaken as well. In terms of the model, the fire sale represents an unobserved (positive) shock to the supply of one stock, which pushes down the equilibrium price of both the fire sale stock and its peer. Over time, investors figure out that the reason for the price drop was non-fundamental and prices revert. Hence, models with cross-asset learning (e.g., Admati, 1985; Caballé and Krishnan, 1994; Veldkamp 2006; Boulatov et al., 2013; Pasquariello and

Vega, 2015; Cespa and Foucault, 2014; Asriyan et al., 2016) unequivocally predict that the impact-reversal pattern observed for fire sale firms should spill over to peer firms. Intuitively, such a spillover should be stronger for stocks for which the available public information is scarce because then investors need to rely more on the stock price signals of economic peers.

The second prediction is about a *liquidity spillover effect*, and arises when the fire sale impairs the price informativeness of the fire sale stock.[10] In the model, this can be seen by assuming that a fire sale, in addition to being a large supply shock realization, also causes an increase in the standard deviation of expected supply shocks. Such an increase reduces the signal-to-noise ratio in the fire sale price and thereby raises the overall uncertainty faced by market participants, causing them to withdraw their liquidity from economically-related peer firms. Thus, we expect peer firms to suffer from a temporary deterioration in liquidity around the fire sale. Finally, when spillovers are severe, there can be an additional *feedback effect* to the fire sale stock (Cespa and Foucault, 2014): as peer stocks drop and become less informative, investors worry more about the possibility that mutual funds' selling pressure reflects bad news and thus require a bigger discount for the fire sale stock. To sum up, we expect cross-asset learning to lead to price and liquidity spillovers, as well as feedback, between fire sale stocks and their economic peers. We call this the *information spillover hypothesis*.

[10] There are at least three reasons for why a fire sale may reduce the price informativeness of the fire sale stock: First, even in the absence of adverse selection (as in Cespa and Foucault, 2014), an extreme noise realization in one period may cause risk-averse market makers to update their expectations about future noise trader risk, to which they respond by decreasing liquidity, thereby rendering the price less informative. Second, when market makers are uncertain whether informed traders are present, a large unexpected trade (as from a fire sale) may cause them to update this probability, leading them to demand a higher price impact (e.g., Easley and O'Hara, 1992; Avery and Zemsky, 1998; Banerjee and Green, 2015). Third, fire sale shocks may hurt informed arbitrageurs, causing them to trade less aggressively in the fire sale stock and thereby rendering its price less informationally-efficient (Dow and Han, 2016).

B. Alternative Spillover Channels

Clearly, there are alternative explanations for the existence of spillover effects. For instance, spillover effects between two assets can be triggered by financially-constrained arbitrageurs that are trading in both (Kyle and Xiong, 2001; Gromb and Vayanos, 2002; Brunnermeier and Perdersen, 2009). As these traders suffer losses in one asset, they may be forced (e.g., because of margin calls) to exit their positions in the other asset. Such a contagion effect fits well with anecdotal evidence from prominent fire sale crises such as the collapse of the hedge fund LTCM in 1998. It is also consistent with empirical evidence that stocks with common owners (Anton and Polk, 2014) or different owners with common shocks (Greenwood and Thesmar, 2011) exhibit comovement over and above what can be explained by fundamentals. This *funding shock channel* could presumably also explain a joint liquidity dry-up, although it has a harder time to rationalize why stocks in a weaker information environment would systematically be more affected than those with stronger public information. To address the possibility that return spillovers are explained by common funding shocks, we control for a rich set of proxies intended to capture common ownership and common flow shocks.[11]

Another explanation for a spillover-like return pattern concerns the activity of liquidity-providing arbitrageurs. Such arbitrageurs buy shares from distressed sellers and hedge their positions by selling peer stocks.[12] If they do so in droves and demand curves are downward-sloping, peer stocks could see a somewhat smaller price pressure effect themselves (Greenwood, 2005; Andrade et al., 2008; Lou et al., 2013). Such an effect naturally arises in models featuring risk-averse investors that trade correlated assets in equilibrium (and it is also present in our model; see Internet Appendix A). We deal with this *cross-asset hedging*

[11] These controls also help to counter the empirical concern that the peer effect could be driven by small-scale fire sales in disguise.

[12] Another possibility is front-running: when some arbitrageurs anticipate the fire sale, they can short-sell the fire sale stock and cover their shorts by buying from distressed funds (indirect evidence for front-running by hedge funds is documented in Chen et al., 2008). When arbitrageurs engaging in front-running want to hedge their positions, they may similarly sell peer stocks at the time of the fire sale.

channel in several ways. First, we construct a proxy for the intensity of cross-asset hedging that we use as a control in our empirical tests. Second, we note (and show in our model) that this explanation is inconsistent with the presence of a liquidity spillover effect (see also Cespa and Foucault, 2014) and does not predict cross-sectional differences in return spillovers across peers (assuming they are equally good for hedging). Finally, we conduct a placebo experiment by looking at another instance of price pressure—S&P 500 index addition events—for which there should be little uncertainty about the value implications for peer firms. Hence, any spillover that we document in this context cannot come from learning and—by comparing it to the spillover intensity in fire sales—allows us to assess the relative importance of cross-asset learning vis-à-vis cross-asset hedging.

Empirically, one key challenge is to distinguish spillover effects—where movements in one stock *cause* movements in another—from comovement driven by other unobserved factors like common economic trends. We argue that we can overcome this challenge by studying spillovers triggered by idiosyncratic fire sale shocks. One important concern, however, is *reverse causality*: it may be that fire sales, rather than causing spillover effects, are themselves caused by industry distress and the simultaneous stock price decline among industry stocks. While we defer a detailed discussion of this potential concern to the robustness section, we note here that the reverse causality story does not predict a swift return reversal as industry distress should arguably persist over several quarters if not years (e.g., Hoberg and Phillips, 2010b).

II. Data and Methodology

Stock market data is obtained from CRSP; mutual fund returns and monthly total net asset (TNA) values come from the CRSP mutual fund database; and quarterly mutual fund holdings are gathered from the Thomson Reuters S12 holdings data. We start from the sample of all common stocks (share codes 10 or 11) with an end-of-quarter price above one dollar and at least 10 non-missing daily returns in a quarter. For each stock, we calculate a measure of hypothetical selling pressure by "fire sale funds" as in Edmans et al. (2012). A detailed description of the construction of their measure is provided in Appendix B. Here, we only provide its intuition. Following their example, we exclude sector funds (third letter of CRSP objective code equal to "S")—as they could suffer from reverse causality—and drop all international, municipal, bond and metal funds (investment objective codes 1, 5, 6, 8). For each fire sale fund, defined as a mutual fund with quarterly outflows exceeding 5% of TNA, we calculate the imputed dollar selling volume for each portfolio stock if the fund had just downscaled his pre-existing portfolio. We then aggregate the imputed selling pressure of all fire sale funds at the stock level, scale by total trading volume, and call this variable *mfflow*. Following Edmans et al. (2012), we say that a fire sale event (defined at the stock-quarter level) occurs when *mfflow* is in the lowest decile.

It is important to note that this approach shuts down endogeneity concerns coming from the choice of stocks are being sold. Specifically, it could be that distressed mutual funds actively sell stocks about which they are the least optimistic—implying that their actual sell decisions contain information. The use of imputed sells circumvents this issue as imputed sells by construction do not reflect an active choice by the fund manager.

We identify the economic peers of fire sale stocks using the Text-based Network Industry Classification (TNIC) developed by Hoberg and Phillips (2010a; 2015). This data covers the period from 1996 to 2013 and is based on a textual analysis of the product description section contained in annual 10-K reports that must be filed with the SEC. For each year, Hoberg and Philips (2015) compute firm-by-firm pairwise similarity scores based on the number of words that two firms share in their product market descriptions. They then define two firms to be economic peers if their similarity score exceeds a pre-specified minimum threshold. Compared to standard industry classifications (such as SIC and NAICS), TNIC offers a finer and arguably more accurate description of peer firm relatedness.[13] This turns out to be important for our analysis—indeed, we confirm below that the spillovers to TNIC peers obtain even after controlling for common industry trends (see subsection III.C).

To ensure that any spillover effect we document is not confounded by another fire sale event, we do not consider a peer when it has itself experienced a fire sale in the preceding or succeeding 8 quarters. In addition, we focus on the 10 closest economic peers (based on the product similarity score) for each fire sale event as we expect cross-asset learning and thus potential spillovers to be the strongest for those firms.[14]

Fire sale events tend to cluster. For example, conditional on having a fire sale, a firm has a 61% (69%) probability of experiencing another fire sale over the subsequent four (eight) quarters, while unconditionally the probability of having a fire sale over a four (eight) quarter period is only 21% (30%). To deal with this clustering of fire sale events, we conduct a multivariate panel analysis that allows to isolate the return effects of overlapping fire sales.[15] Specifically, we run regressions of the following type:

$$y_{it} = \alpha_i + \alpha_t + \sum_{\tau=-16}^{16} \beta_\tau \times FS_{it-\tau} + \sum_{\tau=-16}^{16} \delta_\tau \times PEER_{it-\tau} + \gamma' X_{it-1} + \varepsilon_{it} \qquad (1)$$

where y_{it} is a dependent variable of interest, α_i and α_t are firm and quarter fixed effects, $FS_{it-\tau}$ and $PEER_{it-\tau}$ are a set of dummy variables that flag fire sale firms and their peers in

[13] In addition to being finer, TNIC data has three important features that make it more accurate than standard industry classifications: First, TNIC peer definitions are time-varying and can thus account for changes to the industry landscape. Second, by basing the classification on product descriptions (rather than, say, production processes), TNIC may be better able to capture product market competition where firms are exposed to common demand shocks. Finally, TNIC peer definitions are not forced to be transitive, meaning that each firm can have a different set of peers.

[14] We still find a statistically significant (albeit slightly weaker) spillover effect for returns when we include all peers (instead of only the top 10). This is not surprising as the majority of fire sale stocks has no more than a dozen of associated peer stocks. We nonetheless prefer to include this filter so as to not give a disproportionate weight to a small number of fire sale firms that are linked with hundreds of peer firms.

[15] In Internet Appendix B, we also report results from a classic event study approach. These results also exhibit an impact-reversal pattern for peer firms, but due to event clustering there is more pre-event drift and the reversal is more protracted.

event time, and X_{it-1} is a vector of pre-specified control variables. To see how this works, consider the case where firm A has a fire sale in the first quarter of 2008, implying that $FS_{A2008Q1} = 1$. If firm B is a peer to fire sale stock A (and does not have a fire sale itself), then $PEER_{B2008Q1} = 1$. The specification further includes 32 dummies that flag the 16 preceding and succeeding quarters for the two event firms. For example, the dummies $FS_{A2008Q1-1}$ and $PEER_{B2008Q1-1}$ take the value one in the fourth quarter of 2007 for firm A and B, respectively. Importantly, if firm A had another fire sale in, say, the first quarter of 2007, then $FS_{A2008Q1}$ and $FS_{A2007Q1+4}$ would be one at the same time, ensuring that any reversal from the preceding fire sale does not confound the estimation of the second fire sale effect. In this way, our panel specification allows us to isolate the evolution in y_{it} for fire sale and peer events in event-time. Standard errors are double-clustered at the firm and quarter level.

For our multivariate analyses, we gather a host of firm-specific control variables from a variety of sources: accounting data comes from Compustat; the number of analysts following a stock is taken from I/B/E/S; institutional holdings data are from CDS Spectrum (S34); and quarterly measures of the probability of informed trading (PIN; Easley et al., 1996) are downloaded from Professor Stephen Brown's website.[16] Table I reports descriptive statistics and Appendix A provides detailed variable descriptions for the control variables used in this study. Our final dataset spans the period from 1996 to 2013 and includes 31,403 fire sale events as well as 66,696 associated peer events. Figure 1 shows how these events spread out over time. While the number of events fluctuates quite a bit, there is no apparent trend or an indication that events are concentrated in one particular period.

[Include Table I about here.]

[16] Available at: http://scholar.rhsmith.umd.edu/sbrown/pin-data. These PIN measures are estimated using the Venter and de Jongh (2004) model.

III. Return Spillover

A. Baseline Results

In this section, we study the effect of fire sales on the stock returns of their peers. Specifically, Table II shows the results from estimating equation (1) for the cumulated quarterly return as the dependent variable. For each specification, we show fire sale and peer event-time dummies next to each other to facilitate the comparison.[17] First, we note that the fire sale dummies display the typical impact-reversal pattern. In the fire sale quarter, affected stocks shed 7-8% of their value, which they partly recover over the subsequent 8 quarters. The magnitude of this effect is close to what has been found in the literature (Coval and Stafford, 2007; Edmans et al., 2012; Dessaint et al., 2016). It is also remarkably consistent across different specifications, showing that the results obtain after controlling for a host of accounting variables (column 2), ownership measures (column 3), fund flow proxies (column 4), or all of these combined (column 6). The key result of this table is that the dummy for peer firms in the event quarter (t = 0) indicates a drop in returns of about 1.5%. This amounts to approximately one fifth of the original fire sale effect (e.g., in column 1, ~1.5%/7.5%), which is a reasonable magnitude for a spillover effect.[18] Like the fire sale effect, this drop in peer returns remains stable and highly statistically significant across specifications. We further find that this return spillover completely reverses within four quarters.[19] For example, in column 1, the cumulated reversal over four quarters equals 1.6%

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[17] For brevity, we only report results for event-time dummies $-2 \leq \tau \leq 8$. The other event-time dummies are mostly insignificant.

[18] When observing a drop in the stock price of a peer firm, investors will not be sure whether this price drop reflects fundamentals or noise. For mixed prior beliefs about the unconditional probabilities of fundamental and non-fundamental shocks, it is natural to expect an update which is a fraction of the original price shock.

