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Essays in Institutional Investors, Market Efficiency and the Real Economy

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Abstract

My dissertation consists of three chapters, each of which focuses on how institutional investors trade, impact price efficiency in secondary financial markets and influence the allocation of resources in the real economy.

The first chapter, *Mutual Funds' Fire Sales and the Real Economy: Evidence from Hurricanes*, contributes to the recent debate on whether nonfundamental price variations affect real economic activities, using a novel approach based on hurricanes. The main identification of the paper relies on the following argument. Hurricanes inflict large economic and social costs on the affected locations (Belasen and Polachek, 2008; Deryugina, 2017), and thus create liquidity demand from investors living in disaster zones, inducing them to suddenly withdraw capital from their mutual funds' investments. Inefficiencies in the insurance market make protection against catastrophic events quite limited (Froot, 2001; Garmaise and Moskowitz, 2009), and behavioral biases prevent households to buy adequate insurance products (Kunreuther, 1996). Because investors typically exhibit local preferences (Huberman, 2001) and mutual funds are geographically dispersed, this paper posits and shows that mutual funds located in disaster zones experience $2.5 billion in abnormal outflows following hurricanes. Importantly, mutual funds more exposed to a local clientele, that is those that can operate in one state only, experience larger outflows after the natural disaster. These outflows force mutual funds in the hurricane area to sell their portfolio stocks. However, such abnormal outflows could arise because (local and distant) investors rationally anticipate that the portfolio stocks will be negatively affected, either directly (if they are in the disaster zone) or indirectly (e.g., through supply chain linkages). To avoid this contamination the analysis focuses on stocks of companies that are located outside of the disaster zone, and economically unrelated to any affected stock, both in terms of supply-chain and industry relations. The paper reports two main findings. First, treated stocks experience a significant 7% temporary price drop following hurricane events. Second, firms respond to these price variations by reducing investments. We use an instrumental variable approach, that isolates the non-fundamental variations in Tobin's Q through the fraction of mutual funds holding a stock that is headquartered in the disaster area and report that in the year after the hurricane, treated firms decrease investments by 4% of the average value. Taken together, these results indicate that when the source of outflows is identified ex-ante and stems from sudden investors' liquidity needs, the resulting non-fundamental price variations actually distort firms' real decisions.

The second chapter, *Strategic Trading as a Response to Short Sellers*, co-authored with Marco Di Maggio, Francesco Franzoni, and Massimo Massa, studies whether short selling deters the incorporation of positive information. There is a widespread view in finance that short selling is beneficial for the market because it lets negative information seep into prices, improving price efficiency. However, regulators have not consistently embraced this view, as they fear the distortive effect of short sales for security prices. Therefore, this chapter revisits the question of the effect of short selling on financial markets using, as a convenient laboratory, the period before earnings announcements, a time in which investors potentially disagree on the fundamentals of the asset. For identification, we use the Reg SHO experiment, which induced a release in short-sale constraints in a random subset of listed stocks. We find that, in the case of positive news, the amount of information that prices reflect is 18% lower when short selling is more aggressive. To corroborate the hypothesis that this is the result of strategic behavior, we show that institutional investors slow down significantly their buy trades and break their orders across multiple brokers when short-selling activity is more pronounced. Before earnings announcements, the trading speed of investors' buy trades decreases by 7% for Pilot stocks during the Reg SHO experiment. We observe a shift of buying activity from central and familiar to peripheral and unfamiliar brokers, consistent with a strategic attempt to avoid the information leakage taking place through central brokers. These findings suggest that short-sellers hinder price discovery when better-informed investors are present in the market.

The third chapter, *Institutional Investors and the Announcement of Share Repurchases*, co-authored with Clemens Sialm, focuses on institutional trading around buyback announcements. In recent years, shares repurchases have become an increasingly important means of payout. One often overlooked aspect of share repurchases regards the identity of the investors who are the counterparties of the firms buying back their own stocks. This chapter studies the trading of institutional investors around share repurchases to better understand the impact of share repurchases on the ownership of corporate equities. We find that mutual funds are significantly more likely to liquidate their positions and sell about 1% of a firm's share outstanding in the year after the announcement. The identity of the counterparty is important as the previous literature has documented that the stocks of repurchasing firms tend to outperform the market over the long-term (Ikenberry, Lakonishok, and Vermaelen, 1995, 2000). Thus, the investors who sell their securities to the repurchasing firms may forgo some profitable investment opportunities. Consistent with this view, we find that in the week after the announcement when the stock price increases the most, mutual funds have a lower propensity of selling the announcing firm compared to similar non-announcing firms. The tax status and the capital gains overhang of the liquidating investors influence their tax burden. In line with this, we find that investors in firms that repurchase their shares tend to liquidate the positions with relatively low embedded capital gains, reducing their tax burden.

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One of the first things I learned when I started my doctoral studies was that a Ph.D. is like a marathon. A quote attributed to the former runner Bill Rodgers says: *"Aiming for the marathon is a task of sorts which can include terrific highs and lows"*. Therefore, I was lucky enough to have great people around me while I was running my doctoral marathon.

I will always be grateful to my dissertation advisor, Laurent Frésard, for inspiring and guiding me as a researcher. When I was a first-year student, I fell in love with empirical research after taking his class. Thank you, Laurent, for always having my back. Thank you for your immeasurable support from day zero to the hectic days of the academic job market in the era of Covid-19.

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Chapter 1

Mutual Funds' Fire Sales and the Real Economy: Evidence from Hurricanes

1.1 Introduction

Asset fire sales occur when funds are forced to sell assets quickly in order to meet sudden capital withdrawals (i.e., large outflows) by investors facing unexpected liquidity needs. Because the sales arise on a short notice, capital available in the market may be insufficient to absorb such flow-induced shocks, resulting in prices that are temporarily below their fundamental values, until capital becomes progressively available (Coval and Stafford, 2007; Duffie, 2010). However, if the decision process of firms' managers is influenced by stock prices that are far from their fundamental value, then the allocation of resources may be inefficient (Stein, 1996; Polk and Sapienza, 2009).[1] This paper provides new evidence on the link between nonfundamental price swings and corporate investment (Bond, Edmans, and Goldstein, 2012; Baker and Wurgler, 2013). We do so by shifting the perspective from an ex-post to an ex-ante identification of mutual funds' outflows.

A large number of papers followed the instrumental variable approach introduced by Edmans, Goldstein, and Jiang (2012), based on mutual funds' fire sales, to show that the temporary flow-induced price distortions do have real implications in that they alter various firms' decisions, such as investment, capital structure, takeover activity, or governance mechanisms.[2] The idea is to flag fire-sales events ex-post, by looking at (realized) extreme outflows and, then, use subsequent price pressure to isolate the non-fundamental component of stock prices. Yet, these conclusions have been recently challenged (Berger, 2019; Wardlaw, 2020), because the

[1] More recently, vanBinsbergen and Opp (2019) develop a model in which asset pricing anomalies cause material real inefficiencies in firms' investments.

[2] See e.g. Khan, Kogan, and Serafeim (2012), Phillips and Zhdanov (2013), Norli, Ostergaard, and Schindele (2015), Lee and So (2017), Bonaime, Gulen, and Ion (2018), Eckbo, Makaew, and Thorburn (2018), Lou and Wang (2018), Dessaint, Foucault, Frésard, and Matray (2019).

approach proposed by Edmans, Goldstein, and Jiang (2012) does not properly identify liquidity needs of mutual fund investors that are truly exogenous to the fundamentals of portfolio firms. Therefore, the observed price patterns for stocks exposed to mutual funds' outflows may actually respond to fundamental information.[3]

We isolate temporary nonfundamental price drops by identifying the actual origin of capital withdrawals (ex-ante) and showing that they are exogenous to firms' fundamentals. In particular, we focus on the liquidity needs of mutual fund investors created by large and damaging hurricanes hitting various locations in the United States. The main identification of the paper relies on the following argument. Hurricanes inflict large economic and social costs in the affected locations (Belasen and Polachek, 2008; Deryugina, 2017), and thus create liquidity demand from investors living in disaster zones (e.g., to cover house repairs, relocation, or health expenses). Inefficiencies in the insurance market make protection against catastrophic events quite limited (Froot, 2001; Niehaus, 2002; Garmaise and Moskowitz, 2009), and behavioral biases prevent households to buy adequate insurance products (Kunreuther, 1996). To cope with damages, they suddenly withdraw capital from their mutual funds' investments.

Because investors exhibit local preferences (Grinblatt and Keloharju, 2001; Huberman, 2001; Seasholes and Zhu, 2010; Ivkovic and Weisbenner, 2003), mutual funds located in disaster zones experience abnormally large outflows following hurricanes, forcing them to sell portfolio stocks. Such abnormal outflows could arise because (local and distant) investors rationally anticipate that the portfolio stocks will be negatively affected, either directly (if they are in the disaster zone) or indirectly (e.g., through supply chain linkages).[4] To address this concern and correctly isolate nonfundamental shocks, we exploit the variation in exposure to mutual fund ownership in the disaster area of firms not affected by the natural event, both geographically and economically.

We implement this novel approach using a panel covering $3,822$ U.S. mutual funds and $11,493$ U.S. stocks, with the former headquartered in 126, and the latter in 437 distinct locations (i.e., Core-based Statistical Areas - CBSAs) and focusing on the fifteen most damaging hurricanes between 1989 and 2008 (cumulative damages over \$350 billion). We consider only the set of stocks of companies that are (i) located outside of the disaster zone, and (ii) economically unrelated to any affected stock, both in terms of supply-chain and industry relations. Stocks held by funds located in the disaster zone are labeled as "treated". In particular, treated stocks are those for which our novel instrument, defined as the number of funds holding a stock and headquartered in the disaster zone divided by the total mutual fund ownership

[3]In particular, Wardlaw (2020) shows that, by construction, the measure used to identify firms exposed to fire-sales accidentally includes the stock's quarterly return, which eventually drives the price pressure. In addition, Berger (2019) suggests that large outflows are non-random as there are fundamental differences between the firms exposed to fire-sales and those used as control group.

[4]For example, a recent paper by (Dou, Kogan, and Wu, 2020) suggests that mutual funds experience an increase in outflow risk in the subsequent quarters when the stocks in their portfolios are negatively affected by natural disaster shocks.

for that firm, is above the 75th percentile of its distribution. By construction, this measure is bounded below at zero and takes positive values for stocks held by funds located in the disaster zone and with no links to the hurricane events.

We report two main findings. First, treated stocks experience significant temporary price declines following hurricane events. Second, firms respond to these price dislocations by reducing investment. We also address the concerns raised on the traditional measure of mutual fund pressure. In particular, we define our measure such that it is not mechanically affected by the contemporaneous stock return. We also show that our findings are not driven by past stock returns, suggesting that we are truly isolating a nonfundamental origin of fund flows. Finally, using a homogeneous sample, as suggested by (Berger, 2019), or using matching techniques do not alter our results. Taken together, our findings indicate that when the source of outflows is identified ex-ante, and stems from sudden investors' liquidity needs unrelated to fund performance, the resulting nonfundamental price dislocations actually distort firms' real decisions.

To validate the analysis and interpretation, we first show that mutual funds have a significant local clientele. For instance, we report that the time-series variation in flows exhibits a strong local component (identified using location-time fixed effects), and is significantly related to variation in local economic activity (e.g., house prices or unemployment rates). Moreover, the correlation between funds' flows and local economic activity is particularly strong for funds that only operates in one state, for which investors are more likely to be exclusively local.

Second, using a difference-in-differences approach, we show that hurricanes cause large outflows for all funds headquartered in affected locations relative to unaffected funds of about 1.35-2% in the event quarter. This represents an abnormal quarterly outflow of $16.15 million for the average affected fund, and $2.5 billion aggregated across all affected funds. While outflows experienced by affected funds truly concentrate in the hurricane quarter, the outflows do not revert over time, indicating that hurricanes permanently lower the size (i.e., total net assets) of the affected funds. Notably, we show the absence of any pre-trend in mutual fund flows before the event quarter, confirming that the abnormal outflows are actually generated by the hurricane.

The flow-hurricane sensitivity holds when we compare funds located in the same state (with the inclusion of state-quarter fixed effects) that differ only for whether they are headquartered in affected areas, and in specifications in which affected and unaffected funds are matched on their characteristics (TNA, past returns and flows, and expense ratio), or using a homogeneous sample with funds hit by the hurricane serving as their own control group when they are not actually affected (Michaely, Rubin, and Vedrashko, 2016). Further mitigating possible selection issues, we show that, prior to hurricanes, funds in the disaster zone are comparable to non-affected funds in terms of their own characteristics (e.g., size, performance, turnover, style) and that of their portfolio stocks (e.g., size, or liquidity).[5]

We then turn to the sample of stocks unrelated to the disaster and estimate a dynamic difference-in-differences regression around hurricane events, with firm and time fixed effects, where the dependent variable is the monthly abnormal DGTW returns.[6] We document a price response for treated stocks in the months following the hurricane. The stock price starts decreasing as soon as the hurricane hits and, after five months, we report a cumulative drop in abnormal returns of 7%. Such a dislocation is however almost completely reversed within ten months, suggesting that the deviation from fundamentals is actually temporary. This reversal pattern is faster than the one identified in previous literature, which is usually of about 24 months, further suggesting that our approach truly identifies a liquidity shock exogenous to firms' fundamentals.[7] Notably, after the recovery, prices stabilize to their fundamental values and there is no difference between the treatment and control groups in months [15, 48] after the event. In the cross-section, we document that these results are more prominent for smaller and less liquid firms.

In a series of robustness tests, we show that selection bias is unlikely to drive the price pattern we observe for treated stocks after a hurricane event. In particular, the temporary nonfundamental price drop is confirmed in a subsample of firms with positive institutional ownership, which we use as a proxy for unobservable firm characteristics, as institutions might pick stocks for which they have superior information (Berger, 2019). Moreover, matching treatment and control stocks on size and institutional ownership does not alter the results significantly. We also find that treated and control firms do not differ with regards to many characteristics.

Finally, we study whether these temporary deviations of prices from fundamentals have real effects, by analyzing investments in the year after the hurricane. Investment is the most widely studied firm policy in the literature on the real effects of finance (e.g. Chen, Goldstein, and Jiang, 2007; Foucault and Frésard, 2014), therefore, we can compare our novel evidence to previous results. Moreover, the question of whether nonfundamental shocks affect the real economy primarily entails the efficient allocation of resources, of which managers' investment decisions are the most prominent example (Dow and Gorton, 1997). In line with previous research, we adopt an instrumental variable approach, that assesses the presence of real effects using our novel instrument to isolate the nonfundamental variations in Tobin's Q (i.e., the firm's normalized stock price). Intuitively, the coefficient of the instrumented Q - the nonfundamental component of stock prices - should be zero

[5] As pointed out by (Berger, 2019), when identifying large outflows that drive mispricing, the assumption is that fund flows are exogenous to firm characteristics. However, if affected and unaffected funds differ for their trading styles, than this identifying assumption fails to hold.

[6] As suggested by (Wardlaw, 2020), this avoids any mechanical effect due to stocks characteristics.

[7] Recently, (Bogousslavsky, Collin-Dufresne, and Sağlam, 2020) have shown that in settings where nonfundamental trading is clearly distinguishable (the occurrence of a trading glitch at a high-frequency market-making firm) from informed trading, the reversal is much faster (one day). Nevertheless, our setting is different and the slower price reversal is justified not only by the slow moving of capital, but also by the fact that the liquidity shock analyzed in this paper actually has real effects, which amplify and reduce the speed of the reversal.

if investments are not affected by the liquidity shock. We report that, in the year after the hurricane, treated firms respond to the price pressure through a reduction in investments, measured by total capital expenditure as a percentage of property, plant and equipment, of about 4% of the average value.

Importantly, adopting alternative definitions of the instrument, closely related to the approach of (Edmans, Goldstein, and Jiang, 2012), does not substantially change the results. Finally, the decrease in investments for treated firms is confirmed in a more homogeneous sample, where treated firms serve as their own control in periods where hurricanes do not hit.