[19] Interestingly, the reversal for peer firms occurs somewhat faster than the reversal for fire sale stocks, as the latter have not fully reversed after even 8 quarters. While explaining the slow reversal for fire sale firms is outside the scope of this paper, we note that our finding that the peer effect reverts faster is consistent with recent explanations that focus on the role of adverse selection risk surrounding fire sales (Dow and Han, 2016; Ringgenberg et al., 2016). According to this argument, there is substantial uncertainty about which stocks are sold by fire sale funds: on the one

and is significant at the 5%-level. The existence of the reversal confirms that the stock price drop for peer firms is not caused by fundamental news. Rather, it suggests that investors become aware of the non-fundamental reason for the price drop in the fire sale stock and reevaluate their initial negative assessment for peer firms.

[Include Table II about here.]

We emphasize that the return spillover effect obtains after controlling for an array of potentially confounding factors. The inclusion of firm and quarter fixed effects, for instance, ensures that our results are not driven by unobserved (fixed) firm characteristics or market-wide trends. Nor is the effect explained by standard accounting controls, analyst coverage or institutional ownership.[20] Given our identifying assumption that fire sales occur for reasons outside of affected firms, it is actually reassuring to observe that the return spillover effect is unaffected by the inclusion of these controls. Finally, we note that both the spillover and reversal are robust to controlling for the mutual fund selling pressure in peer firms (columns 4-6).[21] This suggests that the return spillover we document is not driven by peer firms experiencing distressed selling themselves, a point which we belabor further in the robustness section.

One slightly worrying aspect of Table II is that returns of fire sale stocks already show a small but significant reduction one quarter prior to the fire sale event. This could be indicative of reverse causality: some stocks experience distress and this makes investors to pull out of

hand, fire sale funds may sell their most liquid positions to mitigate price impact (in which case trading on the reversal should be profitable). On the other hand, they may sell stocks about which they are particularly pessimistic (in which case the reversal may never materialize). Given this uncertainty, investors may be reluctant to bid up the fire sale stock even after they become aware of the fire sale, thereby explaining why the price reversal for fire sale stocks is more protracted than the one for peer stocks.

[20] The coefficient estimates for these control variables mostly have the expected sign: small firms, more-levered firms, firms with fewer analysts, and firms with a lower market-to-book ratio have higher quarterly returns.

[21] The coefficient on the *mfflow* variable is significantly positive as expected, suggesting that higher mutual fund selling pressure (i.e., a more negative *mfflow*) triggers lower returns. The flow measure for non-fire sale mutual funds (*mfflow_complement*) is not significant.

funds heavily invested in these stocks. While we tackle this concern in the robustness section, we acknowledge that it is difficult to rule this out completely. We note, however, that reverse causality cannot explain the entirety of our findings. In particular, it is hard to explain the return reversal without resorting to price pressures triggered by fire-selling mutual funds. Thus, even if some fire sales have been caused by negative fundamentals, the fire sale events themselves cause an impact-reversal pattern, which we show to be spilling over to peer firms (that do not experience a fire sale themselves). In other words, potential endogeneity concerns notwithstanding, the fact that we observe a return shock and its *reversal* for both fire sale stocks and their peers constitutes strong evidence in favor of a spillover mechanism.

B. Cross-sectional Tests

In this subsection, we provide results for two types of cross-sectional tests. First, we study whether the return spillover effect is stronger for peers for which alternative sources of public information are scarce—i.e., when a rational learner would need to place a larger weight on the fire sale stock. Second, we examine whether the spillover is stronger when the peer is more closely related to the fire sale stock—i.e., when the stock price of the fire sale firm should be a more informative signal. For brevity, we drop all observations of fire sale firms from the analysis as our focus is on how the spillover effect is mediated by peer characteristics (more precisely, we drop all firm-quarter observations surrounding fire sale events within 8 quarters).[22]

B.1. Sample Splits by Firm Characteristics

Panel A of Table III shows results for sample splits based on several proxies of a stock's information quality. In our first test, reported in columns 1 and 2, we split peer firms by their size (measured by total assets). The literature routinely finds that small stocks are less

[22] When we do not drop fire sale firms, we find that some firm characteristics—in particular the absence of an investment grade credit rating—are associated with a stronger fire sale effect. Our results for peer firms are unaffected by including fire sale stocks.

efficient and more often mispriced (Lee et al., 1991; Hong et al., 2000; Hou and Moskowitz, 2005). In addition, big stocks are known to lead small stocks in terms of price discovery (e.g., Lo and MacKinlay, 1990; and Hou, 2007). Thus, when conditioning on publicly available prices, investors of small firms should put a lower weight on their own stock and a higher weight on other stocks. As such, small stocks should respond more strongly to a fire sale hitting one of its peers. The results confirm this intuition: at 2.4%, the spillover effect for small peers is almost twice as large as the one for large peers (1.3%). As shown at the bottom of the table, this difference is statistically significant at the 5% level.

[Include Table III about here.]

Next, we investigate the effect of having an investment grade credit rating. Rating agencies have been found to provide valuable information for stock market investors (Holthausen and Leftwich, 1986) and firms with an investment grade rating should thus be deemed safer than those with a speculative grade rating or no rating at all. We therefore expect a lower return spillover effect for investment grade firms. Columns 3 and 4 of Table III, Panel A indeed show that the spillover effect for non-investment grade firms (i.e., unrated or speculative grade firms) is more than three times larger than the one for investment grade ones—a highly significant difference. In columns 5 and 6, we split peer firms by S&P 500 index membership. Index members are widely recognized and receive more attention by the public media (Chang et al., 2014), which should make their prices more efficient. Consistent with this intuition, we find that the return spillover for S&P 500 members is only half as large as for non-members. This difference is again statistically significant.

Finally, we use financial analyst data to measure a stock's information environment more directly. We start by splitting the sample based on the number of analysts following a firm. The literature finds that analysts provide valuable information to investors and reduce information asymmetry in the market (Brennan and Subrahmanyam, 1995; Womack, 1996; Barber et al., 2001; Gleason and Lee, 2003; Loh and Stulz, 2011; Kelly and Ljungqvist, 2012).

Consistently, we find that the return spillover effect is more than twice as large for peer stocks with below-median analyst following (column 7) compared to those with above-median analyst following (column 8). With a *t*-statistic of 1.9, this difference is marginally significant. For our last test, we compute stocks' average (absolute) forecast error (AFE) based on one-year-ahead EPS forecasts over the previous five years. The idea is that stocks with a low AFE have more precise public information and investors should thus place a lower weight on stock prices of their peers (Dessaint et al., 2016). The results shown in columns 9 and 10 confirm this intuition: whereas the spillover effect for stocks with low AFE is 1.2%, it rises to 2.4% for stocks with above-median AFE—again a statistically significant difference.

B.2. Cross-sectional Tests by Relationship Characteristics

In Panel B of Table III, we investigate whether the spillover is stronger for *closer* peers of fire sale firms, or for peers connected to more severe fire sales. To this end, we estimate equation (1) after categorizing peer stocks into groups based on different relationship characteristics between them and their respective fire sale stocks, and separately including different sets of event-time dummy variables for each peer category.[23] We begin by examining whether the return spillover effect is stronger for peers of fire sale firms that experience a larger return drop in the quarter of the fire sale. Specification 1 of Panel B shows that, as expected, the return spillover is larger and highly statistically significant for more severe fire sales, whereas it is insignificant for less severe ones.[24]

[23] We have to proceed in this way rather than conducting sample splits because the fire sale-peer relationship characteristics are only defined for the peers of fire sale firms and not for the control stocks.

[24] Less severe fire sales exhibit a return of (only) -1% on average; it is thus not surprising that the return spillover onto peers is not significant for this group.

The second relationship characteristic we consider is the similarity score assigned to each firm pair in the TNIC data, which allows us to group peers according to their proximity to the fire sale firm. A higher similarity score means that the peer is a closer rival of the fire sale firm in the sense that they offer more similar products, and we thus expect the return spillover effect to be stronger in this case. The results shown in specification 2 of Panel B confirm this intuition: peers with an above-median similarity score exhibit a price spillover that is three times as strong as the one for peers with below-median similarity score. The difference is strongly statistically significant.

Next, we investigate whether the spillover effect varies with the number of peers of a given fire sale firm. The idea is that the average spillover effect may be weaker when there are many peers, because then the price drop of the fire sale firm is only one out of many available price signals. The results for specification 3 in Panel B are in line with this expectation. For fire sales with ten or more peers, the average spillover effect is 1.7%, compared to 2.1% for fire sales with less than ten peers. This difference is, however, not statistically significant.

Our results so far show that, on average, negative returns for fire sale firms are considered bad news for their peers. In our third cross-sectional test, we study whether there is variation among peers along this dimension. To this end, we estimate return correlations between fire sale firms and their peers (using 8 quarters of daily return data prior to the fire sale). Intuitively, we expect the return spillover effect to be stronger for peers that have a higher return correlation with the fire sale stock. As shown in specification 4 of Panel B, this intuition is clearly borne out in the data: peers with an above-median return correlation to the fire sale stock see a spillover effect of 2.8%, whereas peers with below-median return correlation only see a price drop of 1.2%. This difference is statistically significant. In principle, bad news about the fire sale stock could even be good news for some peers. We therefore explore the relation between return spillovers and return correlations in more detail. Specifically, in Figure 3, we report the estimated return spillover effects that we obtain when we group peers into quintiles. In the lowest quintile, the spillover effect is still negative, but close to zero and statistically insignificant. In this group, the average return correlation is low but still positive. It thus seems to be difficult to identify peer firms which unconditionally benefit from bad news to one of its product-market competitors. Figure 3 further shows that the return spillover effect rises (almost) monotonically across quintiles. Hence, the stronger the prior return correlation, the stronger the return spillover from the fire sale stock onto its peer.

In our last cross-sectional test, we examine whether the return spillover effect is stronger for peer stocks with a larger turnover-volatility correlation. Banerjee (2011) shows that this correlation may serve as a proxy for the extent to which investors condition on prices. In his model, a low correlation between share turnover and volatility is consistent with investors being oblivious to information contained in prices, whereas a high correlation obtains when investors condition on prices. We therefore group peers based on the turnover-volatility correlation (estimated using daily stock market data over the 8 quarters prior to the fire sale). As predicted by Banerjee (2011), we find that the return spillover effect is significantly larger for peer stocks with an above-median turnover-volatility correlation.

In summary, this subsection shows that return spillovers are stronger (1) for peers whose own prices are less efficient and (2) for peers that are more closely related to fire sale firms. Overall, these results offer strong support for the information spillover hypothesis. Finally, we acknowledge that there is little evidence that stocks with a larger spillover also display a stronger return reversal. This is likely explained by a loss in statistical power, as returns cumulated over a 1-2 year period tend to be quite volatile. We note, however, that while being statistically insignificant, reversals are typically of the same economic magnitude than those found in Table II and we can never reject the hypothesis of there being a full reversal within four quarters (unreported).

C. Robustness

In this subsection, we examine the robustness of the return spillover. Because many of the control variables used below cannot be defined for fire sale stocks, we exclude all stock-quarter observations within eight quarters of a fire sale. Consequently, we also drop the fire sale dummies from the regressions. The results are shown in Table IV. For comparison, we report in specification 1 of this table the return spillover effect that obtains in this setting. Not surprisingly, the results closely resemble those reported in Table II.[25]

[Include Table IV about here.]

The first alternative explanation we consider is liquidity provision. Even in a world without asymmetric information, price pressure effects arise when market makers are averse to deviating from their target inventory (e.g., Ho and Stoll, 1981; Grossman and Miller, 1988). When there is a drop in stock prices due to a fire sale, arbitrageurs have an incentive to provide liquidity to the fire-selling funds, and they may want to hedge their positions by selling peer stocks. If enough arbitrageurs hedge their exposure to fire sale stocks, this could explain why peer stocks also see a small price pressure effect themselves. Our first argument against this alternative explanation draws on the rational learning model that we present in Internet Appendix A. Specifically, we show there that, while both cross-asset learning and cross-asset hedging give rise to price spillovers, only cross-asset learning can also explain the presence of a simultaneous liquidity spillover.[26] Thus, evidence for liquidity spillovers to peer stocks—which we present in subsection IV.C below—favors our learning interpretation over cross-asset hedging.

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[25] The only difference is that the return reversal is now only marginally significant. However, this is solely explained from the loss in power that comes with the reduction of the sample size (due to the exclusion of all stock-quarter observations surrounding fire sales) as, in terms of economic magnitude, the return reversal continues to completely offset the return spillover effect.

[26] In Cespa and Foucault (2014), hedging by cross-market arbitrageurs even dampens the liquidity spillover as these arbitrageurs absorb part of the selling pressure by distressed funds, thereby mitigating the shock to the price informativeness of the fire sale stock.

We now employ two proxies to deal with this concern empirically. Our first proxy is designed to capture liquidity provision by current owners of peer stocks. Indeed, these investors are natural liquidity providers to fire-selling funds as they can buy from them at fire sale prices and hedge their purchases by selling peer stocks without needing to sell short—a trade that promises to return the fire sale discount in expectation.[27] The *liquidity provision* proxy measures the extent by which current peer stock owners enter this arbitrage trade. Specifically, for each stock, we calculate the minimum of the dollar selling volume by its current owners and their corresponding buy volume in fire sale stocks, and scale this by the stock's market capitalization. Our second proxy is short interest, i.e. the number of shares on loan as a fraction of the number of shares outstanding, and captures liquidity provision by non-owners of peers stocks. Specifications 2 and 3 of Table IV show the results when we include these proxies as control variables. In both cases, we obtain significantly negative coefficients, consistent with cross-asset hedging having some price impact: more liquidity provision by current owners or short sellers is associated with lower returns for peer stocks. Nonetheless, the return spillover effect (i.e., the coefficient of the $PEER$ dummy for $t = 0$) is hardly affected and remains strongly significant.[28] Hence, the drop in the stock price observed for peer firms does not seem to be explained by liquidity provision.[29]

Next, we explore whether peer firms themselves experience mutual fund selling pressure which causes the impact-reversal pattern in their stock returns. Note, however, that this selling pressure could not have been very large, as we require a peer firm *not* to have had a

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[27] In addition to saving the short-lending fee, they may also be more informed about peer stocks compared to other potential liquidity providers, enabling them to guess better the non-fundamental nature of the fire sale shock.