This paper primarily contributes to the literature on the real effects of secondary financial markets[8] and fire sales[9] by proposing an economically-grounded channel for the origin of fund outflows, through which it provides novel evidence on the link between nonfundamental price shocks and firms' investment decisions. Using a unique setting, we contribute to the recent debate (Berger, 2019; Wardlaw, 2020), as we address one of the main drawback of the traditional approach, that is, the inability of ruling out that outflows are not indeed caused by (informed) mutual fund investors expecting low future performances. Moreover, the results of this paper are consistent with many non-mutually exclusive mechanisms discussed by previous literature (e.g. learning, corporate governance, financial constraints)[10] We propose the *strategic hypothesis* as a possible channel that rationalizes our findings. In particular, we provide evidence that the managers of treated firms understand that the price drop will be temporary and move resources from investments to the repurchase of shares, which became a cheap activity due to the temporary price pressure. This allows them to correct the mispricing (Peyer and Vermaelen, 2009) and signal the market that the shock is nonfundamental (Massa, Rehman, and Vermaelen, 2007).

These findings are also consistent with the theory outlined in (Gabaix and Koijen, 2020) that idiosyncratic shocks can generate flows that eventually affect prices in a sizable manner. In our setting, hurricanes generate outflows for an amount equal to \$4.16 billions in the five months after the event. This translates into \$18.4 billion of market value destroyed for firms unrelated to the hurricane but held by affected mutual funds, which corresponds multiplier of about 4.4[11], in line with the calibration and estimation of (Gabaix and Koijen, 2020). Moreover, the share repurchase

[8]In particular, we focus on the real effects of nonfundamental shocks to stock prices. For a review on this topic see (Baker and Wurgler, 2013)

[9]Some relevant contribution to the fire sales literature include Coval and Stafford (2007), Frazzini and Lamont (2008), Duffie (2010) Ben-Rephael, Kandel, and Wohl (2011), Lou (2012), Shive and Yun (2013), Akbas, Armstrong, Sorescu, and Subrahmanyam (2015), Barbon, Di Maggio, Franzoni, and Landier (2019), Chernenko and Sunderam (2020).

[10]The learning channel is outlined by (Chen, Goldstein, and Jiang, 2007). An extension of the learning channel is the faulty informant channel of (Dessaint, Foucault, Frésard, and Matray, 2019), where managers have limited ability to separate information from noise. (Polk and Sapienza, 2009) introduce the catering channel according to which corporate governance relations play a role in the investment sensitivity to nonfundamental shocks. Finally, (Baker, Stein, and Wurgler, 2003) propose the financial constraint channel, according to which the sensitivity is higher for "equity-dependent" firms.

[11]A multiplier of 4.4 means that outflows of \$1 depress market value by \$4.4.

activity that follows the price depression can be seen as a flow of opposite sign that brings the price back to its fundamental value.

Our findings also add to the literature on the relevance of geography for finance. A large body of work discussed home bias - the propensity of investors to allocate most of their funds to stocks headquartered within a close geographic proximity.[12] This paper adds to this strand of literature by uncovering a particular type of home bias; namely, the presence of a local clientele of mutual funds, which creates frictions as local shocks arise.

Finally, this paper relates to the literature on the propagation of idiosyncratic shocks within a network (Gabaix, 2011; Barrot and Sauvagnat, 2016; Herskovic, Kelly, Lustig, and Van Nieuwerburgh, 2020). We contribute to this literature by showing that between retail investors and mutual funds there exists a customer-supplier network within which shocks get amplified and negatively affect the real economy.

The paper proceeds as follows. Section 1.2 describes the data used in the analysis, while Section 1.3 discusses the identification strategy. Section 1.4 focuses on showing that mutual funds have a significant local clientele. The impact of hurricanes on mutual fund flows is studied in Section 1.5, while the effects on prices and real decisions are described in Sections 1.6 and 1.7, respectively. Section 1.8 concludes.

1.2 Data

This section briefly describes the main sources for data used in the analysis. Further details can be found in the appendix. We refer to Table A.1 for a description of the variables used throughout the paper.

Mutual fund data are the common 1980-2017 sample from the CRSP Survivor-Bias-Free US Mutual Fund and Thomson Reuters (TR) s12 (formerly CDA/Spectrum). The final sample comprises 3,822 funds with quarterly observations between 1980 and 2017. Panel A of Table 1.1 reports summary statistics for the main variables of interest. Figure 1.1 shows the geographic distribution of mutual funds across the United States. Panel A displays in red the CBSAs with at least one fund, while we distinguish between CBSAs with less than 1 billion in total net assets, and those where the funds in aggregate breach this threshold. While the sample covers only 126 CBSAs out of the 923 in which continental US is divided, mutual funds are pretty dispersed and not concentrated in few regions only. This is a key point for our identification. Were mutual funds concentrated in few areas only, then the presence of a local clientele would have been utterly unlikely, as the investor base is broadly dispersed.

[12] A non-extensive list of papers on home bias, which affects not only professionals but also retail investors and analysts, includes (French and Poterba, 1994), (Coval and Moskowitz, 1999), (Coval and Moskowitz, 2001), (Hau, 2001), (Pirinsky and Wang, 2006), (Ivkovic and Weisbenner, 2003), (Grinblatt and Keloharju, 2001), (Seasholes and Zhu, 2010), (Massa and Simonov, 2006), (Malloy, 2005), (Bae, Stulz, and Tan, 2008), (Van Nieuwerburgh and Veldkamp, 2009), (Sialm, Sun, and Zheng, 2019).

For the sample of US firms we use CRSP MSF and CRSP-Compustat annual file from 1980 to 2017 to match the availability of mutual fund data. We select ordinary non-financial shares traded on the NYSE, NASDAQ, or AMEX stock exchange. The final sample is made of $11,493$ firms, for which summary statistics at the annual level are shown in Panel B of Table 1.1.

Figure 1.2 shows the geographic distribution of firms across the United States. The sample covers 437 CBSAs, and similarly to mutual funds, firms appear to be quite scattered across the United States.

Hurricanes names, dates, and county location are obtained from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) at the Arizona State University.[13] In order to have meaningful events, we follow Dessaint and Matray (2017) and select hurricanes with total direct damages (adjusted for inflation) above five billion dollars. Table A.2 describes the hurricanes in the sample and reports the name, year, landfall date, number of fatalities, and damages in billion of dollars (both raw and adjusted to CPI in January 2020). We also report the composition of treated and control groups for both the funds and firms samples. Not surprisingly, hurricane Katrina was the most devastating event with $1,500$ fatalities and \$142.54 billions in damages. Nevertheless, Katrina hit only 123 funds, or 5.94% of the industry.

Mitigating concerns on our identification strategy, Panel (a) of Figure 1.3 shows that hurricanes randomly affect a large fraction of the US mainland. Moreover, previous literature suggests that "estimating the marginal increase in the local probability of hurricane landfall in response to the occurrence of a hurricane over the past two years produces a statistically insignificant coefficient that is negative or equal to zero" Dessaint and Matray, 2017, p.98. This is consistent with the climate literature that finds that, in US mainland, hurricane frequency has been mostly stationary (Elsner and Bossak, 2001; Pielke, Gratz, Landsea, Collins, Saunders, and Musulin, 2008).

Furthermore, disaster areas are scattered through time. As anecdotal evidence, we show in Panel (b) and (c) of Figure 1.3 that the portion of US mainland hit by hurricane Katrina in 2005, is generally different from that where hurricane Floyd struck six years before. In addition, hurricanes are well suited for the analysis proposed in this paper because their occurrence is likely exogenous to funds, retail investors and firms. Therefore, variations in prices and corporate policies observed after a hurricane, especially in firms unrelated to the disaster, cannot easily be attributed to unobserved heterogeneity or reverse causality.

All the tests in this paper rely on the identification of funds' and firms' headquarters[14] in terms of Core-Based Statistical Area (CBSA). CRSP Mutual Fund and

[13] Detailed information about their characteristics is from the archive section of the National Hurricane Center (NHC) website and the 2011 NOAA Technical Memorandum by Blake, Landsea, and Gibney (2011).

[14] One concern is that Compustat only reports the current county of firms' headquarters. However, Pirinsky and Wang (2006) show that in the period 1992–1997, less than 3% of firms in Compustat

CRSP-Compustat stock file provide firms' and funds' zip-codes, respectively. We link zip-codes to county codes and then to CBSA codes using the cross-walks provided by the Census Bureau and the U.S. Department of Housing and Urban Development (HUD).

Other variables used in the analysis include CBSA-level macroeconomic indicators such as the quarterly unemployment rate, and the quarterly house price index. The former is from the Bureau of Labor Statistics, while the latter is downloaded from FRED.

Finally, we identify funds registered in one state only, by using Form ADV from the SEC, available at https://adviserinfo.sec.gov/ (IAPD). We match this information to the main fund sample using fuzzy match on fund name, followed by a manual check. This procedure is able to match roughly 62% of the original sample to the Form ADV information. We use the fact that a fund is registered in only one state to proxy for the fact that it is more likely to have a local clientele (e.g., because it focuses most of its marketing in that state). We use the smaller sample that results from the fuzzy match in tests based on where the fund operates.

1.3 Empirical Strategy

This section describes the identification strategy of the paper. Throughout the main analysis, we use both a difference-in-differences approach and instrumental variable regressions. For each hurricane event, we draw treatment and control stocks from a sample that comprises all non-financial firms headquartered in any of the CBSAs not hit by the hurricane, and satisfying the following requirements: *(i)* the firm does not have any customer-supplier link with any firm located in the disaster zone, and *(ii)* the firm is not active in industries adversely affected by the hurricane. The first requirement is based on the identification of customer-supplier links using the approach of Barrot and Sauvagnat (2016).[15] Industries most affected by a hurricane are determined by computing, for each natural event, the fraction of total number of firms in one of the 48 Fama-French industries, which are headquartered in the disaster zone. The industries are then ranked by this measure and the most affected industries are the 10 displaying highest values.

The definition of the treatment and control groups relies on a novel instrumental variable, *Hurricane Hypothetical Sales (HHS)*, which proxies for mutual fund pressure after a hurricane event. There are $i = \{1, \ldots, n\}$ firms held by $j = \{1, \ldots, m\}$ mutual funds. A hurricane can hit location (CBSA) l in quarter t. Then, for each event, we define:

changed their headquarter locations. Moreover, plant location instead of headquarters might better address the question of the paper. Unfortunately, we do not have access to those data. However, Chaney, Sraer, and Thesmar (2016) show that most of the firms' real estate is located in the headquarter.

[15]The linking file is available on Barrot's website at: https://mitmgmtfaculty.mit.edu/jnbarrot/data/.

$$HHS_{i,t} = \frac{\sum_{j=1}^{m} \mathbb{1}\{(Location_{j,t} = l) \,\&\, (Holdings_{i,j,t-1} > 0)\}}{\sum_{j=1}^{m} \mathbb{1}\{(Holdings_{i,j,t-1} > 0)\}}, \tag{1.1}$$

where, $\mathbb{1}\{A\}$ is an indicator variable equal to 1 if condition A is satisfied, and zero otherwise. $Location_{j,t}$ is the fund's headquarter at the beginning of event-quarter t, and $Holdings_{i,j,t}$ is the number of shares of firm i held by fund j in quarter t. In other words, HHS measures the number of funds headquartered in the disaster area that hold firm i as a fraction of the total number of funds holding the stock at the beginning of the hurricane quarter. For instrumental variable regressions, we use HHS as a continuous variable with values in the interval between zero and one. For the difference-in-differences analysis, the treatment group is based on an indicator for HHS greater than the 75th percentile of the across-events distribution. The other firms serve as control group.

Hurricane Hypothetical Sales is, in spirit, similar to the instrument introduced by Edmans, Goldstein, and Jiang (2012) in that it does not look at actual sales, but assumes increased price pressure to correlate with the number of affected funds holding a stock. Actual sales are not a valid instrument, because fund managers either deviates from proportional trading for liquidity reasons (Lou, 2012) or because they trade on information (Huang, Ringgenberg, and Zhang, 2016).

For comparison with existing literature, we define two additional measures of mutual fund pressure. The first, *Hurricane Induced Flow (HIF)* follows the approach of Edmans, Goldstein, and Jiang (2012) and takes into account the caveats of Wardlaw (2020). The definition is as follows:

$$HIF_{i,t} = \sum_{j=1}^{m} \frac{flow_{j,t} \times Holdings_{i,j,t-1}}{Shares\ Outstanding_{i,t-1}} \times \mathbb{1}\{(Location_{j,t} = l) \,\&\, (flow_{j,t} < 0)\}, \tag{1.2}$$

where $flow_{j,t}$ is the net dollar flow received by fund j in quarter t, scaled by beginning-of-period total net assets.

However, as noted by Schmickler (2020), there is an additional concern regarding the standard flow-induced trade measure, that is, a reverse causality driven by the contemporaneous correlation between quarterly fund flows and the returns of the stocks in their portfolios. The reverse causality argument goes that instead of outflows inducing fire sales, which drive down prices, poor stock returns reduce mutual fund returns, which in turn trigger outflows. To overcome this issue, the author proposes a new measure which isolate the random liquidity shock component of fund flows. This component, *surprise flow*, is the residual of a cross-sectional regression of fund flows onto past flows and returns.

We adapt this measure to the context of this paper and define *surprise flow* as the coefficient of the regression of quarterly fund flows onto an indicator equal to 1 if the fund is headquartered in the hurricane area at the beginning of the disaster quarter. Intuitively, here, the surprise stems from the occurrence of hurricanes because, not only do retail investors redeem their shares of local mutual funds for reasons unrelated to the fundamentals of the fund's portfolio, but the occurrence of a hurricane has no predicting information about the probability of future events (Dessaint and Matray, 2017; Elsner and Bossak, 2001; Pielke, Gratz, Landsea, Collins, Saunders, and Musulin, 2008). Therefore, we define *Hurricane Induced Surprise Flow (HISF)* as,

$$HISF_{i,t} = \sum_{j=1}^{m} \frac{surprise_{j,t} \times Holdings_{i,j,t-1}}{Shares\ Outstanding_{i,t-1}} \times \mathbb{1}\{(Location_{j,t} = l)\}. \tag{1.3}$$

Summary statistics for the three measures at the firm-year level are shown in Panels B of Table 1.1.

Equation (1.1) does not condition on the fraction of market value of a stock held by a fund, hence the measure is immune to the critique in Wardlaw (2020) as, by construction, the stock return does not appear in equation (1.1). In general, if the instrument can be predicted by past returns, the exclusion restriction fails to apply because the fire sale might happen for fundamental motives, that show up in the stock returns. To further assess whether *HHS* is free from this bias, columns (1)-(3) of Table A.3 regresses the quarterly realizations of the variable in equation (1.1) onto past-quarters stock returns. Our estimates clearly reject the hypothesis that past returns predict *HHS*, mitigating the concerns on the validity of the instrument. Interestingly, the same analysis on *HIF* and *HISF* (columns 4-9) shows some form of predictability, further suggesting that the novel instrument proposed in this paper allows for the cleanest identification.

1.4 The Local Clientele of Mutual Funds

Our identification strategy relies on the assumption that, following a hurricane, retail investors have liquidity needs and withdraw money from the mutual funds they are invested in. In particular, we hypothesize that these investors have savings invested in mutual funds located in the disaster zone. In other words, we postulate the presence of a local clientele of mutual funds. This is a key point of our identification strategy, as non-local investors might withdraw money from affected funds because they anticipate poor future performance, while this is less likely to happen for investors living in the disaster zone who have to face the direct costs of the hurricane.

Past literature (French and Poterba, 1994; Coval and Moskowitz, 1999; Coval and Moskowitz, 2001; Pool, Stoffman, and Yonker, 2012) extensively discussed the home bias in mutual funds portfolio or in the investment choices of individual investors (Grinblatt and Keloharju, 2001; Huberman, 2001; Seasholes and Zhu, 2010; Ivkovic and Weisbenner, 2003), while the particular type of home bias outlined in this paper has been overlooked. A recent paper by Sialm, Sun, and Zheng (2019) looks at a situation similar to the one we address in our analysis, by showing that funds of hedge funds overweight hedge funds located in the same geographical area. While this provides better performance, it generates destabilizing flow comovements and return clustering within geographical areas.

To address the question of whether mutual funds exhibit a local clientele, we run two main tests. The first draws from Bertrand and Schoar (2003), and looks at the importance of location fixed-effects in standard flow regressions. The idea behind this test is that if a fund has unobservable drivers that correlate with its headquarter, then adding location fixed-effects should increase the R-squared of the regression. We argue that one of these unobservable drivers of fund flows is the geographical origin of the mutual fund clientele. The second test studies the correlation between fund flows and the state of the economy in the CBSA where the fund is headquartered. Intuitively, a negative (positive) shock to local economic condition might induce retail investors to withdraw (invest) money from (in) their mutual funds, making the correlation apparent.

Table 1.2 shows results for the first test. We report adjusted-R2 and number of observations for two different models. The first is the one used by Coval and Stafford (2007) and regresses current quarterly mutual fund flows onto past eight-quarters flows and fund returns. The second is a model with similar interpretation, but less demanding in terms of number of observations as it includes fewer lags, which incorporate as explanatory variables the past quarter return, the return volatility in the past 12 months, the fund's log-TNA, the total expense ratio, and fund's turnover ratio (Franzoni and Schmalz, 2017). For each model, we report three specifications. The first (row 1) includes the interaction fixed-effect between location and time, together with the fund FE. The second (row 2) includes location fixed-effect on top of the firm and time dimensions, while the third (row 3) is the base-line specification with fund and time fixed-effects. We run specifications where the dependent is a continuous variable for fund flows (columns 1-2), a dummy variables for extreme inflows ($> 90^{th}$ percentile of fund flows distribution) or extreme outflows ($< 10^{th}$ percentile of fund flows distribution). Notably, in every specification we report a non-negligible increase in the adjusted-R2 as we move from row 3 back to row 1. For example, in the specification with continuous dependent variable and mutual fund characteristics (lower-left part of the table) the adjusted-R2 goes from 12.49% in the baseline specification, to 14.53% in the regression in which location and time fixed effects are interacted, which corresponds to a 16.3% increase. These results are indicative of the presence of a local component in mutual fund flows, which is likely to be driven by a clientele concentrated in the area where the fund operates.[16]

Next, to test the correlation between fund flows and the state of the local economy, we use two proxies for the latter; that is, the unemployment rate and the house

[16]For the ease of reading, we do not include F-tests for the joint significance of the fixed-effects, which nonetheless display a significant role of location FEs. However, the econometric interpretation of the F-tests in this context is troublesome since, as noted by Wooldridge (2010) and Fee, Hadlock, and Pierce (2013), standard asymptotic theory does not apply, and the properties of standard F-tests for joint significance of the coefficients on these variables are unknown.

price index (HPI). Both variables are at the CBSA-quarter level and lagged 1 quarter, to allow retail investors to respond to the new state of the economy. Intuitively, lower (higher) unemployment rate (HPI) is a sign of improvement in the state of the local economy. Table 1.3 shows the results. We use as dependent variable either fund flows in percent of TNA (columns 1-2 and 5-6) or an indicator for outflows ($flow < 0$). We control for total expense ratio, fund turnover, log-TNA, past quarter return, and past 12-month volatility. Fund, time and location fixed effects are also included in the regressions, and standard errors are clustered at the location-quarter level to take into account that the explanatory variable does not change across funds within this dimension.

The coefficients on the proxies for the state of the local economy are statistically and economically significant. For example, a one standard deviation increase in unemployment rate decreases flows by 50 bps or 73.3% of the sample average. The result for house price index is similar although slightly smaller in magnitude, with an increase in flows of roughly 35 bps per standard deviation increase in HPI.

Finally, we study whether the results in Table 1.3 are stronger for funds whose clientele is more likely to be local. The best available proxy comes from Form ADV. Investment advisers shall register either with the state regulator or the SEC and declare the geographies in which they operate. Using this information, we construct an indicator equal to 1 if the fund operates in one state only; that is, it is more likely to have a local clientele. We then test the hypothesis that fund flows are more sensible to the state of the local economy by running a linear probability model where a dummy for outflows is the dependent variable, and the main explanatory variable is the interaction between *One State*, an indicator equal to 1 if the fund operates in one US state only, and *Improved Economy*, an indicator for improvements in the state of the local economy across two adjacent quarters (negative change in unemployment rate and positive in hpi). Results are shown in Table A.4. Consistent with the hypothesis, we find that outflows are consistently less likely if the state of the local economy improves. For example, an increase in house price index is 3.5% less likely to generate an outflow if the fund is registered in one state only.

Taken together, the results in this section suggest that one driver of mutual fund flows is a geographic component that refers to the area in which the fund operates.This sheds some evidence on the presence of a local clientele of mutual funds and justifies the main hypothesis of this paper: the trigger for mutual funds' outflows following a natural disaster is the local clientele of the mutual funds.

1.5 Hurricanes and Mutual Fund Flows

This section tests the hypothesis that the occurrence of a hurricane generates outflows from mutual funds headquartered in the disaster area, using generalized difference-in-differences regressions. The treated group is composed of all funds located in one of the CBSAs hit by the hurricane (affected funds), while the control group consists of all the other funds.

The main specification is as follows:

$$\begin{aligned} flow_{j,q} = \alpha_j + \gamma_q + \zeta_l + \beta_1 Disaster\ zone_{j,q-4,q-1} + \beta_2 Disaster\ zone_{j,q} \\ + \beta_3 Disaster\ zone_{j,q+1,q+4} + \beta_4 Disaster\ zone_{j,q+5,q+8} \\ + \beta_5 Disaster\ zone_{j,q+9,q+12} + \sum_{c=1}^{C} \theta_c X^c_{j,t} + \varepsilon_{j,q}\ , \end{aligned} \tag{1.4}$$

where $Disaster\ zone_{j,s,t}$ is a dummy variable equal to one if the fund, at quarter start, is located in a CBSA hit by a hurricane during quarter q and the observation is recorded in quarters $[s, t]$ around the disaster, with $s \leq q \leq t$. The set of control variables, $X_{j,t}$, includes the fund's total expense ratio (TER), log-TNA, volatility of fund returns in the previous 12 months, and the fund's return in quarter *q-1*. α_j, γ_q, ζ_l represent fund, time, and location fixed effects, respectively. Location is either the CBSA or the state in which the fund headquarters, and we allow for interactions between different fixed effects. Standard errors are double clustered at the fund and time level.

The results are summarized in Table 1.4. The null hypothesis of no effect of hurricane on fund flows is strongly rejected in all the specifications. In the hurricane quarter, affected funds experience flows between 1.35 and 2.01 percentage points lower than the control group, depending on the specification. Notably, the inclusion of the highly stringent state-time fixed effects (columns 5-6) yields a negative and strongly significant coefficient for the event quarter. This specification addresses the concern that, because different states might have insurance regulations which are not perfectly aligned[17], the difference-in-differences estimator compares flows of funds with clienteles exposed to different laws - and, hence, incentives - when it comes to liquidating their fund investments.

Table 1.4 shows that, while affected funds suffer most of the additional outflows in the quarter when the hurricane hits, flows continue to be abnormally low also in the subsequent quarters, as we find significant coefficients also between five and twelve quarters after the disaster. On the one hand, this might be driven by the fact that households react slowly to hurricanes, as they will incur in most of the hurricane-related costs only after some time.[18] On the other hand, these outflows

[17]Insurance regulation in the United States is managed by the National Association of Insurance Commissioners (NAIC), which develops regulatory standard that, even though are usually widely adopted by individual states, do not have the force of law, and in principle states could develop their own regulation. It is worth noting that the Dodd-Frank Act, passed in 2010, made regulations more homogeneous across states.

[18]A recent study by Baker and Hermann (2017) finds that the bulk of the spending from losses related to natural disasters will occur only after 2-3 years. Similarly, Turnham, Spader, Khadduri, and Finkel (2010) surveyed the exterior conditions of properties damaged by Hurricanes Katrina and Rita, and found that many properties continued to show observable damage several years after the storms had passed. By 2010, five years after the disasters, 17% of hurricane-damaged properties in Louisiana and Mississippi still showed substantial repair needs.

might just be a result of current flows responding to past flows, as reported in Coval and Stafford (2007).

Further, we analyze the magnitude of the hurricane-driven abnormal outflows. The outflow must be severe enough to force funds to liquidate their portfolios. We address this question by multiplying the average and total industry TNA of the treated funds by the coefficient of a difference-in-differences regression similar to that of Table 1.4, but that includes individual quarters dummies. Results of this exercise are shown in Table A.5. Using January 2020 dollars as a reference, on average, treated funds experience outflows that are $16.15 million bigger than the control funds during the event quarter. This equals roughly 6% of the size of the median fund. At the industry level, after one year, the cumulative effect of the hurricane downsizes the group of treated funds by $6.7 billion, which makes up for about one quarter of the average total damage of a hurricane as reported in Table A.2 ($26.68 billion).

The analysis in this section lies on the assumption that funds affected by the hurricane experience bigger outflows because they have a local clientele. This investor face adverse economic outcomes after the disaster and withdraw their money invested in the affected funds. If this is the case, following a hurricane event, treated funds that are more likely to have a local clientele must display even bigger outflows. This is the conjecture we test in Table A.6, where fund flows are regressed onto the interaction of the difference-in-differences dummy (*Disaster zone*) and another indicator that proxies for the presence of a local clientele (*Local clientele*). Fund, time, and location fixed effects are also included in the regressions, and the level of *Local clientele* is subsumed by the fund fixed effects. When included, the set of controls is made of the same variables used in Table 1.4.

We proxy local clientele in two ways. First, we look at the correlation of outflows with unemployment rate within each CBSA, and set the indicator equal to one if the t-statistics of the regression is greater than two in absolute value. Intuitively, CBSAs with higher t-statistics have funds that respond more to the state of the local economy, and are more likely to have a local clientele. Second, we use the subsample of funds for which we can match a Form ADV report, and set the indicator equal to one for those mutual funds registered in one state only. Results for the first proxy are shown in columns (1)-(4), while those for the latter are displayed in columns (5)-(8). The results confirm the underlying hypothesis that funds with a local clientele are hit more strongly by the hurricane. For these funds, we estimate flows which are between 0.94 and 2.14 percentage points lower than those observed for treated funds bought by non-local investors.

To address possible selection biases, we test whether, before the event, treatment and control funds are comparable in terms of their characteristics. Results are shown in Table A.7. For each variable, we report the pre-event mean for the treatment and control group, together with a t-test for the differences. The t-statistic for double clustered standard error at the fund and quarter level are report below the t-test.

We find that the treatment and control groups do not differ both in terms of fund (flow, return, TNA, turnover, return volatility), and portfolio characteristics (number of stocks held, stock size, stock turnover). The only exception is the fund's total expense ratio for which we report a significant 10 bps higher value for the treatment group.

Berger (2019) suggests that, since mutual fund regulations require that funds commit to broad investment strategies that correlate with firm characteristics, if the funds exposed to severe outflows have trading styles that differ from those in the control group, than firms characteristics matter in explaining which firm will experience a fire sale. Therefore, to further validate the analysis, we test the null hypothesis of no difference between the style of affected funds and funds headquartered outside the hurricane area. We divide funds in eight categories by investment style, namely income, hedged, growth, growth and income, large cap, mid cap, small/micro cap and no-category, and run a t-test for the difference in the fraction of funds that are in each group for treated and control funds. Table A.8 shows that for each of the eight categories, we cannot reject the null hypothesis that treated and control groups are equal in terms of fund style. This result suggests, to a greater extent, that selection biases is not likely to be a concern in the analysis of this paper.

Finally, we address any residual concerns regarding the compositions of the treatment and control groups of funds in two additional robustness tests. The first is a generalized difference-in-differences where each treated fund is matched to the two closest control funds by TNA, flow, return, and expense ratio using nearest neighborhood matching with replacement, based on observations recorded one quarter before the hurricane. The second robustness test constructs the time series of fund-quarters for the treated funds only. Therefore, affected funds serve as the control group for themselves when the diff-in-diff dummy is equal to zero. This approach borrows from the insights of Michaely, Rubin, and Vedrashko (2016) and Berger (2019). Tables A.9 and A.10 show that, even when we impose more stringent requirements for our econometric specification, we can confirm that hurricanes induce outflows from funds headquartered in the disaster zone.

1.6 Hurricanes and Stock Returns

The next step is to assess whether the hurricane-induced outflows are the origin of a liquidity shock to firms linked to the disaster zone only through the mutual funds' portfolios. Intuitively, the hypothesis is that the abnormal outflows estimated in the previous section generate a fire sale which moves the stock price away from its fundamental. If the trade occurs only for liquidity reason and there is no information attached to it, then we should expect the price to revert back to its long-run average after some time. Therefore, to better address such conjecture, we focus on stocks unrelated to the hurricane area, as those that have any link - be it geographical or economical - to the natural disaster might experience price dislocations that are actually driven by fundamental reasons.

We test this hypothesis using a difference-in-differences model similar to that in equation (1.4). However, this time we focus on the subsample of stocks unrelated to the hurricane and with the treated and control groups defined as described in section 1.3. The dependent variable is the monthly stock abnormal return, calculated using the Daniel, Grinblatt, Titman, and Wermers (1997) benchmark.[19] The main explanatory variables are treated dummies interacted with time indicators. For consistency with Table 1.4, we call the interaction term *Disaster Zone* indicating that the firms is held by mutual funds headquartered in the hurricane area. The regression also includes firm and time fixed effects, and controls for firm size (log-market cap) and firm turnover in the past 6 months).

This specification is more stringent than that used in the traditional fire sales literature (e.g. Coval and Stafford, 2007; Edmans, Goldstein, and Jiang, 2012), as this paper uses a generalized difference-in-differences with firm and time fixed effects, as opposed to an event study without any control group and/or fixed effects. Therefore, our methodology compares firms exposed to liquidity shocks to those that are not exposed, after taking into account time-invariant firm characteristics and time-varying unobservables, which might be confounding factors in an event study. Moreover, we impose a dynamic structure to the model which allows for a direct assessment of the selection bias concerns of Berger (2019), as we test the difference between the treated and control groups in the months preceding the occurrence of a hurricane. Such a dynamics also allows for a non-constant response of the outcome variable to the treatment, which enables us to identify the drop and subsequent reversal further discussed below.

The results are shown in Table 1.5, where columns (1)-(4) display estimations when the DGTW-adjusted returns are used as dependent variable, and columns (5)-(8) use the value-weighted CRSP return as benchmark for comparison. The table shows a striking result that confirms the hypothesis. Between event-months 0 and 5 the stock price drops at a rate of 1.1% per period, while significantly reverting between months 6 and 15 and remaining equal to the long-run value after month 15. This result is better shown in Figure 1.4, that plots the cumulative coefficients of a regression similar to that in Table 1.5 but with single-period dummies. The temporary drop and subsequent reversal within less than 12 months clearly emerges from the graph. The cumulative drop of around 7% after 5 months is in line with that shown in previous research, although a comparison is difficult to make provided the different empirical approach. Most importantly, there is no significant difference between the treated and the control group by month 15; that is, 10 months after the price has reached its plateau. This reversal pattern is faster than that estimated

[19] Wardlaw (2020) suggests that using the market model to adjust returns might mechanically overestimate the reversal pattern, provided that a portfolio composed only of stocks held by mutual funds in Thomson/CDA Spectrum outperforms the CRSP average by 2-3% per year. Further, he shows that the characteristics adjustment can alleviate this concern.

by the traditional fire sales literature of about 24 months, and is suggestive of the presence of a true nonfundamental shock. This is consistent with the findings of Bogousslavsky, Collin-Dufresne, and Sağlam (2020) that, in settings where the nonfundamental shock is better isolated, the reversal pattern is actually faster. However, in our setting, the price dislocation is not absorbed instantaneously because, as we show in the next section, it affects real decisions. In other words, what starts as a nonfundamental drop in prices, has fundamental implications that reduce the speed of price reversal.

A natural question to ask is whether the 7% drop is in line with the estimated outflows. To answer this question, we translate this figure into dollar amounts. Provided that the median firm in the sample has a market capitalization of 300 millions, and that 878 firms are "treated" on average, the 7% drop means that $300 \times 878 \times 0.07 = \$18.4$ billions of market capitalization are destroyed five months after the hurricane. Moreover, Table A.5 suggests that, a quarter after the hurricane, affected mutual funds lose \$2.5 billions in outflows, that is \$4.16 in 5 months. Therefore, we estimate a multiplier of $18.4/4.16 = 4.42$, which is in line with the estimation of (Gabaix and Koijen, 2020), where flows usually affect prices with a multiplier of 5.