[28] The liquidity provision proxy has a median of 0 and never exceeds 1‰. Thus, it appears as if the current owners of peer stocks do not provide much liquidity to fire sale funds, explaining why the price spillover effect is virtually unchanged when we include this control.

[29] The return spillover effect is unlikely to be explained by liquidity providers' short-selling activity for yet another reason. Indeed, if the return spillover comes from the selling pressure induced by short sales, we would expect it to be stronger for peers that are easy to short. In fact, we find the opposite since the return spillover effect is weaker for large stocks and stocks that are member of the S&P 500 (e.g., Saffi and Sigurdson, 2011).

fire sale itself within eight quarters. Nevertheless, since the impact-reversal pattern for peer firms is only one fifth of the fire sale effect, it is conceivable that it was triggered by a small-scale fire sale. In our main specification from Table II, we deal with this concern by including a stock's own *mfflow* as a control variable. The *mfflow* measure turns out to be non-normal and highly skewed, however (see Table I). As a robustness check, we therefore replace it by a set of dummy variables that flag different *mfflow* deciles. In different tests, we also control for the fraction of the stock owned by fire sale funds (labeled *fire sale stock share*) and for the portfolio fraction of fire sale stocks held by the mutual funds owning the stock (labeled *fire sale fund share*). Specifications 4 to 6 of Table IV report that the price spillover effect is not affected by any of these changes. We conclude that it is unlikely that the impact-reversal pattern for peer firms is due to forced selling by distressed mutual funds.

We next discuss the possibility that the return spillover effect is explained by a corporate investment channel. Indeed, Dessaint et al. (2016) find that peer firms temporarily curb their investment around mutual fund fire sales. If this cut in investment is tracked or anticipated by investors, this could explain why they discount the shares. To mitigate this concern, we directly control for peer firms' capital expenditures (scaled by the stock of PPE at the end of the previous quarter), obtained from the Compustat quarterly files, in column 7. While the capex control garners a negative significant coefficient (suggesting that higher capex leads to lower returns), its economic magnitude is small, explaining why the return spillover effect is not much affected. This rules out the corporate investment channel as an alternative explanation.

Next, we consider reverse causality: it could be that negative fundamentals about an industry trigger outflows from mutual funds heavily invested in that industry, which forces them to liquidate part of their assets at fire sale prices. The worry is that the drop in returns for peer firms reflects the negative fundamentals instead of being caused by an information spillover channel like we claim. As noted above, the quick reversal of the peer effect is clearly inconsistent with this explanation. We now strengthen this conclusion by showing that the return spillover effect is robust to controlling for industry trends through the inclusion of industry-quarter fixed effects.[30] The results in the last column of Table IV confirm that the impact-reversal pattern for both fire-sale stocks and their close economic peers is hardly affected by this change. We conclude that the return spillover result is not explained by industry distress.

Finally, we verify that the return spillover result is robust to measuring returns in different ways. Note first that, although we use raw returns for our main spillover tests in Table II, the inclusion of time fixed effects means that we are always neutralizing general market trends. In other words, it is effectively as if we were using market-adjusted returns. In column 8 of Table IV, we further show that the spillover effect survives the inclusion of industry-time fixed effects. This implies that the spillover effect is robust to using industry-adjusted returns. In Internet Appendix C, we confirm that we get very similar results if we use different variants of risk-adjusted returns: benchmark-adjusted returns as recommended by Daniel et al. (1997), CAPM-alphas, Fama and French (1993) 3-factor alphas, Carhart (1997) 4-factor alpha, or Fama and French (2014) 5-factor alphas. Indeed, for all these measures, we obtain significant fire sale and peer spillover effects that revert over time.[31] Thus, common risk factors do not explain the impact-reversal patterns that we observe.

IV. Additional Evidence

A. Placebo

The "learning channel" explanation for the return spillover of fire sales relies on the presence of uncertainty: investors cannot be sure that the price decline in a fire sale stock is not due

[30] We use the Fama-French 48 industry classification.

[31] For Carhart 4-factor and Fama-French 5-factor alphas, the return reversal for peer firms fails to be statistically significant. However, economically the reversal almost fully offsets the peer spillover effect and we can never reject the null that there was a complete return reversal.

to fundamentals and therefore discount its peer firms. In other words, if we were to identify price pressure effects whose causes are well understood by the market, there should be no learning and thus no learning-based spillover. We argue that S&P 500 index additions are ideally suited for this type of placebo experiment.[32] Indeed, the literature finds that stocks that are announced to become a member of the S&P 500 index experience a strong run-up in returns (Harris and Gurel, 1986; Shleifer, 1986; Beneish and Whaley, 1996; Lynch and Mendenhall, 1997; Chen et al., 2004), commonly attributed to the forced buying by passive index funds tracking the S&P 500.[33] While there is no agreement in the literature as to whether this run-up completely or only partially reverses after the addition becomes effective,[34] the crucial feature for us is that the public announcement of the addition should remove any uncertainty regarding the value implications for peer firms. As such, we don't expect a return spillover due to cross-asset learning for S&P 500 index addition events, even though the run-up in returns is almost as large in (absolute) magnitude as the fire sale effect.

To identify the inclusion effect as well as any potential spillover, we run panel regressions similar to specification (1) but at daily frequency and where the fire sale dummies are replaced by "addition (AD) dummies" that flag the days surrounding an index addition event, defined as the day when a stock's addition to the S&P 500 index becomes effective according to the Compustat index constituents database. Our sample includes 247 index addition

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[32] We focus on index additions rather than index deletions because the latter are often confounded by corporate events such as mergers, takeovers, spinoffs, or immanent bankruptcies (Barberis et al., 2005; Patel and Welch, 2016) and tend to be associated with smaller or even insignificant price effects (Lynch and Mendenhall, 1997; Chen et al., 2004; Patel and Welch, 2016).

[33] Consistent with this interpretation, the run-up in returns has been increasing over time concomitant to the growth of passive investment.

[34] It is thus not clear whether the run-up constitutes a pure price pressure effect or also something else. For instance, Denis et al. (2003) show that newly added stocks see a rise in analysts' earnings forecasts as well as realized earnings and Chen et al. (2004) document evidence of increasing investor awareness in line with the Merton (1987) model. The literature agrees, however, that price pressure is part of the explanation (see, for instance, Lynch and Mendenhall, 1997; Chen et al., 2004, and Chang et al., 2014). Moreover, as documented by Patel and Welch (2016), index additions post 2000 have seen stronger price reversals and are thus more consistent with a price pressure effect.

events and 2,502 corresponding peer events over the sample period 1996 to 2013.[35] The peer dummies now flag the economic peers of newly added stocks in event-time and we employ the same battery of controls from before. All regressions include firm and day fixed effects and standard errors are double-clustered at the firm and day level.[36]

[Include Table V about here.]

The results are reported in Table V and visualized in Figure 4. For the added stocks, we find a statistically significant and economically sizable run-up in returns setting in about five days prior to the effective index addition. This is consistent with previous literature (Beneish and Whaley, 1996; Lynch and Mendenhall, 1997; Chen et al., 2004) and reflects the fact that S&P typically announces the index change roughly five days before it becomes effective (Beneish and Whaley, 1996). Column 1 shows, for instance, that added stocks rise by 5.6% over the eight trading days before the effective date of the addition (t=0) and see their returns partly reversed thereafter (see also Figure 4, Panel A). Looking at the peers of added stocks, we find that there is only a weak spillover of this price pressure effect. For instance, in the specification without controls (column 1), peer stocks have an insignificant cumulated abnormal return of only 0.5% over the eight days before the addition (see also Figure 4, Panel B).[37] When all controls are added (column 6), this figure remains small and insignificant. Economically, the spillover to peers is less than 10% when compared to the size of the addition effect, which contrasts with a spillover of about 20% that we found for fire sales (see Section III.A). This suggests that roughly one half of the return spillover for

[35] We again focus on the top ten peers excluding all firms that become S&P 500 index members themselves within one year of the respective addition event.

[36] In the Internet Appendix, we report similar results using an event study methodology.

[37] If anything, Figure 4, Panel B, shows slowly increasing returns for peer stocks *after* the addition event. This may reflect the existence of a common upward trend underlying all stocks in that industry. After all, stocks that are added to the S&P 500 have been growing in the past and this may be also true for their peers.

mutual fund fire sales is due to cross-asset learning, whereas the other half is likely explained by cross-asset hedging (and is thus present in both settings). We therefore establish that information spillovers contribute to return comovements over and above what can be explained by cross-asset liquidity provision alone.

B. Feedback

We found above that the return spillover effect is stronger for peer firms with low information quality. A strong spillover effect, in turn, will increase concerns that the selling pressure for fire sale stocks is due to bad information rather than noise. In other words, cross-asset learning predicts that the spillover effect *feeds back* to fire sale firms. While the joint determinacy makes such feedback effects difficult to identify, this subsection tests for their existence indirectly by looking at whether peer characteristics affect how much fire sale stocks drop in response to mutual funds' selling pressure. Specifically, we focus on the sample of fire sale events and run regressions of the type:

$$y_{it} = \alpha_t + \beta \times \overline{PEER\ Characteristics}_{it} + \gamma' X_{it-1} + \varepsilon_{it} \qquad (2)$$

where y_{it} is the return of fire sale stock i in quarter t, $\overline{PEER\ Characteristics}_{it}$ is a measure of peer characteristics averaged over the ten closest peers of firm i, and X_{it-1} is the same vector of control variables already used for specification (1) above. We include quarter fixed effects but not firm fixed effects as the latter would throw away all the meaningful variation in (persistent) peer characteristics across event firms. Standard errors are again double clustered at the firm and quarter level.

As peer characteristics, we consider the same five dummy variables that we used for sample splits reported in section III.B—above-median firm size, investment grade rating, S&P 500 membership, above-median analyst coverage, and below-median average forecast error. In addition, we consider a composite "information index" that is defined as the mean of these five dummy variables for a given peer stock. Since the characteristics of closer economic peers should matter more, we calculate weighted averages of peer characteristics across the ten closest peers based on the TNIC similarity scores.[38] For each characteristic, we run two regressions—one in which we control for the continuous *mfflow* measure and one in which we replace it by decile dummies to allow for a non-linear relationship between returns and mutual funds' selling pressure.

[Include Table VI about here.]

Since there should be less feedback from peers with more informative prices, we expect fire sales of such peers to exhibit a smaller (i.e., less negative) drop in price, implying a positive β coefficient. The results in Table VI confirm this prediction: β is positive across all specifications. It is also at least marginally significant for three out of the five information quality proxies (the exceptions are analyst coverage and average forecast error, for which the t-statistics are between 1.3 and 1.5). The coefficient estimate in column 1, for example, implies that the price drop for fire sale firms is lower by 1.5% when all peers have above-median size. For the information quality index, the effect rises to a strongly significant 2.1%, suggesting that the index summarizes the different information proxies in a meaningful way. Compared to the unconditional price drop of roughly 6.8% (see Table II, column 6), the fire sale effect is thus about 31% lower for firms with informative peers. These results are consistent with a feedback effect as hypothesized by cross-asset learning models (e.g., Cespa and Foucault, 2014).

C. Liquidity Spillovers

To the extent that fire sales reduce the price informativeness of fire sale stocks, models with learning from prices predict that peer stocks should see a deterioration in liquidity (e.g.,

[38] Using equal weights gives similar albeit slightly weaker results.

Admati, 1985; Cespa and Foucault, 2014). Such a decrease in the price informativeness of fire sale stocks could occur for several reasons. First, the selling pressure by fire sale funds may lead to the perception of higher noise trader risk, for which risk-averse market makers would demand higher compensation (e.g., Ho and Stoll, 1981; Grossman and Miller, 1988). Second, when there is uncertainty about whether informed traders are present, a large unexpected fire sale may lead to an update of this probability, causing market makers to increase price impact to protect themselves against the perceived increase in adverse selection (e.g., Easley and O'Hara, 1992). Finally, it is possible that the price drop in fire sale stocks hurts informed arbitrageurs, who in response trade less aggressively, thereby rendering the stock price less efficient (Dow and Han, 2016). Whatever the cause, once price informativeness falls, liquidity providers in peer stocks face higher uncertainty and respond by curbing their liquidity provision. Importantly, this liquidity spillover is a side effect of cross-asset learning—alternative spillover channels such as cross-asset hedging do not make such a prediction.[39]

[Include Table VII about here.]

In this subsection we test whether mutual fund selling pressure hurts the liquidity of fire sale stocks and their peers. To this end, we estimate equation (1) for four different liquidity proxies: bid-ask spreads, the logarithm of the Amihud illiquidity ratio (Amihud, 2002), the probability of informed trading (PIN, Easley et al., 1996), and share turnover. Table VII, Panels A to D, show the results. The first thing to notice is that there is strong evidence for a dry-up in liquidity for fire sale firms with all four liquidity measures. For instance, bid-ask spreads go up by roughly 15-18 basis points (Panel A), representing an increase of 10% relative to the unconditional mean, and remain elevated for about four quarters after the fire sale. For PIN, the increase is smaller with about 4-5% (Panel C) but still statistically

[39] See our model in Internet Appendix A for more discussion on this point.

significant. For the logarithm of Amihud (Panel B) and share turnover (Panel D), the decrease in liquidity is even larger, but we acknowledge that these results have a mechanical touch to them, as fire sale events are defined as events where funds' selling pressure is large relative to the stock's trading volume (see Appendix B). Overall, the evidence for a deterioration in liquidity is nonetheless overwhelming.

Table VII also shows that the dry-up in liquidity spills over to the economic peers of fire sale firms. Indeed, the event-time dummy for peer firms is at least marginally significant for all four measures of liquidity in the quarter of the fire sale. In terms of magnitude, the liquidity spillover represents between one tenth (for turnover) to one third (for bid-ask spreads) of the original fire sale effect. All these effects disappear after at most two quarters (not shown for brevity), proving that the shock to the liquidity of peer stocks is only temporary. These results are consistent with models of rational learning, which predict that market makers react to the decreased price informativeness of the fire sale stock by curbing back their liquidity provision in peer firms. Indeed, we demonstrate in Internet Appendix A that such a liquidity spillover effect is a unique prediction of cross-asset learning and cannot be explained by alternative spillover channels such as the hedging activity by liquidity-providing arbitrageurs.