Following the analysis in Wardlaw (2020), we test further the reversal mechanism by constructing a long-short portfolio that buys stocks exposed to the hurricane shock between the previous 5 and 15 months, and sells the control stocks. Results are shown in Table A.11. The calendar time portfolio analysis is run using the three Fama and French (1993) factors plus momentum (Carhart, 1997), and estimated either with Newey-West standard errors with 6 lags (column 1), or using a weighted least squares with the number of stocks in the portfolio as weight. Both the specifications show a positive and significant alpha of 1.1% per month, formally confirming that the reversal pattern is present for the group of treated stocks.

Next, we test whether the price dislocation is more prominent for some stocks. In particular, the hypothesis is that small and illiquid stocks are likely to be more affected by the hurricane. We test this hypothesis using a triple differences specification in which the difference-in-differences dummies are interacted with indicators for the stock characteristics. Figure 1.5 summarizes the results. In Panel A, we look at different price response for big (size above 75% of the sample distribution), medium (size between 25% and 75% of the sample distribution), and small stocks (size below 25% of the sample distribution), while, in Panel B, we distinguish between illiquid (Amihud illiquidity above 50th percentile of the sample distribution) and liquid stocks (below median Amihiud illiquidity measure). In both the specifications, the control variables and the fixed-effects are also interacted with the characteristic dummy. As conjectured, we find that the price drop is more severe and takes more time to revert for small and, to some extent, for illiquid stocks.

We discussed above the importance of remaining agnostic about the way mutual funds liquidate their portfolios. However, to rule out that other factors might explain our results, we have to show that treated stocks are more likely to be sold by mutual funds in the sample than the control stocks. To test this hypothesis we use percentage trading of fund in a stock during a quarter (Lou, 2012) and regress this variable (or an indicator for sale trades, that takes values equal to one when the percentage trading is negative) onto a dummy, *Disaster Zone*, equal to one if the stock falls in the treated group and the fund is headquartered in the disaster area and the observation is recorded in quarters 0 and 1 after the hurricane. The control group is made of all the other firms that fall in the control group for the analysis of Table 1.5. Therefore, to avoid confounding effects, we exclude fund-stock-quarter information for stocks that are affected by the hurricane.[20] The control variables are the fund and stock characteristics used throughout the paper. Table A.12 shows results for this test. Columns (1)-(4) report estimates when the dependent is the continuous variable of trades, while results for the linear probability model of sell trades are shown in columns (5)-(8). Remarkably, the first raw of the table suggests that, in each specification, treated firms are more likely to be sold than firms in the control group right after the hurricane hits.

In contrast to the existing literature, the results in this section stem from a definition of the treated group of stocks that does not depend on the amount of shares held by affected funds. Therefore, as a robustness test, we rerun the analysis of Table 1.5 using the two measures described in equations (1.2) and (1.3), and define treated those stocks that display negative values of those variables.[21] Table A.13 shows that results are largely unchanged when using these more standard approaches for defining the treatment group.

One concern is that these results are driven by non-random characteristics of the stocks in the treated group. For example, Berger (2019) shows that fire sales stocks differ in many dimensions from those in the control group. However, the use of a difference-in-differences design alleviates this concern, as what is required is not a perfect match between the treated a control group, but that the parallel trend assumption is satisfied. Moreover, both Figure 1.4 and Table 1.5 show no sign of pre-trend in abnormal returns: the difference-in-differences coefficient is zero in the six months preceding the hurricane.

To further validate this point, we test whether other important firm characteristics, such as size, financial constraints, tangibility, profitability, cash flow, payout ratio, change with the hurricane. We show the results for this test in Figure 1.6. For each of the characteristic, the graph displays the coefficients of a generalized difference-in-differences regression for years [-5, 5] around the hurricane. Notably, our methodology does not seem to induce any selection bias in the analysis. In particular, for many firm characteristics, the treated and control groups do not seem to differ significantly both before and after the occurrence of hurricanes. The only

[20] In an unreported analysis, we show that results are qualitatively unchanged when these observations are not excluded from the sample.

[21] The context of this paper, which builds on 15 events only, makes it impossible to use the decile approach usually adopted by researchers when constructing an instrument based on fire sales. In the firm-year sample the dummy treated is equal to 1 for roughly 3-3.5% of the sample only.

variable to change with the hurricane seems to be the payout ratio. We argue that this further validates the point that the price drop is nonfundamental. The pattern displayed in panel (f), which shows an abnormal increase in payout for treated firms after the hurricane, is consistent with the firm buying back shares that are likely to remain relatively cheaper for a limited period of time.

Finally, Berger (2019) suggests that stocks usually selected as control group for fire sales stocks are likely to have low or zero institutional ownership. Since institutional investment in firms might be driven by unobservable firm characteristics, this generates an additional selection bias in the analysis. To address this matter, we test whether the results of Table 1.5 hold in a subsample where firms with low or zero institutional ownership are excluded. Results are reported in Table A.14, where column (1)-(3) focus on the sample of stocks with institutional ownership greater than zero, while columns (4)-(6) discard stocks with a less than 1% institutional ownership. First, we document that our strict selection of the treatment and control groups already discards most of the firms without institutional ownership. Looking at the difference in number of observations between Table 1.5 and Table A.14, we stress that only 4% (10%) of the sample have zero (less than 1%) institutional ownership.[22] Most importantly, Table A.14 shows that the main results of this section are unchanged also in these subsamples.

Next, to further investigate whether observable and unobservable firm characteristics might affect the results, we use a matching routine in which we assign, to each treated firm, two control firms using nearest neighborhood matching with replacement, and then rerun the difference-in-differences analysis. Results are shown in Figure 1.7, where panel (a) matches on institutional ownership, panel (b) on log-size, and panel (c) on both. The nonfundamental price drop following a hurricane event is confirmed also by this matching analysis. In each of the three panels, cumulative DGTW abnormal returns start dropping after the hurricane and reach the floor at -6% by month 5, and completely reverts while approaching month 20.

1.7 The Real Effects of Hurricane-Induced Flows

1.7.1 Main Result

This section studies whether the managers of the treated firms change their investment policy after the occurrence of the hurricane. Absent any response of firms to nonfundamental decrease in stock prices, the natural disaster should not affect investments of firms unrelated to the hurricane. This is the null hypothesis tested in this section.

As is common in the literature on the real effects of finance, we test the null by using an instrumental variable (IV) approach, where Tobin's Q is instrumented with a proxy for nonfundamental liquidity shock and then used to explain firm policies.

[22] Berger (2019) reports that roughly 30% of her sample has 0 institutional ownership.

Addressing the question of whether nonfudamental price dislocations affect real economic activities requires the use of an instrumental variable approach, because using a standard OLS model where firm policy is regressed directly onto Tobin's Q returns a biased coefficient, as the main explanatory variable includes both the fundamental and nonfundamental component of stock prices. On the contrary, the methodology used in this paper, first, isolates the nonfundamental component of stock prices and, then, tests whether this has an effect on next year real decisions.

We propose, as novel instrument, the *Hurricane Hypothetical Sale (HHS)* measure of equation (1.1). Notably, the results in previous sections support the exclusion restriction for using this instrument; that is, the origin of the liquidity shock are retail investors that have to confront unexpected negative economic conditions following a natural disaster. Importantly, they withdraw money from local mutual funds only for their liquidity needs, and not because they possess information about future funds' returns. In our instrumental variable model, the dependent variable is investments, proxied, as is standard in the literature, by the ratio between capital expenditure and lagged property plant and equipment (PPE), and the main explanatory variable is Tobin's Q; that is, the ratio between market and book value of assets. The regression also controls for firm log-size and cash-flows, as in Dessaint, Foucault, Frésard, and Matray (2019).

The choice of investment as outcome variable is driven by two reasons. First, the overarching question of whether nonfundamental variation in stock prices affects the real economy is strongly related to whether and how stock prices allow for an efficient allocation of resources. With these regards, how much to invest in (possibly NPV-positive) projects is the most important matter faced by firm managers (Dow and Gorton, 1997). Second, the literature on the real effects of finance has widely studied investments (e.g. Chen, Goldstein, and Jiang, 2007; Foucault and Frésard, 2014). Hence, using this firm policy allows for a fair comparison with the existing research.

The results of this IV estimation on a sample at the firm-year level are shown in Table 1.6. We report several specifications with different fixed-effects combinations, going from the less stringent firm and time to the most-stringent industry-location-time, which compares firms in the same industries, headquartered in the same CBSA and differing only for whether they are part of the portfolio of affected funds. In all specifications, we report the IV estimation, the first stage where Tobin's Q is regressed onto the instrument, and the reduced form model where investment is directly regressed onto the instrument.

The IV estimations are all strongly statistically significant as predicted by the hypothesis of this paper. The point estimate on the reduced form model for the most-stringent specification with industry-location-time fixed effect (Panel B, column 6) is -0.022 with a t-statistics of -2.71. This means that a one-standard-deviation increase in the instrument (i.e., a nonfundamental decrease in the stock price) is associated with 1.14-percentage-points decrease in firms' investment, which corresponds to 4.4% of the average investment level in the industry. The table also reports results for the analysis of the relevance of the instrument (i.e., the Kleibergen and Paap (2006) (KP) F-tests) together with the p-value with respect to the Stock and Yogo (2005) critical values. This is a generalization of the standard 1st stage F-statistics, adapted to non-independently and non-identically distributed errors.[23] All the specifications show that *HHS* is a strong instrument, with p-values for the F-test always close to zero.

For robustness, we run the same analysis using as instrument the indicator based on *HHS*, used in the analysis of stock returns to identify the treated and control sample. The results are reported in Panel A of Table A.15, which shows that estimates are largely unchanged when the dummy variable is used in place of the continuous instrument. Moreover, Panels B and C report the IV analysis when the instruments defined in equations (1.2) and (1.3) are used in place of *HHS*. Even though the point estimates seem comparable, the diagnostics for weak instruments does not seem to confirm that these alternative instruments are strong. This evidence additionally speaks in favor of the new instrument introduced in this paper.

Finally, as a further robustness test, we rerun the IV analysis using a more homogeneous sample to mitigate the selection bias concerns raised in Berger (2019). Similarly to what we have done for fund flows, we design an IV regression where only firms that are treated at least once are included in the sample. We report results in Table 1.7 and show that the hypothesis of a negative effect of *Hurricane Hypothetical Sales* onto investments is generally confirmed even in this more tight sample.

1.7.2 Channel

We conclude the analysis on the real effects of hurricane-induced flows by providing evidence on the channel that drives firm managers to decrease investments. We proceed as follows.

We test whether the drop in investment is transient or permanent. On the one hand, if the shock is nonfundamental, then investment should bounce back, once the shock is completely absorb. This hypothesis relies on the fact that investment opportunities are long-lived and that the firm preserve resources to postpone investment to the next year. However, competition might decrease the duration of an investment opportunity, or cash might be used in activities other than investment. To shed lights on these two alternative hypothesis, we estimate the reduced-form specification (column 3 of Table 1.6) with lagged values of HHS (namely, HHS_{t-2} and HHS_{t-3}). To have a cleaner picture of the dynamics of a firm's investment in

[23] We report p-values for the null hypotheses that the bias in the point estimate on the endogenous variable is greater than 10 percent or 30 percent of the OLS bias, or that the actual size of the *t*-test that the point estimate on the endogenous variable equal zero at the 5 percent significance level is greater than 10 or 25 percent. As discussed in Bazzi and Clemens (2013), to which we refer for a lengthy analysis of the issue, the Stock and Yogo (2005) critical values do not directly apply to the KP F-statistics, however it is common in the econometric literature to make inference using this tool.

response to nonfundamental variations caused by hurricanes, we also add the contemporaneous and 1-year-ahead values of HHS to this more general specification. Figure 1.8 plots the point estimates (and their confidence intervals) for the coefficients on HHS at dates t-3 to t+1.

In line with the results in the previous section, investment in year t responds negatively to a hurricane hitting, in year $t-1$, an area where mutual funds that hold the firm are headquartered. However, the point estimates increase, while remaining statistically indifferent from zero, 2 and 3 years after the hurricane (coefficients on HHS_{t-2} and HHS_{t-3}). Therefore, we do not find any evidence of over-investment as a way of making up for the hurricane-related under-investment. This is consistent with a permanent decrease in investment.

In section 1.6, we have incidentally shown that the drop in investment cannot be explained by a change in financing constraints, as there not seem to be any change in the Kaplan and Zingales (1997) index induced by the hurricane. Therefore, we test the *strategic hypothesis*. This hypothesis suggests that firm's managers correctly understand that the shock to their stock price is temporary and they want to signal that to the market. We conjecture that a manager of a firm exposed to the hurricane only through the mutual funds holding its shares will temporarily increase payouts to her investors and, in particular, they will buyback more shares to correct the mispricing (Peyer and Vermaelen, 2009) and inform the market that the future prospects of the firm are not impaired (Massa, Rehman, and Vermaelen, 2007). This hypothesis relies would also reconcile the finding that capital expenditure does not bounce back, as resources are moved to other activities.

To test the *strategic hypothesis*, we run a difference-in-differences regression around the hurricane, where the treated and control groups are the same of section 1.6 and the dependent variable is a firm-year proxy for payout or share repurchases. We report results for this analysis in Figure 1.9. Consistent with the hypothesis, Panel (a) shows a 4% increase in the ratio of total payout (dividends plus repurchases) to operating income in the year after the hurricane. Moreover, Panel (b) suggests that treated firms increase the purchase of preferred and common stocks once the hurricane hits.

Strikingly, we observe no reversal in years 2 and 3 after the hurricane. Nevertheless, we measure the dependent variable as the ratio of Compustat item 115 to item 13 and, as suggested by Stephens and Weisbach (1998), the numerator is an aggregate of all security repurchases and retirements during the year, which overstates the actual repurchases of common stocks.[24] Therefore, in Panel (c) we replace the

[24]For example, it might include money spent to redeem preferred stock or the conversion of preferred into common shares.

numerator with the actual repurchase of common stocks[25] and we find that the effect is concentrated only in the year after the hurricane, while it completely vanishes in the subsequent years.

Overall, the results in this section suggest that firms that are unrelated to the hurricane but held by mutual funds headquartered in the disaster zone strategically respond to the nonfundamental drop in their stock prices by relocating finances toward share repurchases as their managers anticipate that the price drop will be temporary.

1.8 Conclusions

This paper addresses a long-standing question in finance: do nonfundamental price dislocations affect real decisions? Using mutual funds' outflows to isolate the nonfundamental component of stock prices, a large literature provided evidence on the existence of a relation between temporary price drops and real economic activities. However, recent contributions challenged these findings on the grounds of methodological issues.

This paper provides new evidence on the link between nonfundamental price swings and corporate investment, by shifting the perspective from an ex-post to an ex-ante identification of mutual funds' outflows. We focus on the liquidity needs of mutual fund investors created by large and damaging hurricanes, to identify the actual origin of capital withdrawals. We show that, following a hurricane, firms held by affected funds, but unrelated - both geographically and economically - to the disaster, experience a sizable 7% drop in their stock price. The price decline is temporary, and reverts back to the fundamental value within 10 months. Moreover, we document that firms respond to the price dislocation, as investment, in the year after the hurricane, decreases by 4% with respect to the sample mean. Our results are robust to several tests addressing the recent critiques. In particular, we show that our findings are not driven by past stock returns, suggesting that we are truly isolating a nonfundamental origin of fund flows.

We show that the nonfundamental drop in stock prices is due to the pressure from mutual funds that hold firms otherwise unrelated to the hurricane. Following the calamity, investors living in the disaster zone withdraw money from their mutual funds located in the hurricane area. We report that these funds experience an abnormal outflow of $16.15 million in the first quarter, which continues in the following quarters without any sign of reversal. To enhance these claims, we show that mutual funds, in general, display a local base and that the hurricane-induced outflows are stronger for funds more likely to have a local clientele.

[25] Regulatory changes to Rule 10b-18 of the Securities Exchange Act of 1934 in 2003 required U.S. firms, starting from 2004, to make quarterly disclosures of actual share repurchases and average prices paid. These figures are available in the quarterly Compustat file; hence, we aggregate quarterly values to get the total value of shares repurchased at the end of the year.

Overall, our results contribute to the debate as they imply that stock price inefficiencies, when correctly isolated, actually affect real decisions. Moreover, we provide evidence that the firm's managers understand that the shock is nonfundamental and relocate resources from investment to share repurchases to correct the mispricing and signal good future prospects. The literature proposed a series of other channels (e.g., learning, catering, and financial constraints), to which future extensions should devote further attention.

Finally, future work should research the type of home bias outlined in this paper. In particular, more granular data on household investments in mutual funds might provide additional grounds for studying how geography matters in their allocation of savings. Testing to what extent investors buy local mutual funds would be another interesting venue for future research, as we show that such a form of home bias has implications for the real economy.

1.9 Tables

Table 1.1: Summary statistics. This table reports summary statistics for the main samples used in the analysis. Panel A, displays statistics for the CRSP-Thomson Reuters merged mutual fund database. The sample is at the wficn-quarter level and spans the period between 1980q1 and 2017q4. Panel B describes the firm-year variables in the CRSP-COMPUSTAT merged database from 1980 to 2017. The samples are selected as described in section 1.2. For each variable the table reports the number of observations mean, standard deviation, 25^{th} percentile, median, and 75^{th} percentile. Note that in Panel B, return variables and turnover are expressed as yearly averages of monthly data. Finally, Panel C shows the statistics for the instrumental variables used in the analysis, conditional on the firm being held by at least one affected fund. All variables are constructed as described in Table A.1.

Panel A: Sample of mutual funds						
	N	Mean	SD	p25	Median	p75
Flows	133,934	-0.007	0.124	-0.048	-0.017	0.023
Return	133,934	0.026	0.092	-0.016	0.033	0.080
TNA	133,934	1,346.642	3,299.205	77.200	277.518	1,019.100
TER	133,934	0.011	0.005	0.009	0.011	0.014
Turnover	133,934	0.781	0.775	0.290	0.560	1.000
Volatility	133,934	0.045	0.021	0.030	0.040	0.055
No. funds: 3,822						
No. CBSAs: 126						

Panel B: Sample of firms						
	N	Mean	SD	p25	Median	p75
Q	105,519	2.193	3.141	1.084	1.474	2.309
Capex/PPE	105,519	0.387	0.516	0.128	0.231	0.430
CF/A	105,519	0.004	0.252	0.001	0.073	0.121
Size	105,519	4.772	2.175	3.155	4.647	6.286
Turnover	104,244	0.001	0.002	0.000	0.001	0.001
Return	105,510	0.012	0.056	-0.016	0.011	0.036
DGTW-Adj. Return	100,004	0.002	0.064	-0.023	-0.001	0.022
Market-Adj. Return	105,510	0.002	0.054	-0.025	0.000	0.024
Financial constraints	100,588	-7.173	25.625	-4.918	-0.787	1.038
Profitability	105,449	0.044	0.249	0.018	0.106	0.168
Tangibility	105,490	0.275	0.223	0.097	0.213	0.392
Payout ratio	98,638	0.163	0.350	0.000	0.006	0.177
Hurricane Hypothetical Sale (HHS)	105,519	69.983	330.037	0.000	0.000	0.000
Hurricane Induced Flow (HIF)	105,519	-0.142	0.862	0.000	0.000	0.000
Hurricane Induced Surprise Flow (HISF)	105,519	-10.249	56.987	0.000	0.000	0.000
No. firms: 11,493						
No. CBSAs: 437						

Table 1.2: Do location fixed-effects matter? This table reports R-squared and number of observations from fixed effects panel regressions, where the dependent variable is the quarterly fund flow and the explanatory variables are the lagged flows and fund return, up to the 8th lag (*Specification with all lags*), or past quarter return, log-TNA, fund turnover, past year return volatility, and total expense ratio. For each specification, columns (1) and (2) report results when the dependent variable is the continuous variable for flows, while a dummy equal to 1 if the flow is in the top or bottom decile of its distrubion is used in column (3)-(4), and (5)-(6), respectively. Regressions are run with fund and location-time (interacted) fixed-effects (row 1), fund, location and time fixed-effects (row 2), and fund and time fixed-effects (row 3). Core-based Statistical Area (CBSA) of the fund headquarter is used as location fixed-effects.

Dependent variable	Flows					
	Continuous variable		Dummy inflows		Dummy outflows	
	Adj. R2	Obs	Adj. R2	Obs	Adj. R2	Obs
Specification with all lags						
Fund + Location × Time	21.52	65,767	18.10	65,767	16.44	65,767
Fund + Location + Time	20.08	65,767	17.35	65,767	15.39	65,767
Fund + Time	19.90	65,767	17.29	65,767	15.31	65,767
Parsimonious specification						
Fund + Location × Time	14.53	131,557	13.35	131,557	13.83	131,557
Fund + Location + Time	12.55	131,557	12.17	131,557	12.78	131,557
Fund + Time	12.49	131,557	12.07	131,557	12.72	131,557

Table 1.3: Preference for proximity: Fund flows and the local economy. This table reports results for the following regression:

$$y_{i,t} = \alpha_i + \gamma_t + \zeta_l + \beta \times \text{Local economy}_{l,t-1} + X^{'}\theta + \varepsilon_{i,t}\,,$$

where the outcome variable, $y_{i,t}$, is either the percentage flow to fund i in quarter t or a dummy equal to 1 if the flow represents an outflow. The main explanatory variable, Local economy$_{i,t-1}$ if either the unemployment rate or the house price index computed in quarter $t-1$ for MSA l, where fund i is headquartered. The vector of control variables, X, includes the total expense ratio, the fund turnover, previous quarter fund return, and the fund return volatility in the previous 12 months. α_i, γ_t, ζ_l represent fund, time, and location fixed-effects, respectively. Standard errors are clustered at the location-time level and t-statistics reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Proxy for local economy	HPI				Unemployement rate			
Dependent variable	Flow (%)		Outflow indicator		Flow (%)		Outflow indicator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local economy (q-1)	0.494**	0.351*	-0.020***	-0.016***	-0.508***	-0.513***	0.023***	0.023***
	(2.541)	(1.817)	(-3.353)	(-2.643)	(-3.180)	(-3.259)	(4.590)	(4.672)
Total Expense Ratio		1.380***		-0.024***		1.410***		-0.021***
		(8.315)		(-4.857)		(7.732)		(-3.808)
Turnover		-0.050		0.005**		-0.072		0.007***
		(-0.581)		(2.240)		(-0.781)		(2.923)
TNA		2.632***		-0.085***		2.826***		-0.088***
		(17.411)		(-21.163)		(17.260)		(-20.754)
Return		3.886***		-0.141***		3.933***		-0.141***
		(29.750)		(-33.314)		(29.129)		(-32.245)
Return Volatility		-0.567***		0.020***		-0.592***		0.023***
		(-4.376)		(4.610)		(-4.302)		(5.072)
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	122,367	122,367	122,367	122,367	116,847	116,847	116,847	116,847
Adjusted R-squared	0.112	0.132	0.190	0.212	0.113	0.133	0.195	0.217

Table 1.4: Hurricanes and fund flows. This table reports difference-in-differences estimates of the effects of hurricanes on funds located in the area affected by the adverse natural event. The dependent variable is the fund flow, expressed in percentage points. Fund headquarters are identified in terms of Core-based Statistical Areas (CBSAs). *Disaster Zone (q+i-j, q+i)* is a dummy variable equal to one for funds headquartered in any of the CBSAs hit by the hurricane in quarter (q) and the observation is recorded in quarters (q+i-j, q+i) around a hurricane event. The control variables are the Total Expense Ratio (TER), the log-TNA, the volatility of fund returns in the previous 12 months, and the fund return in quarter *q-1*. The control group is made of all funds with headquarters outside the hurricane area. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Flows (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster zone (q-4, q-1)	-0.323	-0.368	-0.333	-0.353	-0.303	-0.434
	(-0.849)	(-0.935)	(-0.881)	(-0.907)	(-0.490)	(-0.687)
Disaster zone (q)	-1.446*	-1.387*	-1.438*	-1.346*	-1.566**	-2.014***
	(-1.921)	(-1.733)	(-1.902)	(-1.680)	(-2.129)	(-2.734)
Disaster zone (q+1, q+4)	-0.606	-0.656	-0.611	-0.643	-0.556	-0.633
	(-1.462)	(-1.577)	(-1.459)	(-1.528)	(-1.021)	(-1.116)
Disaster zone (q+5, q+8)	-1.104***	-1.160***	-1.088***	-1.142***	-1.077**	-1.207***
	(-3.132)	(-3.421)	(-3.112)	(-3.400)	(-2.393)	(-2.845)
Disaster zone (q+9, q+12)	-1.587***	-1.670***	-1.542***	-1.636***	-1.230*	-1.661***
	(-3.665)	(-3.801)	(-3.548)	(-3.700)	(-1.939)	(-2.762)
Total Expense Ratio		1.433***		1.428***		1.631***
		(5.428)		(5.434)		(6.026)
Turnover		0.093		0.090		0.108
		(0.595)		(0.571)		(0.685)
TNA		3.090***		3.095***		3.326***
		(10.882)		(10.859)		(11.379)
Return		3.829***		3.829***		1.111***
		(11.787)		(11.777)		(6.616)
Return volatility		-0.260		-0.257		0.024
		(-0.858)		(-0.846)		(0.099)
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	No	No
Location FE	No	No	Yes	Yes	No	No
State-Time FE	No	No	No	No	Yes	Yes
Observations	133,934	133,934	133,933	133,933	133,908	133,908
Adjusted R-squared	0.107	0.126	0.107	0.126	0.110	0.122

Table 1.5: Hurricane and stock returns. This table reports difference-in-differences estimates of the effects of hurricanes on the returns of firms headquartered outside the hurricane area. The sample is made of firms unrelated to the hurricane as described in section 1.3. The main treated group is made of firms held by funds hit by the natural event. *Disaster Zone (t+i-j, t+i)* is an indicator equal to one if the firm falls in the treated group and the observation is recorded in months $[t+i-j, t+i]$ around the hurricane. The dependent variable is the DGTW-adjusted monthly return, and the control variables are the firm log-size and past 6-month volume turnover. Standard errors are clustered at the firm and month level. T-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	DGTW Adj. Monthly Returns			Market Adj. Monthly Returns		
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster Zone (t-6, t-1)	0.003	0.003	0.003	-0.000	-0.000	-0.001
	(0.992)	(1.002)	(0.999)	(-0.058)	(-0.087)	(-0.114)
Disaster Zone (t, t+5)	-0.011**	-0.011**	-0.011**	-0.020**	-0.020**	-0.021**
	(-2.430)	(-2.429)	(-2.428)	(-2.483)	(-2.496)	(-2.532)
Disaster Zone (t+6, t+15)	0.005**	0.005**	0.005**	0.008*	0.008*	0.008*
	(2.085)	(2.092)	(2.085)	(1.799)	(1.800)	(1.735)
Disaster Zone (t+16, t+24)		0.000	0.000		-0.002	-0.002
		(0.090)	(0.086)		(-0.389)	(-0.416)
Disaster Zone (t+25, t+48)			-0.000			-0.003*
			(-0.126)			(-1.902)
Size	-0.049***	-0.049***	-0.049***	-0.049***	-0.049***	-0.048***
	(-28.168)	(-28.195)	(-28.161)	(-15.638)	(-15.681)	(-15.651)
Turnover	-0.002***	-0.002***	-0.002***	-0.003***	-0.003***	-0.003***
	(-3.164)	(-3.164)	(-3.163)	(-3.150)	(-3.147)	(-3.138)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,265,043	1,265,043	1,265,043	1,459,100	1,459,100	1,459,100
Adjusted R-squared	0.014	0.014	0.014	0.047	0.047	0.047

Table 1.6: Hurricanes: real effects. This table reports results for the two-stage least square regression where the dependent variable of interest is a proxy for investments, defined as the ratio of capital expenditure scaled by lagged fixed assets (property, plant, and equipment) in year t for firm i, and the main explanatory variable is Tobin's Q defined as the ratio of market value of assets to book value of assets in year t for firm i. The instrument for Tobin's Q is the same proxy, HHS, for the exposure of the firms to mutual funds headquartered in the hurricane area use in Table 1.5. Panel A reports two specifications, the first (columns 1-3) uses firm and time fixed-effects, while the second includes firm and state-year FE. Panel B shows two additional specifications using either industry and state-year FE (columns 1-3), or industry-state-year FE (columns 4-6). For each specification, we report the second-stage IV regression, the first stage and the reduced form (RF) where the Capex/PPE is regressed onto the instrument directly. The vector of control variables includes the firm's cash flow, and the firm log-size. All variables are standardized. Standard errors are double clustered at the firm and year level. T-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

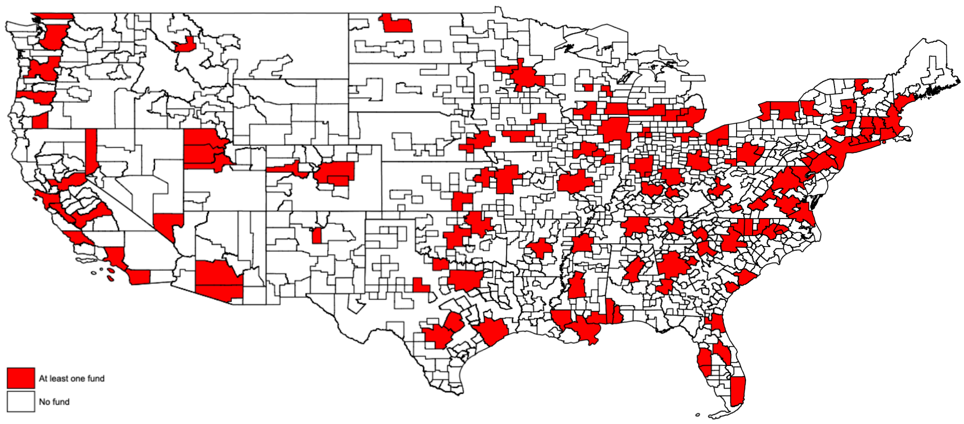
Panel A	Firm, Time, Location FE					
Dependent variable	Capex/PPE					
	IV	1st Stage	RF	IV	1st Stage	RF
	(1)	(2)	(3)	(4)	(5)	(6)
Q	0.621***			0.644***		
	(4.235)			(3.548)		
HHS		-0.036***	-0.022***		-0.032***	-0.021***
		(-4.869)	(-5.784)		(-4.982)	(-5.094)
Cash Flow	0.224***	-0.197***	0.101***	0.228***	-0.198***	0.101***
	(7.020)	(-7.169)	(9.955)	(5.956)	(-7.330)	(9.890)
Size	-0.129	0.648***	0.273***	-0.153	0.643***	0.261***
	(-1.402)	(10.248)	(7.989)	(-1.354)	(10.784)	(8.272)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No	No	No
Location-Time FE	No	No	No	Yes	Yes	Yes
Observations	105,519	105,519	105,519	105,408	105,408	105,408
Kleibergen-Paap F stat	23.710			24.820		
H_0: t-test size $>$ 10% (p-value)	0.007			0.005		
H_0: t-test size $>$ 25% (p-value)	0.000			0.000		
H_0: relative OLS bias $>$ 10% (p-value)	0.002			0.001		
H_0: relative OLS bias $>$ 30% (p-value)	0.000			0.000		
Adjusted R-squared		0.386	0.237		0.389	0.240

Panel B	Industry, Time, Location FE					
Dependent variable	Capex/PPE					
	IV	1st Stage	RF	IV	1st Stage	RF
	(1)	(2)	(3)	(4)	(5)	(6)
Q	0.541**			0.660**		
	(2.181)			(2.375)		
HHS		-0.036***	-0.019**		-0.033***	-0.022**
		(-4.188)	(-2.656)		(-5.042)	(-2.710)
Cash Flow	0.147*	-0.310***	-0.021	0.191**	-0.317***	-0.018
	(1.831)	(-8.990)	(-1.676)	(2.088)	(-8.507)	(-1.489)
Size	-0.104*	0.240***	0.026*	-0.143**	0.250***	0.022
	(-1.788)	(9.312)	(1.845)	(-2.182)	(9.185)	(1.504)
Industry FE	Yes	Yes	Yes	No	No	No
Location-Time FE	Yes	Yes	Yes	No	No	No
Industry-Location-Time FE	No	No	No	Yes	Yes	Yes
Observations	105,411	105,411	105,411	94,368	94,368	94,368
Kleibergen-Paap F stat	17.540			25.420		
H_0: t-test size>10% (p-value)	0.037			0.004		
H_0: t-test size>25% (p-value)	0.000			0.000		
H_0: relative OLS bias>10% (p-value)	0.012			0.001		
H_0: relative OLS bias>30% (p-value)	0.001			0.000		
Adjusted R-squared		0.148	0.068		0.108	0.055