V. Conclusion

In this paper we test and confirm a basic tenet of the large literature on trading under asymmetric information—the assumption that investors can and do learn from prices. We test this conjecture in the context of mutual fund fire sales, which have been found to trigger substantial price pressure effects (Coval and Stafford, 2007). We argue that, when the fire sale occurs, investors are at first unsure whether the price decline is caused by forced selling or negative news about fundamentals. Thus, if investors learn from prices, they should update their expectations of close economic peers. Over time, the non-fundamental nature of the price decline becomes apparent and investors return to their initial expectations. Consistent with this learning channel, we find that the impact-reversal pattern for fire sale stocks spills over onto the stock prices of economic peers. It is precisely the non-fundamental nature of the fire sale shock that helps our identification, as it ensures that this return spillover onto peer firms cannot be explained by investors reacting to new information common to many stocks.

Additional results corroborate the learning channel interpretation. First, the return spillover effect is stronger (1) for peers in a weaker information environment (i.e., smaller stocks, unrated stocks, stocks with fewer analysts, and stocks with larger forecast errors) and (2) for peers that are more closely related to the fire sale firm (i.e., peers with a higher TNIC similarity score or a higher return correlation)—consistent with rational learners placing a larger weight on the stock price of fire sale firms in such cases. Second, we show that another type of price pressure—the S&P 500 index addition effect—leads to a weaker return spillover, consistent with cross-asset learning being less important when the ultimate cause of the price pressure is widely understood by market participants. Finally, we find evidence of a liquidity spillover to peer firms and show that peer characteristics mediate the severity of fire sale shocks. These findings support recent theory showing how cross-asset learning leads to an interdependence of the informational efficiency across stocks (Cespa and Foucault, 2014).

Apart from confirming the long-held but hitherto untested assumption regarding learning from prices, our results have broader implications for our understanding of return and liquidity comovements in the stock market. They show that, as investors try to solve the massive filtering problem posed by a stock market in which every price is a potential signal for any other, they occasionally make mistakes and update on noise. Thus, the very fact that investors engage in cross-asset learning causes spillover effects that contribute to the documented comovement in returns and liquidity (e.g., Pindyck and Rotemberg, 1993;

Chordia et al., 2000). Future research on the sources of commonalities in returns and liquidity should take this cross-asset learning channel into account.

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Figure 1: Number of Fire Sale and Peer Events over time

This figure shows the number of fire sale and peer events over our sample period from 1996 to 2013. Fire sale events are defined as in Edmans et al. (2012) [and explained in Appendix B]. For each fire sale event, we define as peer events the ten closest economic peers (according to the TNIC similarity score developed by Hoberg and Philips, 2010a, 2015) that are not undergoing a fire sale themselves in the preceding or succeeding eight quarters.

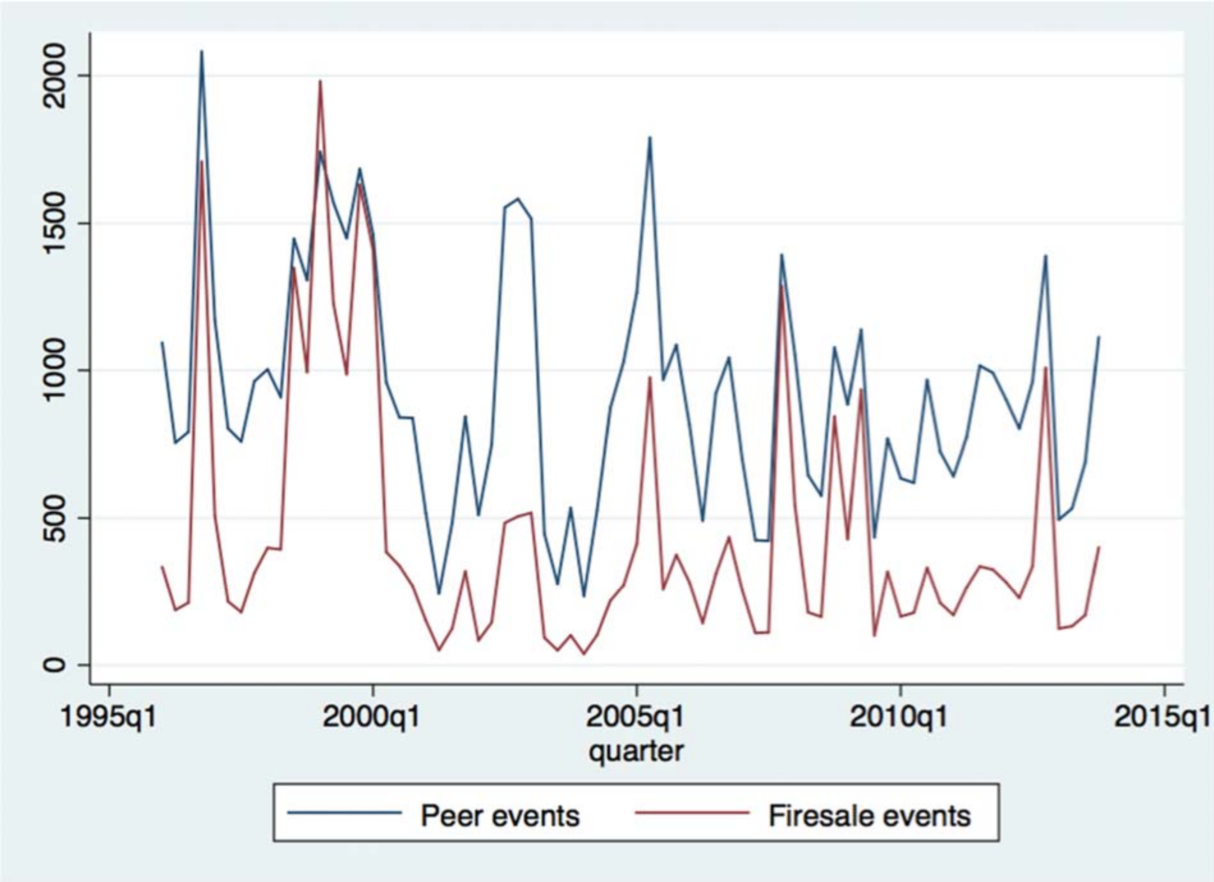
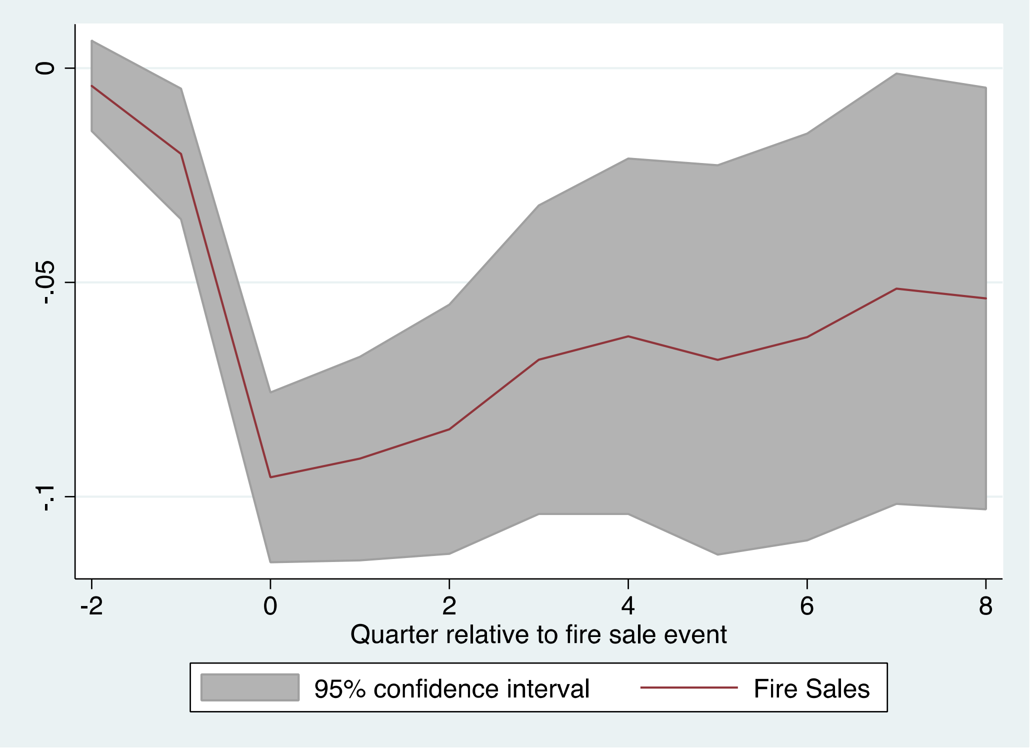


Figure 2: Event-time Returns for Fire Sale and Peer Firms

This figure shows returns for fire sale firms (Panel A) and peer firms (Panel B) in event-time (where 0 is the quarter of the fire sale). These graphs are based on the cumulated coefficient estimates of the fire sale and peer dummies shown in Table II, column 1. The grey band around the cumulated returns represents the 95%-confidence interval.

Panel A: Fire Sale Firms



Panel B: Peer Firms

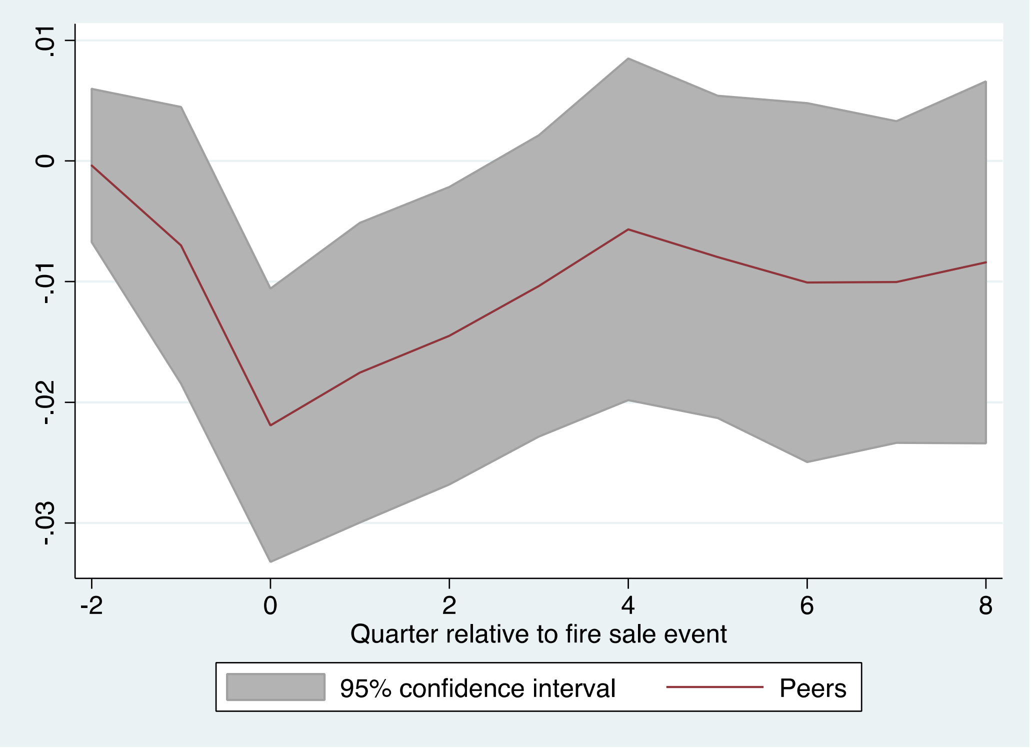


Figure 3: Return Spillover Effect for Peer Firms by Return Correlation

This figure shows the return spillover effect for quintiles of peer firms grouped by their prior return correlation with the fire sale stock. Return correlations are computed using daily stock returns over the 8 quarters prior to the fire sale. The spillover effects are then obtained by estimating equation (1) after replacing the peer dummy variable with five dummy variables for each return correlation quintile. The red lines represent the 95%-confidence intervals based on the standard errors of these estimated dummy coefficients.

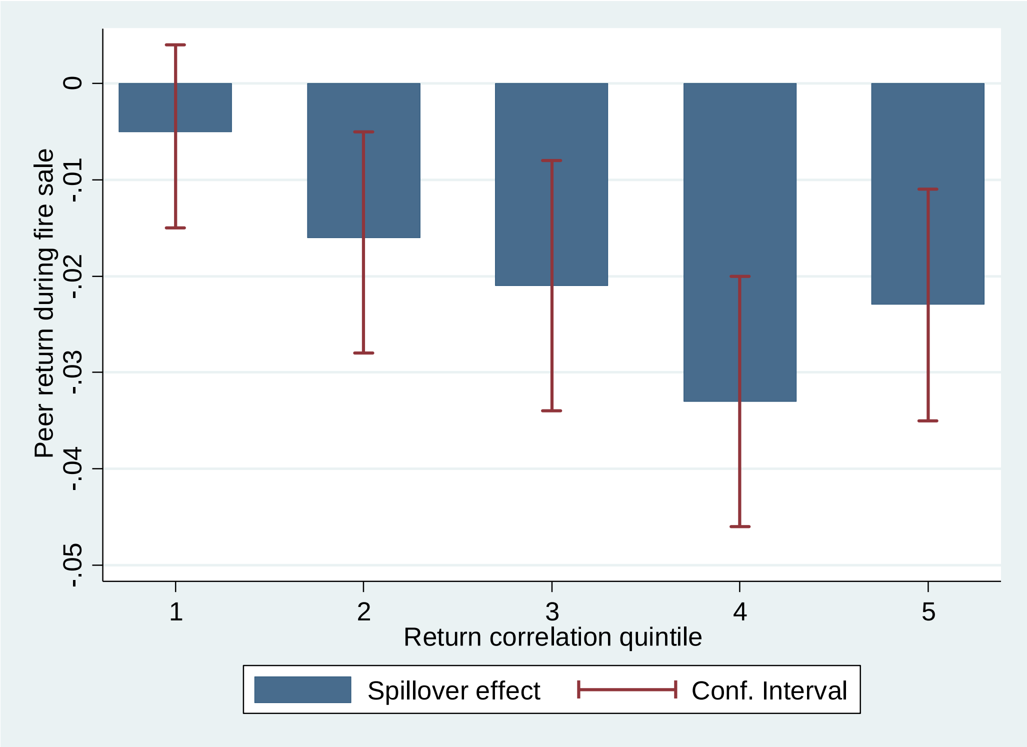
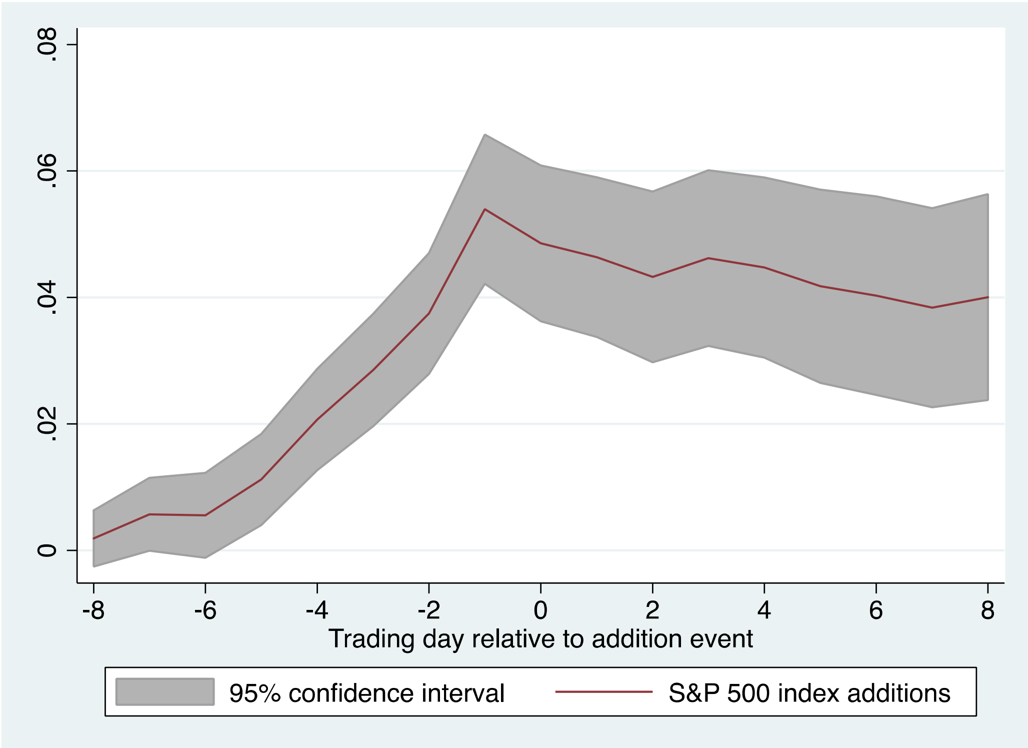


Figure 4: Event-time Returns for S&P 500 Index Additions and Peer Firms
This figure shows returns for firms added to the S&P 500 index (Panel A) and their peers (Panel B) in event-time (where 0 is the day when the index addition becomes effective). These graphs are based on the cumulated coefficient estimates of the addition and peer dummies shown in Table V, column 1. The grey band around the cumulated returns represents the 95%-confidence interval.

Panel A: Added Firms



Panel B: Peer Firms

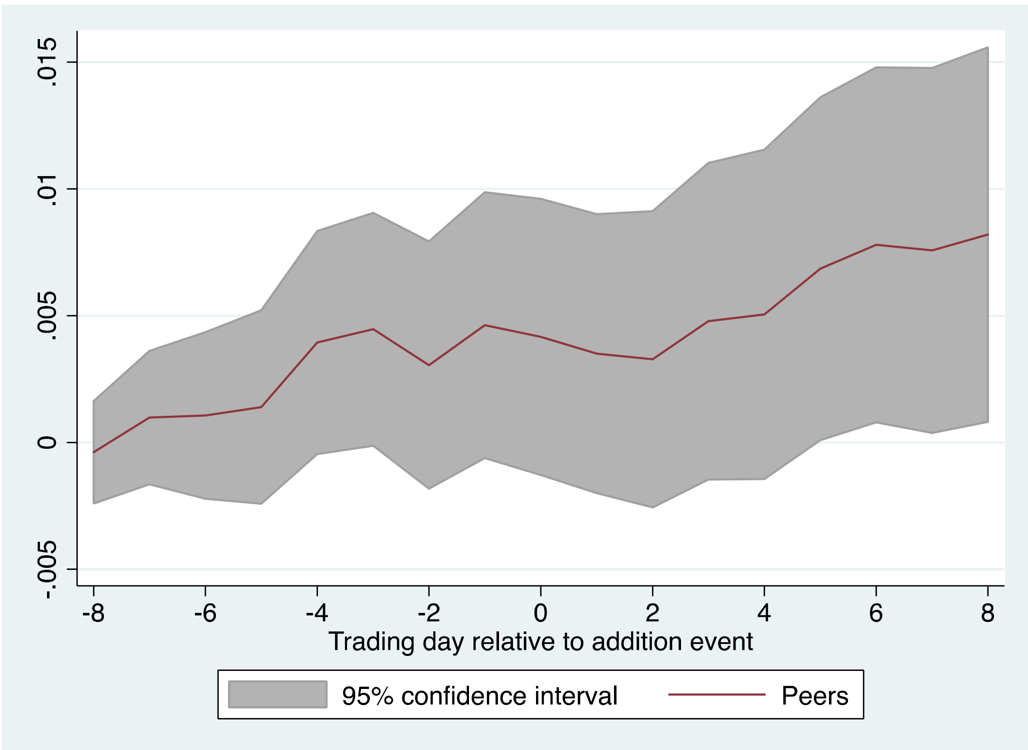


Table I: Descriptive Statistics

This table shows descriptive statistics for the main dependent and control variables used in this study. *N* indicates the number of non-missing observations at the stock-quarter level over our sample period (after dropping non-common shares [i.e., retaining only CRSP share codes 10 and 11], stocks with an end-of-quarter price below $1, and stocks with less than 10 daily non-missing return observations in a quarter). Return is the compounded quarterly return. Bid-ask spread is defined as the average daily relative bid-ask spread (multiplied by 100). Log Amihud is defined as the natural logarithm of the average ratio of absolute returns over dollar volume scaled by one million. PIN is the probability of informed trading (Easley et al., 1996) estimated at quarterly frequency. Turnover is defined as the total dollar volume in the quarter divided by the market capitalization at the end of the previous quarter. Log turnover is the natural logarithm of one plus turnover. Total assets and return on assets are those reported for the end of the previous fiscal year. Log total assets is the natural logarithm of total assets. Leverage is the ratio of long-term debt and current liabilities over stockholders' equity (at the end of the previous fiscal year). Log leverage is the natural logarithm of one plus leverage. Market-to-book is the ratio of the stock's market value at the end of the previous quarter over the stockholders' equity. Investment (speculative) grade is a dummy variable that indicates whether a firm's long-term debt has an investment grade (speculative grade) rating given by S&P. The remaining fraction of stock-quarter observation does not have a long-term bond rating. Num. analysts is the number of analysts following a stock at the end of the previous quarter. Log analysts is the natural logarithm of one plus the number of analysts. Mutual fund ownership is the fraction of shares outstanding owned by open-ended mutual funds at the end of the previous quarter. Institutional ownership is the fraction of shares outstanding owned by institutional investors at the end of the previous quarter. Mfflow is the selling pressure by mutual funds experiencing a fire sale as defined in Edmans et al. (2012). Mfflow complement is the difference between mutual fund trading pressure by all mutual funds and the selling pressure by fire-selling mutual funds. All variables are winsorized at the 0.5% level on both sides.

					Quantiles			
	N	Mean	S.D.	Min	0.25	Median	0.75	Max
Dependent variables:								
Return	353,146	0.04	0.29	-0.71	-0.12	0.02	0.15	1.7
Bid-ask spread	352,528	2.18	3.19	0.01	0.23	1.06	2.83	33.33
Log Amihud	353,138	-3.23	3.38	-11.43	-5.83	-3.34	-0.63	4.94
PIN	271,492	0.21	0.12	0	0.12	0.18	0.28	0.93
Turnover	342,933	0.43	0.55	0	0.11	0.25	0.54	4.53
Control variables:								
Total assets	349,785	3,641.55	15,236.01	0.01	75.94	324.97	1,355.04	16,1775
Leverage	348,278	0.9	3.17	-20.16	0.02	0.37	1.05	33
Investment grade	353,146	0.13	0.33	0	0	0	0	1
Speculative grade	353,146	0.11	0.32	0	0	0	0	1
Market-to-book	349,780	0	0.01	-0.03	0	0	0	0.09
Return on assets	349,222	-0.04	0.35	-17.49	-0.02	0.02	0.06	0.73
Num. analysts	353,146	5.04	6.33	0	0	3	7	56
Mutual fund ownership	353,146	0.16	0.14	0	0.03	0.13	0.26	1
Inst. ownership	353,146	0.43	0.31	0	0.15	0.4	0.7	1
Mfflow	326,122	-0.01	0.06	-13.73	-0.01	0	0	0
Mfflow complement	326,122	0.09	5.53	-5.58	0	0.01	0.02	3,010.7

Table II: Return Spillover Effect

This table reports results from estimating equation (1) at the stock-quarter level. The dependent variable is the quarterly return. The main independent variables are FS and PEER dummies that flag fire sale events and peers for fire sale events, respectively. For example, the FS(t=4) dummy equals one when the given firm experienced a fire sale 4 quarters ago and the PEER(t=4) dummy equals one for all peer firms of a firm that experienced a fire sale 4 quarters ago (and that did not themselves experience a fire sale in the previous or subsequent 8 quarters). All regressions include dummies from t=-16 to t=16; for brevity we only show the coefficients for t=-2 to t=8. Firm and quarter fixed effects are included in all specifications. In specification 2, additional firm-level controls are included (logarithm of total assets, logarithm of leverage, investment grade dummy, speculative grade dummy, market-to-book ratio, return on assets, logarithm of number of analysts). In specification 3, ownership controls are included (mutual fund ownership, institutional ownership). In specification 4, mutual fund flow controls are included (separately for fire sale funds and others). In specification 5, ownership and flow controls are included. In specification 6, firm-level, ownership and flow controls are included. All variables are defined in Appendix A. Standard errors are double-clustered at the firm and quarter level. *t*-statistics are reported below coefficient estimates in parentheses. At the bottom of the table, we report the sum of the FS and PEER dummy coefficients for windows [1, 4] and [1, 8], respectively, together with the corresponding *t*-statistic for the cumulated return reversal. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

	(1)		(2)		(3)		(4)		(5)		(6)	
Event-time	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER
t = -2	-0.004	-0.000	-0.003	-0.001	-0.001	-0.000	-0.004	-0.001	-0.001	-0.001	-0.002	-0.002
	(-0.77)	(-0.14)	(-0.61)	(-0.34)	(-0.14)	(-0.15)	(-0.82)	(-0.38)	(-0.16)	(-0.38)	(-0.46)	(-0.59)
t = -1	-0.016**	-0.006	-0.015**	-0.005	-0.012*	-0.007	-0.016**	-0.005	-0.012	-0.006	-0.013*	-0.005
	(-2.27)	(-1.52)	(-2.22)	(-1.28)	(-1.74)	(-1.57)	(-2.14)	(-1.30)	(-1.64)	(-1.34)	(-1.94)	(-1.19)
t = 0	-0.076***	-0.015***	-0.074***	-0.015***	-0.071***	-0.016***	-0.071***	-0.013***	-0.067***	-0.014***	-0.068***	-0.014***
	(-8.58)	(-3.58)	(-8.53)	(-3.46)	(-8.14)	(-3.75)	(-9.08)	(-3.25)	(-8.66)	(-3.52)	(-8.94)	(-3.33)
t = 1	0.004	0.004	0.004	0.005	0.007	0.004	0.005	0.006*	0.008	0.006*	0.005	0.006*
	(0.71)	(1.42)	(0.62)	(1.60)	(1.21)	(1.43)	(0.82)	(1.91)	(1.31)	(1.91)	(0.88)	(1.98)
t = 2	0.007	0.003	0.007	0.004	0.009	0.003	0.007	0.003	0.009	0.003	0.008	0.004
	(1.10)	(0.91)	(1.16)	(1.24)	(1.49)	(1.04)	(1.12)	(0.85)	(1.50)	(0.96)	(1.26)	(1.05)
t = 3	0.016*	0.004	0.015	0.005	0.018*	0.005	0.015	0.004	0.017*	0.004	0.015	0.004
	(1.69)	(1.00)	(1.66)	(1.20)	(1.89)	(1.12)	(1.53)	(0.86)	(1.71)	(0.96)	(1.56)	(1.03)
t = 4	0.005	0.005	0.005	0.005	0.007	0.005	0.008	0.006	0.010	0.006	0.007	0.006
	(0.82)	(1.23)	(0.73)	(1.49)	(1.08)	(1.37)	(1.23)	(1.46)	(1.50)	(1.62)	(1.17)	(1.63)
t = 5	-0.006	-0.002	-0.006	-0.001	-0.004	-0.002	-0.006	-0.003	-0.004	-0.002	-0.006	-0.001
	(-0.85)	(-0.61)	(-0.95)	(-0.24)	(-0.65)	(-0.46)	(-0.90)	(-0.68)	(-0.70)	(-0.52)	(-0.91)	(-0.29)
t = 6	0.005	-0.002	0.005	-0.001	0.007	-0.002	0.006	-0.002	0.007	-0.002	0.006	-0.001
	(1.09)	(-0.48)	(1.03)	(-0.12)	(1.39)	(-0.37)	(1.21)	(-0.54)	(1.49)	(-0.43)	(1.24)	(-0.19)
t = 7	0.011	0.000	0.011	0.001	0.013*	0.001	0.011	0.000	0.013*	0.001	0.012	0.001
	(1.49)	(0.06)	(1.51)	(0.32)	(1.69)	(0.30)	(1.49)	(0.12)	(1.68)	(0.38)	(1.55)	(0.41)
t = 8	-0.002	0.002	-0.003	0.003	-0.001	0.002	-0.003	0.001	-0.002	0.002	-0.003	0.003
	(-0.37)	(0.51)	(-0.51)	(0.84)	(-0.16)	(0.79)	(-0.39)	(0.44)	(-0.22)	(0.74)	(-0.43)	(0.90)