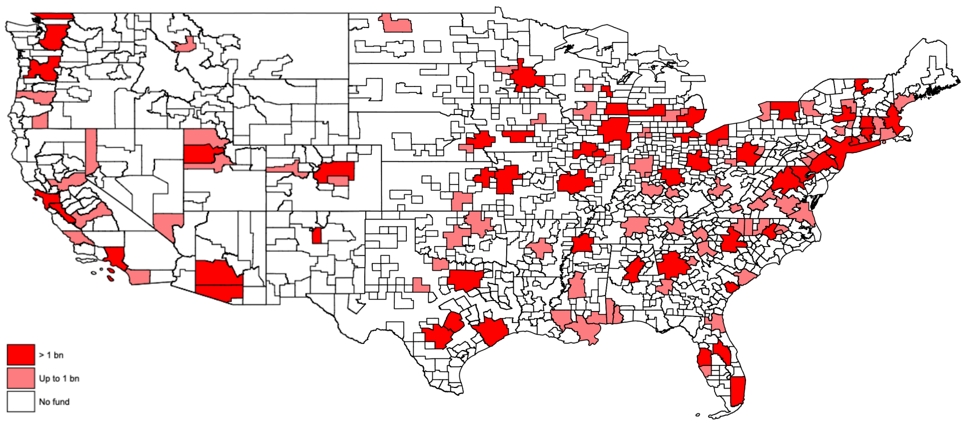
Table 1.7: Hurricanes and real effects: homogeneous sample. This table reports estimates of the real effects of hurricanes on firms unrelated to the natural events. The model is the IV regression used in Table 1.6, but the sample comprises stocks that have non-zero value of the instrument *HHS* at least once during the sample. Hence, the same treated stocks serve as control group when they are not affected by a hurricane. Standard errors are clustered at the firm and time level. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Capex/PPE											
	IV	1st stage	reduced	IV	1st stage	reduced	IV	1st stage	reduced	IV	1st stage	reduced
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Q	0.726***			0.747***			0.696**			0.827**		
	(4.128)			(3.694)			(2.590)			(2.205)		
Hurricane		-0.058***	-0.042***		-0.053***	-0.039***		-0.054**	-0.038***		-0.052**	-0.043***
		(-3.452)	(-5.525)		(-3.551)	(-6.029)		(-2.324)	(-4.298)		(-2.114)	(-4.807)
Cash Flow	0.199***	-0.104***	0.123***	0.200***	-0.104***	0.122***	0.122**	-0.163***	0.009	0.149**	-0.167***	0.011
	(7.350)	(-3.155)	(7.776)	(6.826)	(-3.252)	(7.774)	(2.712)	(-4.204)	(0.456)	(2.533)	(-3.868)	(0.534)
Size	-0.227**	0.546***	0.170***	-0.238**	0.544***	0.168***	-0.159***	0.206***	-0.016	-0.191**	0.223***	-0.006
	(-2.597)	(6.165)	(3.322)	(-2.341)	(6.585)	(3.462)	(-3.208)	(5.155)	(-0.744)	(-2.700)	(4.757)	(-0.264)
Firm FE	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	No	No	No
Time FE	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes	No	No	No
Location-Time FE	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Observations	48,778	48,778	48,778	48,631	48,631	48,631	48,631	48,631	48,631	39,130	39,130	39,130
Adjusted R-squared		0.370	0.245		0.379	0.251		0.147	0.105		0.089	0.087

1.10 Figures

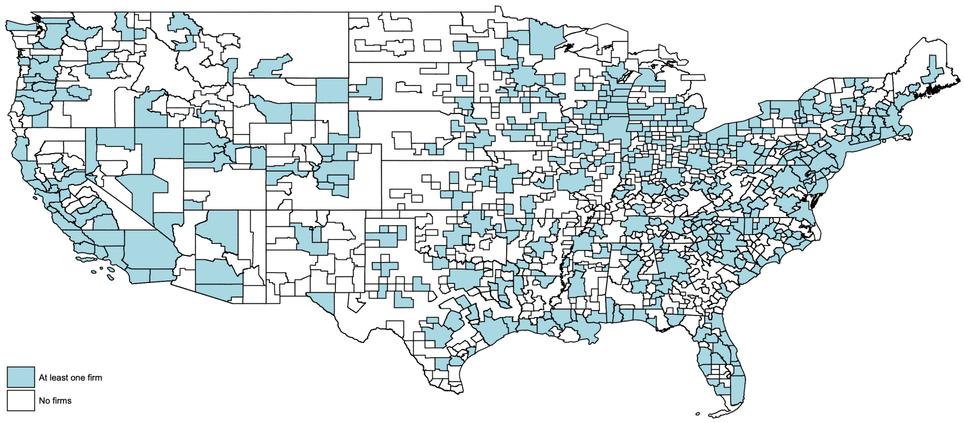


(a) *CBSAs with at least one fund*

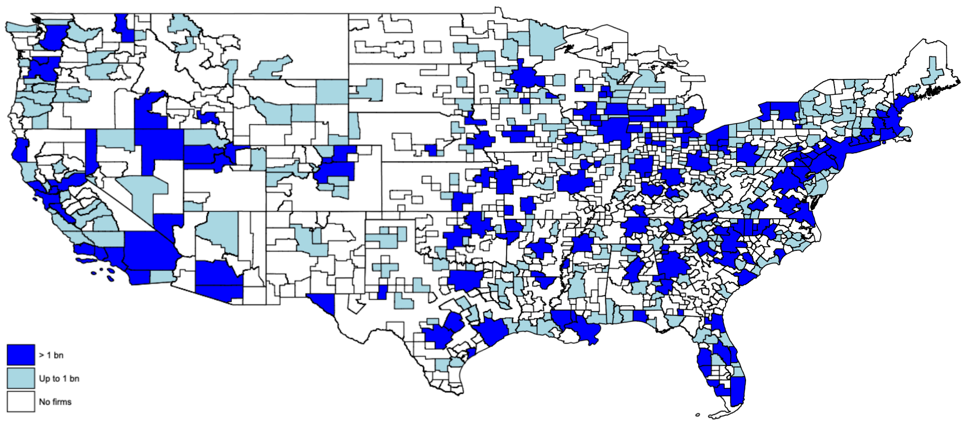


(b) *Total TNAs by CBSA*

Figure 1.1: Geographic distribution of funds. This figure reports the geographic distribution of the sample of mutual funds. In Panel A, CBSAs with at least one fund are shown in red, while in Panel B darker color indicates higher total TNA in 2020 billion of dollars in a CBSA.

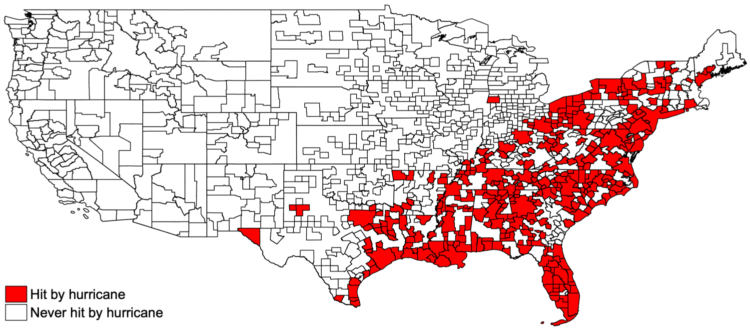


(a) *CBSAs with at least one firm*

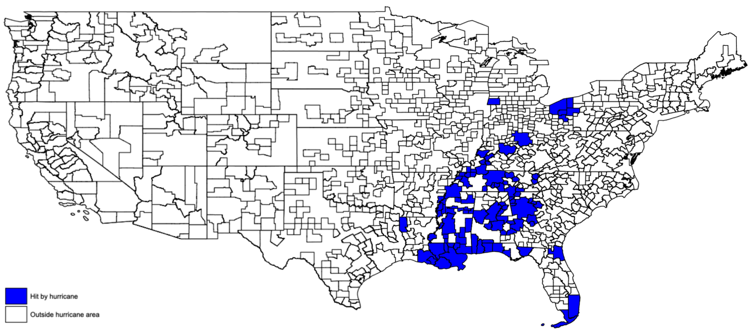


(b) *Total market cap by CBSA*

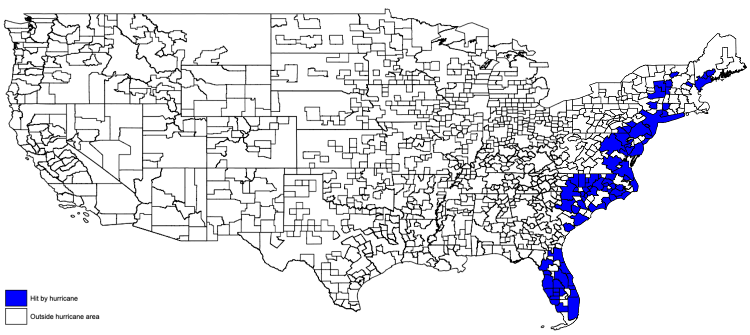
Figure 1.2: Geographic distribution of firms. This figure reports the geographic distribution of the sample of firms. In Panel A, CBSAs with at least one firms are shown in light blue, while in Panel B darker color indicates higher total total market cap in 2020 billion of dollars in a CBSA.



(a) *MSAs hit at least once*



(b) *Hurricane Katrina (2005)*



(c) *Hurricane Floyd (1999)*

Figure 1.3: Localization of hurricanes. This figure displays the localization of the hurricanes in our sample. Panel A reports in red the MSAs hit at least once by one of the 15 hurricanes considered in the analysis. Panel B (C) shows in blue the counties hit by hurricane Katrina (Floyd).

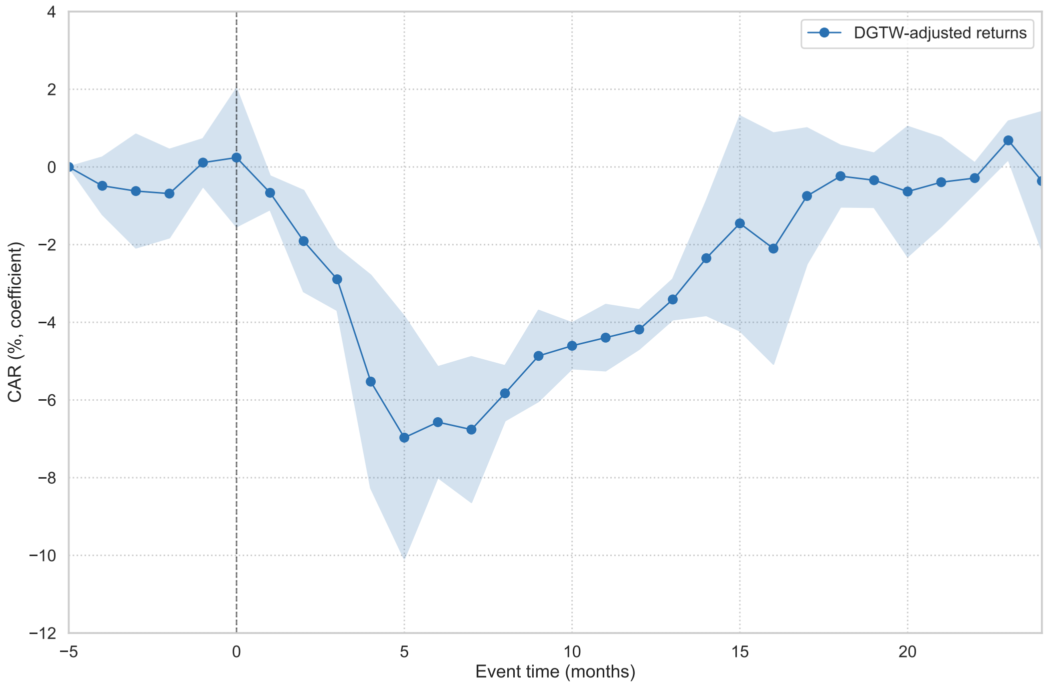
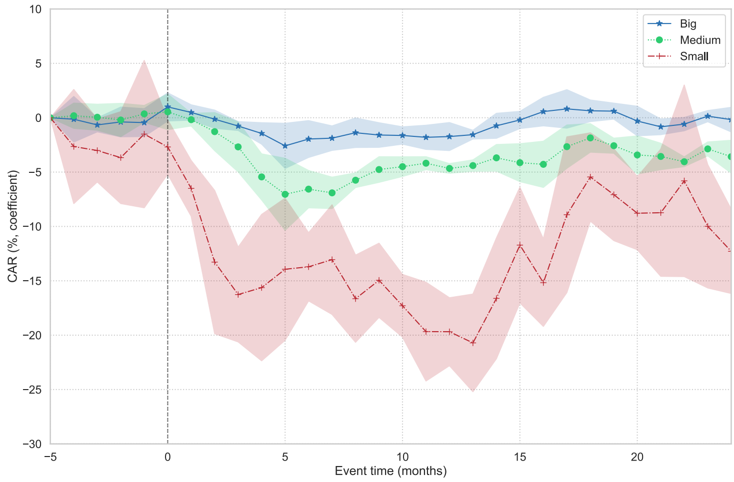
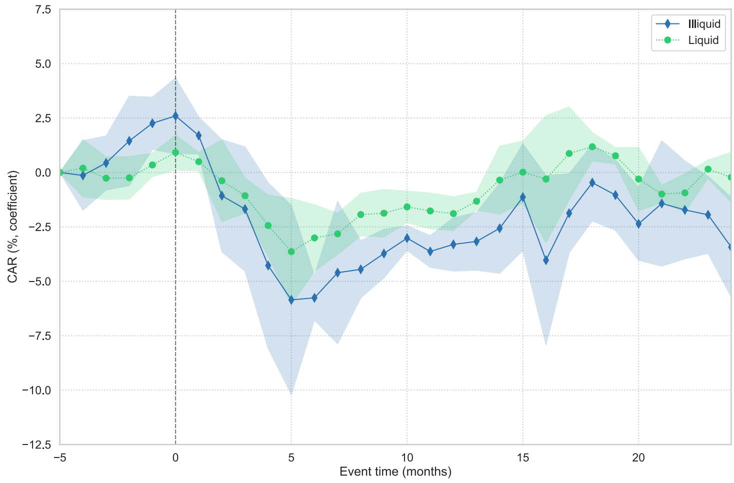


Figure 1.4: Hurricanes and stock returns This figure displays cumulative coefficients of a difference-in-differences model where the dependent variable is the monthly DGTW-adjusted stock return and the treated group is made of stocks unrelated to the hurricane but held by mutual funds headquartered in the disaster zone. The control group is made of firms unrelated both geographically and economically to the hurricane are. The specification estimates coefficients for months [-4, +24] around the hurricane event using stock and month fixed effects and controlling for log-firm size, and previous 6-month stock turnover. The shaded area represents the 95% confidence interval for standard errors clustered at the stock and month level.

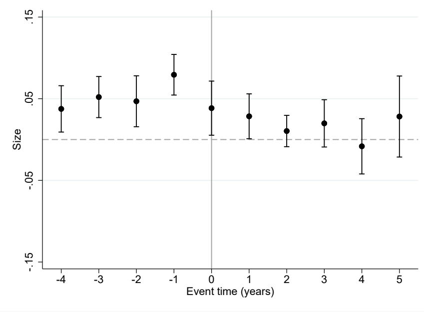


(a) *Cross-section: Size*

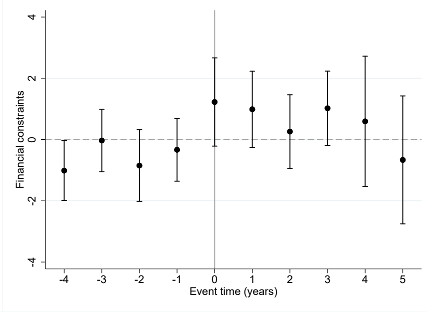


(b) *Cross-section: Liquidity*

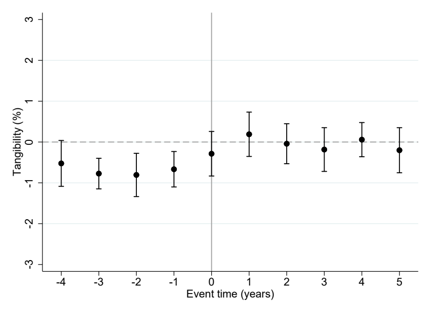
Figure 1.5: Hurricanes and stock returns: cross-sectional analysis. This figure displays cumulative coefficients of a difference-in-differences model where the dependent variable is the monthly DGTW-adjusted stock return and the treated group is made of stocks unrelated to the hurricane but held by mutual funds headquartered in the disaster zone. The control group is made of firms unrelated both geographically and economically to the hurricane are. The specification estimates triple interaction coefficients for months [-4, +24] around the hurricane event using stock and month fixed effects and controlling for log-firm size, and previous 6-month stock turnover. In Panel A, the triple interaction is between the treatment dummy, the time dummy, and a dummy for big stocks (top 75% of size distribution, blue line), medium stocks (between 25% and 75% of size distribution, green line), or small stocks (bottom 25% of size distribution, red line). Similarly, in Panel B, we look at the cross-section in terms of illiquid (top 50% of Amihud illiquidity measure, blue line) and liquid stocks (bottom 50% of Amihud illiquidity measure, green line). The shaded area represents the 95% confidence interval for standard errors clustered at the stock and month level.