(continued on next page)

	(1)	(2)	(3)	(4)	(5)	(6)
(continued from previous page)						
Log total assets		-0.058***				-0.054***
		(-10.36)				(-9.21)
Log leverage		0.045***				0.040***
		(8.61)				(7.49)
IG rating		-0.009*				-0.010
		(-1.71)				(-1.81)
SG rating		-0.002				-0.000
		(-0.58)				(-0.00)
Market-to-book		-3.308***				-3.071***
		(-6.90)				(-5.79)
Return on assets		-0.003				-0.004
		(-0.54)				(-0.64)
Log analysts		-0.031***				-0.021***
		(-9.70)				(-7.29)
MF ownership			-0.085***		-0.084***	-0.013
			(-3.54)		(-3.43)	(-0.3)
Inst. ownership			-0.162***		-0.173***	-0.103***
			(-9.82)		(-10.02)	(-6.51)
Mfflow				0.078**	0.071**	0.073**
				(2.43)	(2.29)	(2.23)
Mfflow compl.				-0.000	-0.000	-0.000
				(-0.91)	(-0.95)	(-1.01)
N	352,870	340,084	352,870	325,817	325,817	315,293
adj. R^2	0.153	0.170	0.160	0.163	0.172	0.182
Firm & qtr. f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Reversal [1, 4]	0.033** 0.016**	0.031** 0.019***	0.042** 0.018**	0.035** 0.018**	0.044*** 0.019**	0.035** 0.019**
	(2.01) (2.25)	(2.02) (2.67)	(2.56) (2.45)	(2.17) (2.37)	(2.72) (2.55)	(2.35) (2.63)
Reversal [1, 8]	0.042* 0.014*	0.038* 0.021**	0.056** 0.017**	0.044** 0.015*	0.058** 0.019**	0.044** 0.022**
	(1.89) (1.69)	(1.83) (2.50)	(2.53) (2.21)	(1.99) (1.75)	(2.61) (2.24)	(2.15) (2.43)

Table III: Cross-sectional tests for Return Spillover Effect

This table reports results from estimating regressions of quarterly returns on PEER dummies that flag peers for fire sale events. For example, the PEER(t=4) dummy equals one for all peer firms of a firm that experienced a fire sale 4 quarters ago (and that did not themselves experience a fire sale in the previous or subsequent 8 quarters). All regressions include dummies from t=-16 to t=16; for brevity we only show the coefficients for t=-2 to t=8. Firm and quarter fixed effects are included in all specifications. To focus on how the return spillover effect varies across different firm and fire sale-peer relationship characteristics, stock-quarter observations with fire sales in the preceding or succeeding eight quarters are excluded. Panel A shows results for sample splits: In columns 1 and 2, stocks are split along the median of firms' total assets. In columns 3 and 4, stocks are split into firms with an investment grade rating and others. In columns 5 and 6, stocks are split for whether they are a constituent of the S&P 500 index or not. In columns 7 and 8, stocks are split along the median of analyst coverage. In columns 9 and 10, stocks are split along the median of analysts' average forecast error. Panel B shows cross-sectional tests for different fire sale-peer relationship characteristics. Since these characteristics are defined only for peer stocks in relation to the fire sale stocks to which they are linked, these tests cannot take the form of a sample split. Rather, peers are grouped based on a specific fire sale-peer relationship characteristic and then different sets of event-time dummies are included for each peer group in the same regression. In specification 1, peers are categorized into two groups based on the severity of the fire sale effect (in terms of the drop in raw return for the associated fire sale stock). In specification 2, peers are categorized into two groups based on the peer score given in the TNIC data. In specification 3, peers are categorized into two groups based on whether they are linked with a fire sale stock that has less or more than ten peers. In specification 4, peers are categorized into two groups based on their return correlation with the fire stock to which they are linked. In specification 5, peers are categorized into two groups based on the turnover-volatility correlation of their stock returns. [Banerjee (2011) shows that investors in stocks with a high turnover-volatility correlation condition on prices more.] The return correlation between peer and fire sale stocks as well as the turnover-volatility correlation of peer stocks are estimated using daily return data in the 8 quarters prior to the fire sale. All variables are defined in Appendix A. Standard errors are double-clustered at the firm and quarter level. *t*-statistics are reported below coefficient estimates in parentheses. For each sample split, we report the *t*-statistic of the difference in the PEER(t=0) coefficient. At the bottom of the table, we further report the sum of the PEER dummy coefficients for windows [1, 4] and [1, 8], respectively, together with the corresponding *t*-statistic for the cumulated return reversal. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Panel A: Sample splits by firm characteristics

	Firm size		Rating		S&P 500 member		Analyst coverage		Average forecast error	
	Small	Large	Other	IG	No	Yes	Low	High	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Event-time	PEER	PEER	PEER	PEER	PEER	PEER	PEER	PEER	PEER	PEER
t = -2	-0.000	-0.000	-0.000	0.000	-0.000	0.001	0.000	-0.002	0.004	-0.005
	(-0.02)	(-0.02)	(-0.01)	(0.15)	(-0.00)	(0.15)	(0.09)	(-0.57)	(0.89)	(-1.20)
t = -1	-0.010	-0.004	-0.009	-0.004	-0.009	-0.005	-0.009	-0.009**	-0.007	-0.012**
	(-1.33)	(-1.33)	(-1.54)	(-1.44)	(-1.51)	(-1.48)	(-1.30)	(-2.23)	(-1.22)	(-2.39)
t = 0	-0.024***	-0.013***	-0.021***	-0.006**	-0.020***	-0.009***	-0.025***	-0.015***	-0.024***	-0.012***
	(-3.81)	(-3.84)	(-4.25)	(-2.06)	(-4.24)	(-2.74)	(-4.36)	(-3.96)	(-4.20)	(-2.95)
t = 1	-0.002	0.000	0.000	-0.001	-0.000	0.002	0.002	-0.001	0.002	0.000
	(-0.40)	(0.12)	(0.01)	(-0.42)	(-0.08)	(0.53)	(0.48)	(-0.23)	(0.53)	(0.11)
t = 2	0.000	0.002	0.002	0.002	0.002	0.001	-0.003	0.003	-0.001	0.005
	(0.07)	(0.58)	(0.48)	(0.61)	(0.46)	(0.39)	(-0.62)	(0.99)	(-0.21)	(1.20)
t = 3	0.002	0.002	0.004	-0.000	0.004	0.001	0.007	0.001	0.002	-0.002
	(0.32)	(0.57)	(0.84)	(-0.13)	(0.78)	(0.25)	(1.27)	(0.36)	(0.48)	(-0.38)
t = 4	0.005	0.006	0.006	0.002	0.006	0.006	0.007	0.006*	0.007	0.008*
	(0.72)	(1.53)	(1.36)	(0.66)	(1.26)	(1.31)	(1.41)	(1.87)	(1.32)	(1.97)
t = 5	0.003	-0.004	-0.002	-0.003	-0.002	-0.004	0.001	-0.007*	-0.003	-0.010**
	(0.45)	(-1.08)	(-0.42)	(-0.89)	(-0.41)	(-1.18)	(0.28)	(-1.96)	(-0.70)	(-2.40)
t = 6	-0.000	0.003	-0.000	0.001	0.001	0.002	-0.000	0.001	-0.003	0.005
	(-0.03)	(0.77)	(-0.00)	(0.42)	(0.14)	(0.61)	(-0.02)	(0.21)	(-0.69)	(1.03)
t = 7	0.005	0.000	0.002	-0.002	0.003	-0.001	0.001	0.003	-0.002	0.004
	(0.91)	(0.07)	(0.51)	(-0.55)	(0.71)	(-0.33)	(0.12)	(0.99)	(-0.37)	(1.02)
t = 8	-0.001	0.003	0.001	0.002	0.001	0.004	-0.002	0.003	0.006	0.001
	(-0.14)	(0.98)	(0.27)	(0.57)	(0.19)	(1.17)	(-0.62)	(0.97)	(1.32)	(0.15)
t-statistic of difference	2.06**		2.71***		2.61**		1.93*		2.50**	
N	89,957	90,175	163,461	25,260	164,587	24,166	103,736	84,014	57,393	57,255
adj. R^2	0.144	0.199	0.144	0.279	0.141	0.278	0.125	0.232	0.169	0.191
Firm & quart. f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reversal [1, 4]	0.005	0.010	0.012	0.002	0.011	0.010	0.014	0.010	0.010	0.011
	(0.44)	(1.39)	(1.30)	(0.38)	(1.19)	(1.34)	(1.41)	(1.28)	(1.10)	(1.26)
Reversal [1, 8]	0.011	0.012	0.013	0.000	0.013	0.011	0.014	0.010	0.008	0.011
	(0.86)	(1.43)	(1.19)	(0.06)	(1.22)	(1.31)	(1.46)	(0.96)	(0.64)	(0.89)

Panel B: Cross-sectional tests by peer relationship characteristics

	Severity of fire sale		Peerscore		# Peers		Return correlation		Volume-volatility correlation	
	Small	Large	Small	Large	≥ 10	< 10	Low	High	High	Low
	(1)		(2)		(3)		(4)		(5)	
Event-time	PEER	PEER	PEER	PEER	PEER	PEER	PEER	PEER	PEER	PEER
t = -2	0.002	0.001	0.002	-0.001	-0.000	0.001	0.004	-0.003	-0.001	0.002
	(0.61)	(0.20)	(0.60)	(-0.23)	(-0.06)	(0.16)	(0.90)	(-0.72)	(-0.15)	(0.52)
t = -1	0.000	-0.017*	-0.007*	-0.009	-0.007	-0.011*	-0.002	-0.015**	-0.009	-0.007
	(0.01)	(-1.79)	(-1.80)	(-1.26)	(-1.47)	(-1.82)	(-0.40)	(-2.41)	(-1.34)	(-1.58)
t = 0	0.005	-0.047***	-0.010**	-0.030***	-0.017***	-0.021***	-0.012**	-0.028***	-0.015***	-0.022***
	(0.94)	(-5.85)	(-2.63)	(-4.53)	(-3.56)	(-4.49)	(-2.60)	(-4.95)	(-3.06)	(-5.11)
t = 1	0.002	-0.001	-0.001	0.002	-0.000	0.001	0.002	-0.001	-0.002	0.003
	(0.43)	(-0.24)	(-0.29)	(0.43)	(-0.05)	(0.20)	(0.51)	(-0.31)	(-0.61)	(0.79)
t = 2	-0.002	0.006	0.000	0.004	0.005	-0.004	0.003	0.003	0.005	0.000
	(-0.71)	(0.93)	(0.06)	(0.85)	(1.28)	(-0.97)	(0.92)	(0.67)	(1.29)	(0.11)
t = 3	-0.004	0.013*	0.001	0.008	0.004	0.002	0.004	0.004	0.004	0.005
	(-1.18)	(1.87)	(0.20)	(1.29)	(0.92)	(0.38)	(0.94)	(0.73)	(0.72)	(1.05)
t = 4	0.004	0.006	0.004	0.006	0.007*	0.002	0.009**	-0.001	0.007*	0.002
	(1.02)	(0.89)	(1.25)	(1.22)	(1.67)	(0.43)	(2.04)	(-0.21)	(1.85)	(0.49)
t = 5	-0.001	-0.004	-0.001	-0.005	-0.004	0.002	-0.001	-0.002	-0.004	-0.000
	(-0.28)	(-0.70)	(-0.18)	(-1.07)	(-0.91)	(0.33)	(-0.21)	(-0.49)	(-0.86)	(-0.03)
t = 6	0.003	-0.004	-0.000	0.001	0.001	-0.002	-0.002	0.003	0.003	-0.004
	(0.88)	(-0.62)	(-0.10)	(0.23)	(0.28)	(-0.50)	(-0.53)	(0.63)	(0.63)	(-0.76)
t = 7	-0.004	0.009	0.003	0.003	-0.001	0.009**	0.004	0.005	0.008*	-0.000
	(-0.93)	(1.51)	(0.88)	(0.52)	(-0.19)	(2.02)	(0.82)	(1.14)	(1.72)	(-0.02)
t = 8	-0.005	0.007	0.001	0.002	0.001	0.003	0.004	0.004	0.001	0.006
	(-1.08)	(1.28)	(0.49)	(0.56)	(0.22)	(0.76)	(1.07)	(0.88)	(0.28)	(1.36)
t-statistic of difference	4.92***		3.44***		1.11		2.89***		1.81*	
N	188,776		188,776		188,776		188,776		188,776	
adj. R^2	0.150		0.147		0.147		0.147		0.147	
Firm & quart. f.e.	Yes		Yes		Yes		Yes		Yes	
Reversal [1, 4]	-0.001	0.023	0.004	0.020	0.017	0.001	0.019	0.004	0.014	0.010
	(-0.08)	(1.64)	(0.64)	(1.90)	(1.70)	(0.10)	(1.80)	(0.47)	(1.53)	(1.27)
Reversal [1, 8]	-0.007	0.031	0.008	0.021	0.014	0.012	0.024	0.014	0.022	0.012
	(-0.93)	(1.65)	(0.92)	(1.66)	(1.24)	(0.87)	(2.01)	(0.98)	(2.02)	(1.27)

Table IV: Robustness of Return Spillover Effect

This table reports results from estimating a reduced variant of equation (1) at the stock-quarter level. Specifically, stock-quarter observations with a fire sale in the preceding or succeeding eight quarters are excluded and the main independent variables are PEER dummies that flag peers for fire sale events. For example, the PEER(t=4) dummy equals one for all peer firms of a firm that experienced a fire sale 4 quarters ago (and that did not themselves experience a fire sale in the previous or subsequent 8 quarters). The dependent variable is the quarterly return. All regressions include dummies from t=-16 to t=16; for brevity we only show the coefficients for t=-2 to t=8. All regressions include firm-level controls (logarithm of total assets, logarithm of leverage, investment grade dummy, speculative grade dummy, market-to-book ratio, return on assets, logarithm of number of analysts), ownership controls (mutual fund ownership, institutional ownership), mutual fund flow controls (separately for fire sale funds and others) and fixed effects as specified at the bottom of the table. Specification 1 reports the return spillover effect when all baseline controls are included. In specification 2, the liquidity provision proxy is added as an additional control variable. In specification 3, short interest is added as an additional control variable. In specification 4, dummies for different mutual fund flow deciles (separately for fire sale funds and others) are used instead of the continuous fund flow variables. In specification 5, the fire sale stock share is added as an additional control variable. In column 6, the fire sale fund share is added as an additional control variable. In specification 7, capx is added as an additional control variable (for visibility, the capx coefficient is multiplied by 1,000). In specification 8, industry×quarter fixed effects (based on the Fama-French 48 industry classification) are used instead of quarter fixed effects. All variables are defined in Appendix A. Standard errors are double-clustered at the firm and quarter level. *t*-statistics are reported below coefficient estimates in parentheses. At the bottom of the table, we report the sum of the PEER dummy coefficients for windows [1, 4] and [1, 8], respectively, together with the corresponding *t*-statistic for the cumulated return reversal. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Event-time	(1) PEER	(2) PEER	(3) PEER	(4) PEER	(5) PEER	(6) PEER	(7) PEER	(8) PEER
t = -2	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	0.000	0.000
	(-0.46)	(-0.62)	(-0.45)	(-0.48)	(-0.54)	(-0.46)	(0.09)	(0.17)
t = -1	-0.007	-0.007	-0.006	-0.007	-0.007	-0.007	-0.007	-0.003
	(-1.27)	(-1.34)	(-1.26)	(-1.33)	(-1.31)	(-1.27)	(-1.48)	(-0.97)
t = 0	-0.016***	-0.015***	-0.016***	-0.015***	-0.016***	-0.016***	-0.014***	-0.009***
	(-3.74)	(-3.67)	(-3.74)	(-3.73)	(-3.80)	(-3.76)	(-3.80)	(-3.62)
t = 1	0.002	0.004	0.002	0.002	0.002	0.002	-0.000	-0.001
	(0.61)	(1.25)	(0.60)	(0.79)	(0.55)	(0.60)	(-0.14)	(-0.50)
t = 2	0.002	0.004	0.002	0.003	0.002	0.002	0.002	-0.001
	(0.67)	(1.20)	(0.68)	(0.98)	(0.66)	(0.67)	(0.56)	(-0.55)
t = 3	0.003	0.005	0.003	0.004	0.003	0.003	0.002	0.001
	(0.87)	(1.18)	(0.87)	(1.11)	(0.84)	(0.87)	(0.61)	(0.47)
t = 4	0.008**	0.008**	0.008**	0.008**	0.007**	0.008**	0.007*	0.005*
	(2.04)	(2.14)	(2.04)	(2.22)	(2.00)	(2.04)	(1.97)	(1.92)
t = 5	-0.002	-0.002	-0.002	-0.001	-0.002	-0.002	-0.000	0.000
	(-0.51)	(-0.47)	(-0.50)	(-0.33)	(-0.56)	(-0.51)	(-0.05)	(0.09)
t = 6	0.002	0.002	0.002	0.003	0.002	0.002	0.002	0.001
	(0.49)	(0.42)	(0.51)	(0.71)	(0.58)	(0.49)	(0.55)	(0.42)
t = 7	0.004	0.004	0.004	0.005	0.004	0.004	0.005	0.004
	(1.09)	(1.11)	(1.10)	(1.36)	(1.12)	(1.09)	(1.64)	(1.66)
t = 8	0.001	0.001	0.001	0.001	0.001	0.001	-0.000	-0.001
	(0.41)	(0.45)	(0.44)	(0.38)	(0.32)	(0.41)	(-0.02)	(-0.24)

(continued on next page)

Event-time	(1) PEER	(2) PEER	(3) PEER	(4) PEER	(5) PEER	(6) PEER	(7) PEER	(8) PEER
(continued from previous page)								
Liquidity provision proxy		-0.022***						
		(-2.78)						
Short interest			-0.178**					
			(-2.08)					
Fire sale stock share					1.518***			
					(4.26)			
Fire sale fund share						-0.025		
						(-0.48)		
CAPX							-0.048**	
							(-2.57)	
N	156,891	149,241	156,891	156,891	156,891	156,891	141,186	156,846
adj. R^2	0.195	0.198	0.195	0.211	0.198	0.195	0.215	0.263
Firm & qtr. f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Firm & ind×qtr f.e.	No	No	No	No	No	No	No	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ownership controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow controls	Yes	Yes	Yes	Yes*	Yes	Yes	Yes	Yes
Reversal [1, 4]	0.015*	0.020**	0.015*	0.018**	0.015*	0.015*	0.010	0.004
	(1.80)	(2.47)	(1.81)	(2.19)	(1.76)	(1.80)	(1.36)	(0.88)
Reversal [1, 8]	0.020*	0.025**	0.020*	0.025**	0.020*	0.020*	0.017	0.009
	(1.84)	(2.34)	(1.88)	(2.33)	(1.81)	(1.84)	(1.60)	(1.28)

Table V: Placebo Test for S&P 500 Index Additions

This table reports results from estimating regressions in the spirit of equation (1) at the stock-day level. The dependent variable is the daily return. The main independent variables are AD and PEER dummies that flag S&P 500 index addition events and peers for these addition events, respectively. For example, the FS(t=4) dummy equals one when the given firm experienced a fire sale 4 quarters ago and the PEER(t=4) dummy equals one for all peer firms of a firm that experienced a fire sale 4 quarters ago (and that did not themselves experience a fire sale in the previous or subsequent 8 quarters). All regressions include dummies from t=-25 to t=25; for brevity we only show the coefficients for t=-8 to t=8. Firm and day fixed effects are included in all specifications. In specification 2, additional firm-level controls are included (logarithm of total assets, logarithm of leverage, investment grade dummy, speculative grade dummy, market-to-book ratio, return on assets, logarithm of number of analysts). In specification 3, ownership controls are included (mutual fund ownership, institutional ownership). In specification 4, mutual fund flow controls are included (separately for fire sale funds and others). In specification 5, ownership and flow controls are included. In specification 6, firm-level, ownership and flow controls are included. All variables are defined in Appendix A. Standard errors are double-clustered at the firm and day level. *t*-statistics are reported below coefficient estimates in parentheses. At the bottom of the table, we report the sum of the AD and PEER dummy coefficients for windows [-4, -1] and [-8, -1], respectively, together with the corresponding *t*-statistic for the cumulated price pressure effect. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

	(1)		(2)		(3)		(4)		(5)		(6)	
Event-time	AD	PEER	AD	PEER	AD	PEER	AD	PEER	AD	PEER	AD	PEER
t = -8	0.002	-0.000	0.003	-0.001	0.002	-0.000	0.002	-0.000	0.003	-0.000	0.003	-0.000
	(0.85)	(-0.24)	(1.01)	(-0.43)	(0.88)	(-0.22)	(0.86)	(-0.12)	(0.89)	(-0.10)	(1.04)	(-0.27)
t = -7	0.003	0.001	0.004*	0.001	0.003	0.002	0.003	0.001	0.003	0.001	0.004*	0.001
	(1.62)	(0.91)	(1.90)	(0.88)	(1.65)	(0.92)	(1.55)	(0.79)	(1.58)	(0.81)	(1.87)	(0.78)
t = -6	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.001	-0.000	-0.000	-0.000	-0.000	-0.000
	(-0.21)	(-0.15)	(-0.09)	(-0.13)	(-0.18)	(-0.13)	(-0.22)	(-0.13)	(-0.19)	(-0.11)	(-0.09)	(-0.04)
t = -5	0.006***	0.001	0.006***	0.001	0.006***	0.001	0.006***	0.000	0.006***	0.000	0.006***	0.001
	(2.81)	(0.46)	(2.93)	(0.58)	(2.84)	(0.47)	(2.66)	(0.28)	(2.69)	(0.31)	(2.79)	(0.47)
t = -4	0.009***	0.003	0.010***	0.003	0.010***	0.003	0.010***	0.002	0.010***	0.002	0.010***	0.003
	(3.61)	(1.49)	(3.70)	(1.64)	(3.63)	(1.50)	(3.60)	(1.35)	(3.62)	(1.37)	(3.70)	(1.50)
t = -3	0.008***	-0.000	0.008***	-0.000	0.008***	-0.000	0.008***	-0.000	0.008***	0.000	0.008***	-0.000
	(2.92)	(-0.06)	(3.08)	(-0.05)	(2.96)	(-0.05)	(2.87)	(-0.01)	(2.90)	(0.00)	(3.04)	(-0.03)
t = -2	0.010***	-0.001	0.010***	-0.000	0.010***	-0.001	0.010***	-0.001	0.010***	-0.001	0.010***	-0.001
	(4.20)	(-0.63)	(4.33)	(-0.36)	(4.22)	(-0.61)	(4.15)	(-0.59)	(4.17)	(-0.56)	(4.30)	(-0.40)
t = -1	0.017***	0.002	0.017***	0.002	0.018***	0.002*	0.017***	0.002*	0.017***	0.002*	0.017***	0.002
	(5.31)	(1.66)	(5.20)	(1.33)	(5.32)	(1.68)	(5.15)	(1.67)	(5.16)	(1.69)	(5.05)	(1.33)

(continued on next page)

	(1)		(2)		(3)		(4)		(5)		(6)	
Event-time	AD	PEER	AD	PEER	AD	PEER	AD	PEER	AD	PEER	AD	PEER
(continued from previous page)												
t = 0	-0.006***	-0.001	-0.006**	-0.002	-0.006***	-0.001	-0.006***	-0.002	-0.006***	-0.002	-0.006***	-0.002
	(-2.72)	(-0.99)	(-2.57)	(-1.15)	(-2.71)	(-0.97)	(-2.83)	(-1.11)	(-2.83)	(-1.09)	(-2.68)	(-1.20)
t = 1	-0.003	-0.001	-0.003	-0.001	-0.003	-0.001	-0.003	-0.001	-0.003	-0.001	-0.003	-0.001
	(-1.66)	(-0.73)	(-1.61)	(-0.63)	(-1.65)	(-0.71)	(-1.65)	(-0.87)	(-1.64)	(-0.85)	(-1.59)	(-0.82)
t = 2	-0.005**	-0.002	-0.005**	-0.001	-0.005**	-0.002	-0.005*	-0.002	-0.005*	-0.002	-0.005**	-0.001
	(-2.05)	(-1.32)	(-2.13)	(-1.09)	(-2.04)	(-1.30)	(-1.97)	(-1.33)	(-1.96)	(-1.30)	(-2.03)	(-1.03)
t = 3	0.001	-0.000	0.001	-0.000	0.001	-0.000	0.001	-0.000	0.001	-0.000	0.001	-0.000
	(0.53)	(-0.16)	(0.68)	(-0.22)	(0.54)	(-0.13)	(0.55)	(-0.30)	(0.55)	(-0.27)	(0.71)	(-0.32)
t = 4	-0.002	0.000	-0.001	0.000	-0.002	0.000	-0.002	0.000	-0.002	0.000	-0.001	0.000
	(-0.95)	(0.35)	(-0.63)	(0.27)	(-0.94)	(0.38)	(-0.99)	(0.45)	(-0.99)	(0.48)	(-0.65)	(0.39)
t = 5	-0.004	0.001	-0.003	0.001	-0.004	0.001	-0.004*	0.001	-0.004*	0.001	-0.004	0.001
	(-1.65)	(0.90)	(-1.49)	(0.88)	(-1.65)	(0.92)	(-1.81)	(0.90)	(-1.81)	(0.92)	(-1.65)	(0.88)
t = 6	-0.001	0.001	-0.001	0.001	-0.001	0.001	-0.001	0.001	-0.001	0.001	-0.001	0.001
	(-0.73)	(1.00)	(-0.60)	(1.07)	(-0.72)	(1.02)	(-0.78)	(0.99)	(-0.78)	(1.02)	(-0.64)	(1.12)
t = 7	-0.001	0.001	-0.001	0.000	-0.001	0.001	-0.001	0.001	-0.001	0.001	-0.001	0.001
	(-0.42)	(0.33)	(-0.35)	(0.22)	(-0.41)	(0.35)	(-0.42)	(0.46)	(-0.42)	(0.48)	(-0.35)	(0.35)
t = 8	0.002	0.001	0.002	0.000	0.002	0.001	0.002	0.001	0.002	0.001	0.002	0.000
	(0.89)	(0.39)	(0.84)	(0.15)	(0.89)	(0.41)	(0.96)	(0.49)	(0.96)	(0.52)	(0.91)	(0.26)
N	17,739,694		17,035,338		17,739,694		15,953,631		15,953,631		15,442,302	
adj. R^2	0.002		0.003		0.002		0.003		0.003		0.093	
Firm & date f.e.	Yes		Yes		Yes		Yes		Yes		Yes	
Firm controls	No		Yes		No		No		No		Yes	
Own. Controls	No		No		Yes		No		Yes		Yes	
Flow controls	No		No		No		Yes		Yes		Yes	
Run-up [-4, -1]	0.044***	0.004	0.045***	0.004	0.045***	0.004	0.044***	0.004	0.044***	0.004	0.045***	0.004
	(8.12)	(1.34)	(8.24)	(1.44)	(8.16)	(1.37)	(7.96)	(1.34)	(7.98)	(1.38)	(8.10)	(1.37)
Run-up [-8, -1]	0.056***	0.005	0.058***	0.006	0.056***	0.005*	0.055***	0.005	0.055***	0.005	0.057***	0.005
	(7.34)	(1.63)	(7.68)	(1.66)	(7.40)	(1.69)	(7.19)	(1.49)	(7.24)	(1.57)	(7.55)	(1.54)