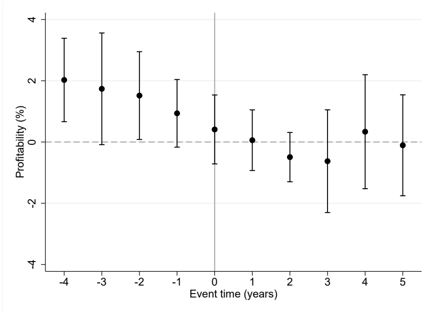
(a)



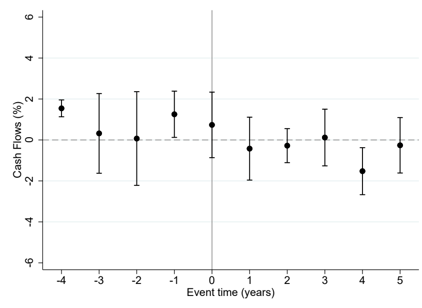
(b)



(c)

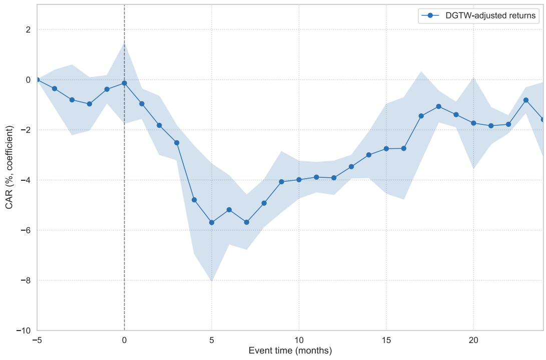


(d)

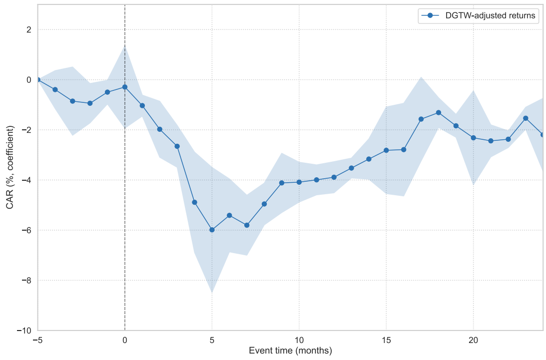


(e)

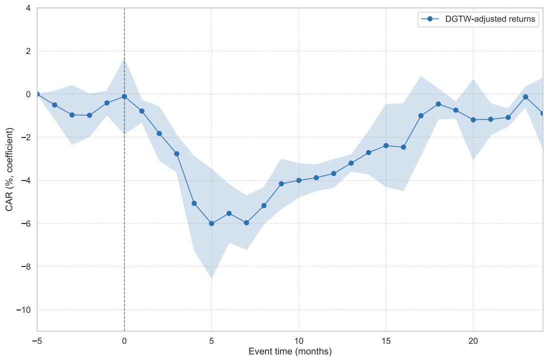
Figure 1.6: Firm characteristics. This figure displays difference in differences coefficients for several firm characteristics around a hurricane event. Panel (a) shows results where the dependent variable is the log-size, while estimates for financial constraints ((Kaplan and Zingales, 1997) index, computed as in Lamont, Polk, and Saá-Requejo (2001)) are shown in Panel (b). Panels (c) and (d) show tangibility and profitability, respectively. Tangibility is computed as the ratio of property, plan and equipments (*ppent*) to total asset, while profitability is operating income before depreciation divided by total assets. Finally, panels (e) reports results where the dependent variable is cash-flow. The figure reports point estimates and confidence intervals for standard errors clustered at the firm and year level for each year in the window [-5, 5] around a natural disaster.



(a) *Match on institutional ownership*



(b) *Match on size*



(c) *Match on institutional ownership and size*

Figure 1.7: Hurricanes and stock returns: matched sample. This figure displays cumulative coefficients of a difference-in-differences model where the dependent variable is the monthly DGTW-adjusted stock return and the treated group is made of stocks unrelated to the hurricane but held by mutual funds headquartered in the disaster zone. The control group is made of firms unrelated both geographically and economically to the hurricane are. For each treated stock, the control group is made of the two closest stocks in terms of institutional ownership (Panel a), size (Panel b), or both institutional ownership and size (Panel c). The selection procedure uses nearest neighborhood matching with replacement. The specification estimates coefficients for months [-4, +24] around the hurricane event using stock and month fixed effects and controlling for log-firm size, and previous 6-month stock turnover. The shaded area represents the 95% confidence interval for standard errors clustered at the stock and month level.

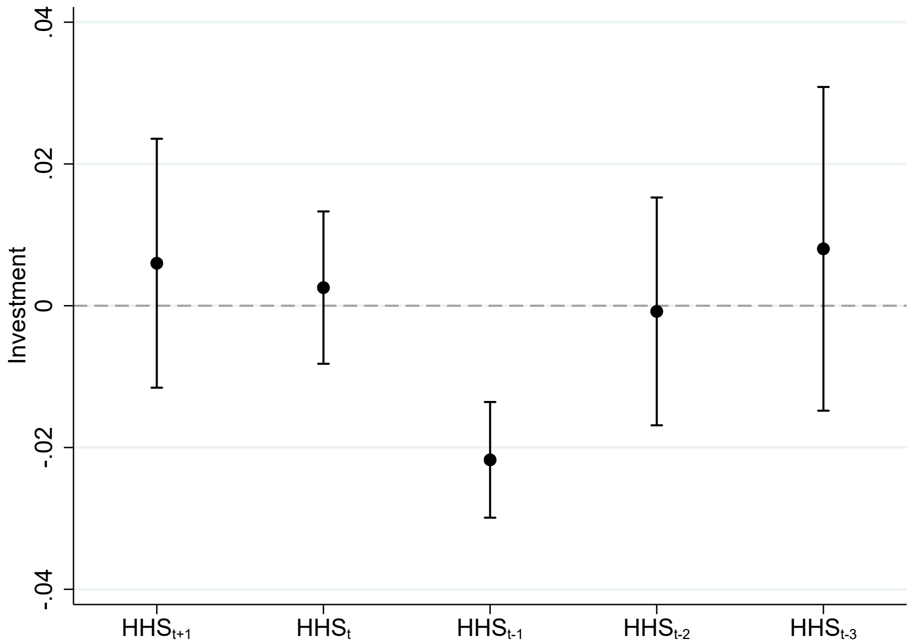
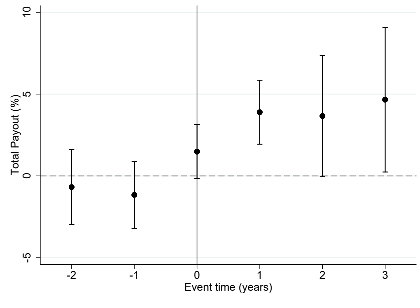
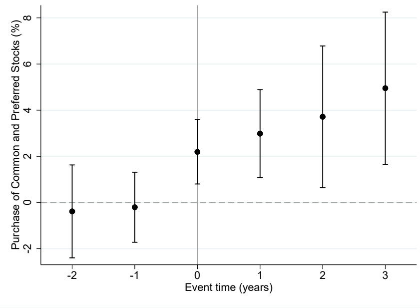


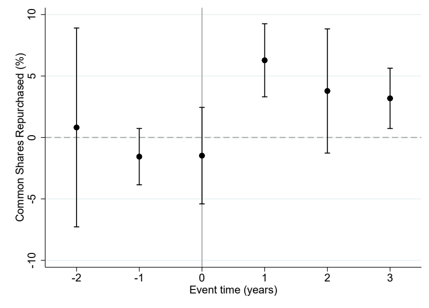
Figure 1.8: The dynamics of hurricanes' real effects This figure displays the regression coefficients of the reduced-form specification in column 3 of Table 1.6 with leads and lags of the instrument, HHS. Each point estimate is accompanied by its 95% confidence interval.



(a)



(b)



(c)

Figure 1.9: The strategic hypotesis. This figure displays difference in differences coefficients around a hurricane event for payout and share repurchases proxies. Panel (a) shows results where the dependent variable is the total payout ratio, measured as the sum of total dividends (*dvp+dvc*) and purchase of common and preferred stock (*prstkc*) divided by operating income before depreciation (*oibdp*). The purchases of preferred and common stocks is shown in Panel (b), where the dependent variable is the ratio between *prstkc* and *oibdp*. We show the effect on actual repurchases of common shares in Panel (c). Again, we divide share repurchases by operating income before depreciation. In this last specification, due to data availability, the sample starts in 2004. The figure reports point estimates and 95% confidence intervals for standard errors clustered at the firm and year level.

Chapter 2

Strategic Trading as a Response to Short Sellers

2.1 Introduction

There is a widespread view in finance that short selling is beneficial for the market because it lets negative information seep into prices, improving price efficiency.[1] However, regulators have not consistently embraced this view, as they fear the distortive effect of short sales for security prices. Over the years, they have imposed several restrictions on short selling, including the uptick rule in the U.S. and outright short-sale bans around the world, such as during the Great Financial Crisis. Given the recent resurgence of limitations to short sales in connection to the Covid-19 crisis, it seems urgent to revisit the question of the effect of short selling on financial markets (Enriques and Pagano, 2020). This paper brings new evidence to this debate by studying a novel channel through which short selling can affect price discovery. Inspired by Foster and Viswanathan (1996)'s theory of differentially informed investors, we conjecture that the anticipation of a price decline induced by short sales leads positively-informed investors to strategically react by trading more cautiously. Specifically, investors receiving a positive signal can rationally decide to delay and conceal their trades, waiting for short sellers to induce a price decline so that they will be able to acquire the asset at a lower price later on. This strategic behavior can reduce price informativeness, potentially offsetting the beneficial impact of short sales.[2] Importantly, this conjecture does not rely on short sellers receiving wrong signals on average, which would run against the ample evidence that short activity predicts negative returns.[3] Rather, the claim is that the presence of short sellers

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[1] Examples of theory papers positing a beneficial impact of short selling are Miller (1977), Diamond and Verrecchia (1987), Hong and Stein (2003), Ofek and Richardson (2003), and Hong, Scheinkman, and Xiong. Consistent empirical evidence is found, e.g., in Saffi and Sigurdsson (2011), Beber and Pagano (2013), Boehmer and Wu (2013), Prado, Melissa, and Sturgess (2016), and Blocher and Ringgenberg (2019). Goldstein and Guembel (2008) is one of the few theoretical contributions proposing a channel for a negative impact of short selling on price efficiency based on strategic price manipulation.

[2] We note that short selling activity is observable by other market participants. U.S. exchanges publish short interest data twice a month, while commercial data providers, such as Markit and Bloomberg, disseminate daily statistics. Finally, brokers may provide information about short sales to their clients.

[3] Several studies show that short selling predicts negative stock returns, including: Boehmer, Jones, and Zhang (2008), Boehmer, Huszar, and Jordan (2010), Engelberg, Reed, and Ringgenberg (2012),

reduces price informativeness when other investors in the market receive more informative signals. Indeed, short sellers are not always the most informed traders and often their informational advantage lies primarily in their ability to interpret public news as opposed to anticipating news events (e.g. Engelberg, Reed, and Ringgenberg, 2012). Moreover, short selling activity may result from uninformed hedging needs or market-making (Blocher and Ringgenberg, 2018). However, irrespective of its informational content, short selling causes a negative price impact on average (Ringgenberg, 2014). Thus, it makes sense for investors with positive information to wait out the price decline before engaging in buy trades. The testable conjecture assumes that the presence of short-selling activity is known to other market participants. Indeed, several channels contribute to spreading information about the extent of short-selling activity. For example, data providers publish daily statistics on the shorting market, while the exchanges publish this information biweekly. Markit Securities Finance, which we use in part of our analysis, is one such example. Importantly, brokers that intermediate share loans can spread the word to their other clients to establish a reputation as valuable sources of information, consistent with the findings in Di Maggio, Franzoni, Kermani, and Sommavilla (2019). Our laboratory for testing this conjecture is the period before earnings announcements, a time in which disagreement on the fundamentals of the asset is more likely. We find that the amount of information that prices reflect is significantly lower when short selling is more aggressive. This effect is present conditioning on positive earnings surprises, that is when buyers are likely to be better informed than short sellers. We show that the mere price pressure of short sellers cannot account for the magnitude of this effect. Instead, corroborating strategic behavior as the underlying channel, institutional investors slow down significantly their buy trades and break their orders across multiple brokers when short-selling activity is more pronounced. This evidence is consistent with the interpretation that positively-informed investors try to hide their trades while they wait for short sellers to push prices down. An alternative interpretation of the observed slowdown in buy trades is that investors update their priors downwards after observing short sellers in action (Diamond and Verrecchia, 1987; Senchack and Starks, 1993). We rule out this alternative because we observe that trading volume on the buy-side increases for the stocks more exposed to short selling. An increase in buying volume is not consistent with investors shifting towards a negative view, but rather it suggests that investors with a positive signal take advantage of the short sellers' presence to acquire a larger position. The evidence of increased volume makes yet another explanation of our findings less likely. In particular, the slow-down and break-up of trades could be the reaction of uninformed buyers to a worsening of liquidity conditions. This development, in turn, occurs when short sellers become more aggressive, shifting from liquidity providers to liquidity demanders. However, the observation of a larger total volume

Cohen, Diether, and Malloy (2007), Diether, Lee, and Werner (2009b), Rapach, Ringgenberg, and Zhou (2016), and Boehmer, Jones, and Zhang (2020).

suggests that investors have a positive view on the stock and take the opportunity of a decreasing price to buy more of it. To infer the trading behavior of informed investors, we use data on institutional transactions from Abel Noser Solutions (aka ANcerno). Prior work establishes the institutions in ANcerno as informed investors (Chemmanur, He, and Hu, 2009; Chemmanur, Hu, and Huang, 2010; Puckett and Yan, 2011; Anand, Irvine, Puckett, and Venkataraman, 2012, 2013a; Jame, 2017). We further select the more active traders in ANcerno to identify the institutions that are more likely to be informed. The key challenge in comparing the behavior of investors across stocks with different amounts of short selling is that short activity is arguably endogenous and itself may be the strategic response to other investors' trades. While the strategic behavior of short sellers is an interesting question, which other literature has tackled (Arif, Ben-Rephael, and Lee, 2015), the original focus of this paper is the effect of short sellers on other market participants. The identification of this effect requires exogenous variation in short-selling activity. The Reg SHO experiment provides an ideal setting to test our hypotheses. Specifically, in the two years between 2005 and 2007, the SEC suspended short-sale price restrictions – i.e. the uptick rule – for a randomly selected group of stocks (the Pilot stocks). This policy was explicitly designed to provide an exogenous release of short sale constraints for one-third of the Russell 3000 universe and assess the effect of short selling on different market outcomes.[4] In their pioneering study of Reg SHO, Diether, Lee, and Werner (2009a) find a significant increase in short-sale volume and a decrease in liquidity. They conclude that the suspension of the uptick rule allowed short sellers to trade more aggressively. We confirm and extend their findings using data from Markit, which is an unbiased sample of the universe of stocks and has the advantage of higher-frequency observations on short-selling activity. We find an increase in short interest for Pilot stocks following the removal of the uptick rule. To reconcile this result with Diether, Lee, and Werner (2009a), who find an insignificant increase in short interest for Pilot stocks, we show that standardizing short interest by average trading volume or by shares outstanding, as we do, is a key choice for achieving statistical significance, likely because it reduces measurement error. We also find that more aggressive liquidity demand by short shellers exerts stronger downward pressure on prices. Specifically, using intraday data from TAQ, we show that Pilot stocks experienced a significant increase in price impact during the experiment. This evidence allows us to address an important point made in a recent paper by Heath, Ringgenberg, Samadi, and Werner (2019). The authors argue that