Table VI: Feedback Effect

This table reports results from estimating equation (2) at the stock-quarter level for the sample of fire sale events. The dependent variable is the quarterly return. The main independent variables are TNIC similarity score-weighted averages of peer characteristics as indicated by the table rows. In row 1), the average across peers is formed over a dummy variable for whether the peer is above median in terms of size. In row 2), the average across peers is formed over a dummy variable for whether the peer has an investment-grade rating. In row 3), the average across peers is formed over a dummy variable for whether the peer is a S&P 500 index member. In row 4), the average across peers is formed over a dummy variable for whether the peer is above median in terms of analyst coverage. In row 5), the average across peers is formed over a dummy variable for whether the peer is below median in terms of average forecast error. In row 6), the average across peers is formed over a dummy variable for whether the peer is above median in terms of the "information index", which is defined as the mean across the five dummy variables analyzed in rows 1)-5). Firm-level controls (logarithm of total assets, logarithm of leverage, investment grade dummy, speculative grade dummy, market-to-book ratio, return on assets, logarithm of number of analysts), ownership controls (mutual fund ownership, institutional ownership) and quarter fixed effects are included in all specifications. In the odd specifications (1, 3, 5, 7, 9, 11), the fund flow by fire-selling funds (*mfflow*) is included as a control. In the even specifications (2, 4, 6, 8, 10, 12), dummies for different mutual fund flow deciles are used instead of the continuous *mfflow* variable. All variables are defined in Appendix A. Standard errors are double-clustered at the firm and quarter level. *T*-statistics are reported below coefficient estimates in parentheses. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fraction of peers with:												
1) Above-median	0.0145***	0.0154***										
size	(2.88)	(2.99)										
2) Investment-			0.0113**	0.0123**								
grade rating			(2.33)	(2.53)								
3) S&P 500					0.0138**	0.0145***						
membership					(2.62)	(2.71)						
4) Above-median							0.0069	0.0064				
analyst coverage							(1.40)	(1.28)				
5) Above-median									0.0072	0.0070		
average forecast error									(1.52)	(1.47)		
6) Above-median											0.0213***	0.0219***
information index											(3.79)	(3.75)
N	27,789	27,789	27,798	27,798	27,798	27,798	27,798	27,798	26,428	26,428	27,798	27,798
adj. R^2	0.257	0.259	0.257	0.259	0.257	0.259	0.256	0.259	0.255	0.257	0.257	0.259
Qtr. f. e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Own. Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow controls	Yes	Yes*	Yes	Yes*	Yes	Yes*	Yes	Yes*	Yes	Yes*	Yes	Yes*

Table VII: Liquidity Spillover Effect

This table reports results from estimating equation (1) at the stock-quarter level. In Panel A, the dependent variable is the average bid-ask spread (multiplied by 100). In Panel B, the dependent variable is the natural logarithm of the average Amihud ratio (scaled by 1,000,000). In Panel C, the dependent variable is the Probability of Informed Trading (PIN) estimated at quarterly frequency. In Panel D, the dependent variable is the natural logarithm of share turnover. The main independent variables are FS and PEER dummies that flag fire sale events and peers for fire sale events, respectively. For example, the FS(t=0) dummy equals one when the given firm experienced a fire sale in a given quarter and the PEER(t=0) dummy equals one for all peer firms of a firm that experienced a fire sale in that quarter (and that did not themselves experience a fire sale in the previous or subsequent 8 quarters). All regressions include dummies from t=-16 to t=16; for brevity we only show the coefficients for t=0. Firm and quarter fixed effects are included in all specifications. In specification 2, additional firm-level controls are included (logarithm of total assets, logarithm of leverage, investment grade dummy, speculative grade dummy, market-to-book ratio, return on assets, logarithm of number of analysts). In specification 3, ownership controls are included (mutual fund ownership, institutional ownership). In specification 4, mutual fund flow controls are included (separately for fire sale funds and others). In specification 5, ownership and flow controls are included. In specification 6, firm-level, ownership and flow controls are included. All variables are defined in Appendix A. Standard errors are double-clustered at the firm and quarter level. *T*-statistics are reported below coefficient estimates in parentheses. *, **, *** indicate statistical significance at the 10%, 5%, and 1% levels.

Panel A: Bid-ask spreads

	(1)		(2)		(3)		(4)		(5)		(6)	
Event-time	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER
t = 0	0.154***	0.043*	0.160***	0.039*	0.173***	0.039*	0.156***	0.081***	0.176***	0.075***	0.152***	0.068***
	(3.63)	(1.94)	(3.88)	(1.91)	(4.02)	(1.74)	(3.00)	(5.29)	(3.37)	(4.94)	(3.01)	(4.88)
N	352,250		339,481		352,250		325,224		325,224		314,711	
adj. R^2	0.677		0.689		0.679		0.663		0.666		0.677	
Firm & quart. f.e.	Yes		Yes		Yes		Yes		Yes		Yes	
Firm controls	No		Yes		No		No		No		Yes	
Own. controls	No		No		Yes		No		Yes		Yes	
Flow controls	No		No		No		Yes		Yes		Yes	

Panel B: Log Amihud

	(1)		(2)		(3)		(4)		(5)		(6)	
Event-time	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER
t = 0	0.311***	0.084***	0.331***	0.075***	0.407***	0.069***	0.305***	0.096***	0.391***	0.077***	0.352***	0.065***
	(8.45)	(5.25)	(9.94)	(5.94)	(11.89)	(4.68)	(8.47)	(5.84)	(11.56)	(4.97)	(11.04)	(5.12)
N	352,863		340,078		352,863		325,817		325,817		315,293	
adj. R^2	0.863		0.903		0.884		0.858		0.881		0.905	
Firm & quart. f.e.	Yes		Yes		Yes		Yes		Yes		Yes	
Firm controls	No		Yes		No		No		No		Yes	
Own. controls	No		No		Yes		No		Yes		Yes	
Flow controls	No		No		No		Yes		Yes		Yes	

Panel C: PIN

	(1)		(2)		(3)		(4)		(5)		(6)	
Event-time	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER
t = 0	0.009***	0.002**	0.010***	0.002**	0.013***	0.002**	0.008***	0.002**	0.011***	0.002*	0.009***	0.001*
	(5.00)	(2.20)	(5.47)	(2.48)	(7.39)	(2.02)	(4.26)	(2.23)	(6.48)	(1.91)	(5.79)	(1.78)
N	271,148		262,086		271,148		256,029		256,029		247,998	
adj. R^2	0.574		0.601		0.588		0.576		0.592		0.609	
Firm & quart. f.e.	Yes		Yes		Yes		Yes		Yes		Yes	
Firm controls	No		Yes		No		No		No		Yes	
Own. controls	No		No		Yes		No		Yes		Yes	
Flow controls	No		No		No		Yes		Yes		Yes	

Panel D: Log turnover

	(1)		(2)		(3)		(4)		(5)		(6)	
Event-time	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER	FS	PEER
t = 0	-0.318***	-0.032***	-0.317***	-0.031***	-0.351***	-0.027***	-0.286***	-0.034***	-0.316***	-0.028***	-0.307***	-0.028***
	(-19.31)	(-3.91)	(-19.63)	(-4.00)	(-22.59)	(-3.40)	(-17.50)	(-4.20)	(-20.59)	(-3.55)	(-20.34)	(-3.58)
N	342,642		330,256		342,642		316,221		316,221		306,062	
adj. R^2	0.671		0.687		0.686		0.673		0.690		0.698	
Firm & quart. f.e.	Yes		Yes		Yes		Yes		Yes		Yes	
Firm controls	No		Yes		No		No		No		Yes	
Own. controls	No		No		Yes		No		Yes		Yes	
Flow controls	No		No		No		Yes		Yes		Yes	

Appendix A: Definition of Variables

Variable name	Source	Definition
Return	CRSP	Quarterly compounded return.
Bid-ask spread	CRSP	Difference between closing bid and ask prices, divided by the mid-price. Daily observations averaged quarterly.
Log Amihud	CRSP	Natural logarithm of the average ratio of absolute returns over dollar volume multiplied by one million.
PIN	Stephen Brown	Probability of informed trading (Easley et al., 1996) estimated at quarterly frequency. Data available at: http://scholar.rhsmith.umd.edu/sbrown/pin-data
Turnover	CRSP	Turnover is the total dollar volume in the quarter divided by the market capitalization at the end of the previous quarter.
Log turnover	CRSP	Log turnover is the natural logarithm of one plus turnover.
S&P 500 member	CRSP	Dummy equal to one if the stock is a current constituent of the S&P 500 index.
Total assets	Compustat	Total assets from the previous fiscal year.
Log total assets	Compustat	Log total assets is the logarithm of total assets from the previous fiscal year.
Leverage	Compustat	Leverage is the ratio of long-term debt and current liabilities over stockholders' equity at the end of the previous fiscal year.
Log leverage	Compustat	Log leverage is the natural logarithm of one plus leverage.
Investment grade Speculative grade	Compustat	Investment (speculative) grade is a dummy variable that indicates whether a firms long-term debt has an investment grade (speculative grade) rating given by Standard&Poors.
Market-to-book	Compustat	Market-to-book is the ratio of the stock's market capitalization at the end of the previous quarter over the stockholders' equity.
Return on assets	Compustat	Return on assets as reported for the previous fiscal year.
Num. analysts	I/B/E/S	Num. analysts is the number of analysts following a stock and/or issuing recommendations at the end of the previous quarter.
Log analysts	I/B/E/S	Log analysts is the natural logarithm of one plus the number of analysts.
Average absolute forecast error	I/B/E/S	Absolute forecast error for analysts' one year ahead EPS forecasts averaged over the previous five fiscal years.
Mutual fund ownership	Thomson Reuters S12	Mutual fund ownership is the fraction of shares outstanding owned by open-ended mutual funds at the end of the previous quarter.
Inst. ownership	Thomson Reuters S34	Institutional ownership is the fraction of shares outstanding owned by institutional investors at the end of the previous quarter.
Mfflow	S12 / CRSP MF database	*Mfflow* is the selling pressure by mutual funds experiencing a fire sale as defined in Edmans et al. (2012). See Appendix B for details.
Mfflow complement	S12 / CRSP MF database	*Mfflow complement* is the difference between mutual fund trading pressure by all mutual funds and the selling pressure by fire-selling mutual funds. See Appendix B for details.
Liquidity provision proxy	S12 / CRSP MF database	For each stock, we calculate the aggregated dollar selling volume in that stock by its current fund owners and their simultaneous aggregate dollar buy volume in peer stocks experiencing a fire sale. We then take the minimum of those two numbers to measure liquidity provision by current owners to fire sale funds. The measure is not defined for fire sale stocks. Because values are very small, we multiply the measure by 1,000,000 for better visibility.
Short interest	Compustat	We average the number of shares held short each month (obtained from the Supplemental Short Interest file) in a given quarter, and scale by the total number of shares outstanding.
Fire sale fund share	S12 / CRSP MF database	Fraction of holdings by current owners invested in fire sale stocks. The measure is not defined for fire sale stocks.
Fire sale stock share		Fraction of shares outstanding owned by fire sale funds (i.e., funds with flow < -5%). The measure is not defined for fire sale stocks.
CAPX	Compustat	Quarterly capital expenditures, scaled by lagged property, plant and equipment.

Appendix B: Construction of the Edmans et al. (2012) Mfflow measure

We compute the mutual fund selling pressure proxy for each stock as in Edmans et al. (2012). The same approach is also used in Dessaint et al. (2016). We start from the sample of open-ended U.S. equity mutual funds contained in the CRSP Mutual Fund Database. We exclude sector funds (third letter of CRSP objective code equal to "S")—as they could suffer from reverse causality—and drop all international, municipal, bond and metal funds (investment objective codes 1, 5, 6, 8).

For all remaining funds, we find monthly total net assets (TNA) and returns (ret). We then compute

$$flow_{j,t} = \frac{(TNA_{j,t} - \left(1 + ret_{t,j}\right) * TNA_{j,t-1})}{TNA_{j,t-1}}$$

at quarterly frequency and construct the *mfflow* measure as

$$mfflow_{i,t} = \sum_{j=1}^{M} flow_{j,t} * \frac{shares_{i,j,t-1} * prc_{i,t-1}}{vol_{i,t}}$$

using only the funds *j* which have *flow<-5%* (called "fire sale funds"). $shares_{i,j,t-1}$ is the number of shares of company *i* owned by fund *j* in quarter *t-1*. ($shares_{i,j,t-1} * prc_{i,t-1}$) gives the total value of investment held in company *i* by fund *j* in quarter *t-1*. $flow_{j,t} * (shares_{i,j,t-1} * prc_{i,t-1})$ gives the "hypothetical" selling volume (in dollars) by fire sale fund *j*. We then sum this hypothetical selling volume over all fire sale funds and scale by trading volume (in dollars) to obtain the *mfflow* measure. Finally, we designate stock-quarter observations in the bottom decile of *mfflow* as "fire sale" events.

Using "hypothetical" rather than actual sales immunizes our approach against selection concerns stemming from funds' endogenous decisions to sell particular portfolio stocks as opposed to others (Ringgenberg et al., 2016). Scaling by dollar volume singles out fire sale events where mutual funds' selling pressure makes up a large fraction of the overall trading volume, ensuring a large price impact.

Finally, as a control variable, we also construct $mfflow\ complement_{i,t}$ as the sum of hypothetical fund sales (and/or purchases) over mutual funds with *flow>-5%* (non-fire sale funds).