[4] Rule 10a-1 from the Securities Exchange Act of 1934 established that short sales should be subject to price tests. This rule was implmented as NYSE's Uptick rule and NASDAQ's bid price test. In particular, NYSE Rule 440B provided that a short sale was only allowed on a plus tick. It was allowed also on a zero tick only if the most recent price change preceding the trade was a plus tick (called a zero-plus tick). According to NASDAQ Rule 3350, short sales were not allowed at or below the (inside) bid when the current inside bid was at or below the previous inside bid. The SEC lifted these restrictions in 2007, only to reintroduce them in 2010 in the form of 'Modified uptick rule' (Rule 201), which is triggered if the price falls at least 10% in one day. At that point short selling is allowed only if the price is above the current best bid. This aims to preserve investor confidence and promote market stability during periods of stress and volatility.

the possibility of reusing the Reg SHO experiment to test previously unexplored effects relies on establishing a valid first stage. In our context, increased short-sale volume and negative price pressure are the first-stage effects on which positively-informed market participants can condition their trades.[5] Our preferred measure of price informativeness is the ratio of the abnormal return in the two weeks before the announcement to the total announcement return (Weller, 2018). This variable fits closely the testable hypothesis as it captures the amount of private information regarding the event that seeps into prices before the announcement. We find that this quantity is 18% lower for Pilot stocks before positive announcements. Instead, we find no significant relationship between short-selling activity and information impounding before negative news. To corroborate the interpretation of this result, we also consider variables that more generally measure the efficiency of prices in the pre-announcement period, namely the variance ratio (e.g. Lo and MacKinlay, 1988; O'Hara and Ye, 2011) and the cross-correlation between the contemporaneous stock return and the lagged market return (Bris, Goetzmann, and Zhu, 2007; Saffi and Sigurdsson, 2011), and we find consistent evidence.[6] Price pressure from unconstrained short selling provides a natural explanation for the finding of lower returns on Pilot stocks before the announcements. However, we rule out this explanation. Using TAQ data on short sales during the Reg-SHO Program period, we compute the price impact of short sales. Our computations reveal that the price impact of the additional short sales can account for a minor fraction of the observed reduction in information impounding (i.e., up to about 20%). We conclude that price pressure from short sellers, by itself, does not explain our finding. Next, we investigate the conjecture of strategic behavior by positively-informed investors and ask whether they delay and conceal their trades. Indeed, we find that before earnings announcements the trading speed of buy trades – defined as the fraction of total volume that they execute early on in the period under consideration – decreases for Pilot stocks during the Reg SHO experiment. In particular, we estimate a 7% decrease in trading speed for buy orders of Pilot stocks before a release of positive news. This result is confirmed with an alternative definition of trading speed based on the size of the trades, which addresses the potential concern that the denominator of the first measure is affected by trades occurring after the event, as in the case of positive-feedback trading. Additionally, we find that positively-informed investors split their buy trades across multiple brokers and they shift their orders from central to peripheral brokers, consistent with an attempt to avoid the information leakage taking place through brokers, especially the more central ones (Di Maggio, Franzoni, Kermani, and Sommavilla, 2019; Barbon, Di Maggio, Franzoni, and Landier,

[5] Moreover, in several specifications, the statistical significance of our estimates passes the adjusted critical values for multiple hypotheses testing, as computed by Heath, Ringgenberg, Samadi, and Werner (2019) using the Romano and Wolf (2005) sequential ordering approach.

[6] Boehmer and Wu (2013) find that when short sellers are more active, prices are more accurate, and, in particular, the post-earnings-announcement drift is reduced for negative earnings news. We differ in that our focus is on the effect of short selling on positive information impounding before earnings releases and uncover a novel strategic interaction between market participants with opposing views.

2019). Finally, investors send their buy orders to more unfamiliar brokers – i.e., those with whom they traded less in the past – when short selling is more likely, which is again consistent with the need of spreading trades more thinly and avoid information leakage (Brogaard, Li, Ma, and Riordan, 2020). Arguably, negatively-informed investors should also engage in strategic behavior in response to short selling. The prediction is that they rush to sell their holdings anticipating the price decline induced by short sellers, a behavior that Massa, Quian, Xu, and Zhang (2015) find for company insiders. Indeed, we find evidence consistent with strategic behavior on the sell side in the sample that only contains hedge funds, i.e., institutions that are more shielded from urgent liquidity needs and typically more informed.We do not find evidence of this effect in our broader sample, but this is probably due to the fact that institutional sales, more than buys, are also motivated by liquidity needs. In robustness analysis, after the conclusion of the Reg SHO experiment, when short-selling constraints were lifted on all stocks, we no longer find a significant difference in the main outcome variables between Pilot and Control stocks. This result corroborates the interpretation that our main findings are driven by the exogenous variation in short selling induced by Reg SHO and they are consistent with the findings in Boehmer, Jones, and Zhang (2020). The paper relates to several strands of the literature. First, we contribute to the studies on the impact of short selling on price efficiency. Previous studies report a positive effect of short selling on price efficiency (Saffi and Sigurdsson, 2011; Beber and Pagano, 2013; Boehmer and Wu, 2013; Prado, Melissa, and Sturgess, 2016). Instead, our contribution is to show that short-selling activity can deter positive information impounding when some investors receive positive information signals and these signals are more informed than the ones received by short sellers. In other words, the beneficial impact of short selling on information efficiency is weakened, and possibly reversed, when other investors are better informed than short sellers. Our results are consistent with theories positing that informed traders hide their information. Chakravarty (2001), Anand and Chakravarty (2007), and Alexander, Cici, and Gibson (2007) document that this is done by reducing the size of the trade. In contrast, some authors argue that informed traders who face borrowing costs resort to large and revealing trade sizes (Froot, Scharfstein, and Stein, 1992; Blau and Smith, 2014). This paper establishes the trade breakup across brokers as an alternative way to carry out stealth trading, a channel that is modeled in Kondor and Pinter (2018). Massa, Quian, Xu, and Zhang (2015) focus on the strategic interaction between short sellers and company insiders that wish to trade on negative information. Kacperczyk and Pagnotta (2019) also focus on company insiders and show that they act strategically when facing legal risk. Different from these papers, we study the reaction of institutional investors to short sellers. The paper proceeds as follows. Section 2.2 describes our data and identification strategy. Section 2.3 studies price informativeness ahead of earnings announcements. Section 2.4 focuses on trading speed and trade breakup across brokers. Section 2.5 considers an alternative hypothesis and provides further evidence and robustness analysis. Section 2.6 concludes.

2.2 Data and Empirical Strategy

The sample for the empirical analysis results from the combination of different data sets. First, we draw institutional trades from Abel Noser Solutions, formerly known as Ancerno Ltd. (we retain the name of "ANcerno" , commonly used in the literature; see Hu, Jo, Wang, and Xie (2018a), for a detailed description of this data set). ANcerno provides consulting services for transaction-cost analysis to institutional investors and made these data available for academic research. While some institutions voluntarily report to ANcerno, the fact that clients submit this information to obtain objective evaluations of their trading costs, and not to advertise their performance, suggests that self-reporting should not bias the data. Indeed, the characteristics of stocks traded and held by ANcerno institutions and the return performance of the trades are comparable to those in 13F mandatory fillings (Puckett and Yan, 2011; Anand, Irvine, Puckett, and Venkataraman, 2012). ANcerno provides information about each single trade execution. Hence, we know: the transaction date; the execution price; the number of shares that are traded; the side (buy or sell); the broker that intermediates the trade; the management company originating the trade (through the variable *managercode*). We are therefore able to identify buyer- and seller-initiated trades and to keep track of how many brokers are used to trade a certain stock. To make sure that we do consider only fund managers with the ability to react to the presence of short-sellers, we select the subset of ANcerno managers that display the highest level of active trading. In particular, for each manager, we construct a portfolio by cumulating the trading activity over an expanding window of at least two years. Then, we regress the fraction of monthly trading in a given stock on the weight of the stock in the manager's portfolio at the beginning of the month. Intuitively, the more active the manager the less relevant are the existing portfolio weights in explaining the trading activity. Finally, we restrict our analysis to the subset of managers for which the R-squared in these regressions is in the lower half of the distribution across managers. This selection criterion provides an additional benefit. Asset managers whose trades display the largest discrepancies from their existing portfolio weights tend to follow short-term trading strategies (i.e., they are more likely to be momentum than value managers). These investors, therefore, will pay more attention to signals coming from the behavior of short interest than the rest of the universe. We draw information on stock level short-selling activity from Markit Securities Finance, formerly known as Data Explorers. This firm provides benchmarking information to the securities lending industry and short-side intelligence to the investment management community. Markit Securities Finance collects data from leading industry practitioners, including prime brokers, custodians, asset managers, and hedge funds, and is one of the biggest providers of securities lending data. These data are available to us at the monthly frequency from June 2002, at the weekly frequency since August 2004, and at daily frequency since July 2006. The short-selling variable provided in the data is the total balance of shares on loan. Given the different frequencies during the sample period, we average these variables within a month, when the available frequency is higher. We also use data from the Center for Research in Security Prices (CRSP - number shares outstanding, prices, return, trading volume), from Compustat (when computing the Daniel, Grinblatt, Titman, and Wermers (1997), i.e. DGTW, adjusted returns), and IBES (for earnings announcements). For the computation of daily measures of liquidity at the stock level, we use intra-day data from TAQ. In our main analysis, we consider the sample of earnings announcements ranging from May 2002 to July 2007. The beginning of the sample is dictated by the availability of Markit data. We set the end date of the sample to the end of the Reg SHO Pilot Program. The pre-event period ranges between May 2002 and the start of the Reg SHO experiment (May 2005), but we find similar results allowing for a pre-event period with the same two-year length as the event period. Our sample includes only ordinary stocks (Share Code 10 or 11 in CRSP) that belong to the Russel 3000 Index and that are present in ANcerno, CRSP, and any other database used to compute the variables of interest. We select the earnings announcement for which a valid earnings surprise (SUE) measure is available. We follow Della Vigna and Pollet (2009) and define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price five trading days before the announcement. The measure for analysts' expectations is the median of the latest individual analysts' forecasts issued within the 90 days before the announcement. The final sample includes 32,638 earnings announcements, of which 23,240 display a positive earnings surprise, and 9,398 correspond to negative news releases.[7] We consider announcements for which at least one active manager is executing a trade. As far as the analysis of price informativeness around the announcement is concerned, we use the same sample selection as in Weller (2018); that is, we require the *absolute* value of the CAR in the days [-10, 1] around the announcement to exceed the standard deviation of daily returns computed in the preceding twenty trading days and scale it by the squared root of the interval length. We use this condition to have announcements with sufficiently large information release and, therefore, to reduce noise in the variable of interest. Panel A of Table 2.1 defines our main variables. Panel B reports descriptive statistics for the key variables used in our analysis distinguishing between positive and negative earnings announcements.

2.2.1 Identification Strategy

We start by laying out our identification strategy. We seek exogenous variation in short-selling activity – i.e., changes in short selling that are not direct responses to either the actions of the other investors or the characteristics of the stocks being traded.

[7] We identify a significant larger number of positive earnings surprises than negative surprises. This evidence is consistent with prior findings in the literature. See e.g. DellaVigna and Pollet (2009).

To this purpose, we take advantage of the Reg SHO experiment conducted by the SEC between 2005 and 2007. In this period, the SEC removed the short-sale price tests – i.e., the uptick rule for the NYSE and the bid rule for NASDAQ (see footnote 1) – for a randomly selected group of about 1000 stocks (the "Pilot stocks") from the Russell 3000. This change amounted to a relaxation of short-sale constraints. The SEC ranked the firms listed on NYSE, NASDAQ, and AMEX by average daily traded volume and selected for the Pilot Program every third firm. In this way, the stratified random sample was representative of the cross-section of stocks and daily volume. The temporary suspension expired on August 6, 2007. However, in our tests, we let the treatment period end on July 5, 2007, because this was the compliance date for a new rule that eliminated any short-selling constraints on all stocks. Given the random selection of Pilot stocks, we follow prior literature in running a difference-in-differences analysis in which Pilot firms represent the treatment group, while the Russell 3000 stocks outside the program are the control group. In defining the treatment and control groups different choices are made in the literature. We opt to proceed with the least restrictive criteria while still retaining only ordinary shares. We start with the list of stocks that were in the Russell 3000 index both when the SEC defined the Pilot Program (April 2004) and when the Pilot Program started (May 2005). Next, we keep only ordinary shares (CRSP share codes 10 and 11) and stocks trading in the NYSE, AMEX, and NASDAQ stock exchanges. Finally, we attach the list of Pilot firms from the SEC website and get a final sample of 2,729 stocks, among which 823 are treated and 1,906 serve as a control group.[8] Appendix Table B.2, Panel A, shows how our selection criteria differ from some of the existing studies (i.e. Diether, Lee, and Werner, 2009a; Heath et al., 2019) and from a sample that does not restrict to ordinary stocks (large sample). Panel B of the same table details the numbers of stocks in the treatment and control groups in each month of the sample and compares them to those obtained with the other potential selection criteria. In their seminal study of the Reg SHO experiment, Diether, Lee, and Werner (2009a) find a significant increase in the ratio of short sales to total volume for Pilot stocks (Table III in their paper), concluding that " *. . . it appears that suspension of NYSE's Uptick rule and Nasdaq's bid price test makes it somewhat easier to execute short sales.*" (p. 40). Moreover, they show that measures of market liquidity (i.e. the quoted spread, the effective spread, and the realized spread) deteriorated after the suspension of price tests for treated stocks (Table VI, Panel B, in their paper). The authors interpret the evidence as suggesting that the uptick rule forced short sellers to become involuntary liquidity providers, i.e. they could only sell at the ask or

[8] Given the combination of different datasets in our analysis, the number of treated and control firms varies in different parts of the analysis. For example, when we require the availability of Markit data, the sample consists of 2,420 stocks (741 Pilot and 1,679 controls), while in the analysis with ANcerno data and earnings announcements, we have a total of 2,552 firms, with 782 Pilot stocks and 1,770 controls. For robustness, we run our main analyses using the sample at the intersection of Markit and ANcerno data, that is, 2,273 stocks of which 704 are Pilot and 1,569 control. As shown in Appendix Table B.1, results remain significant and are qualitatively unchanged.

higher prices. Removing this restriction allowed short sellers also to demand liquidity. The consequence is that the net supply of liquidity decreased. These findings are key to our identification strategy. More aggressive trading by short sellers makes a response by other investors more likely. This channel, therefore, suggests that Reg SHO could trigger the type of behavior that is the focus of this paper. To corroborate the validity of this identification channel, we expand on Diether, Lee, and Werner (2009a) analysis of short selling activity and liquidity, studying new variables, and drawing on different data sources. In particular, to measure short-selling activity, we draw shares on loan from Markit. Confirming the conclusions in Saffi and Sigurdsson (2011), our analysis in Appendix Table B.3 shows that the stocks that are present in Markit qualify as a representative sample of the entire universe and of the sample of stocks that report short interest data to Compustat. The advantage of using Markit comes from the higher frequency of observation for a large part of the sample, which provides more accurate, i.e. less stale, snapshots of the evolution of short selling activity. In Table 2.2, Panel A, we find that shares on loan as a fraction of total shares outstanding increase for Pilot stocks during the Reg SHO period. The analysis is conducted at the monthly frequency, taking within-month averages of the higher frequency observations. While we average the short interest at the monthly frequency, having access to daily observations reduces significantly the measurement error relative to the biweekly-sampled short interest variable from the exchanges. We double-cluster standard errors at the month and stock levels. The finding of an increase in the amount of short interest is a likely consequence of the evidence in Diether, Lee, and Werner (2009a) that the volume of short sales rises.[9] The magnitude is economically significant, as the difference in shares on loan for Pilot relative non-Pilot stocks in the Program period is around 9.8% – i.e. (0.348-0.038)/3.146. This magnitude is consistent with the 8.5% increase in short volume for Pilot stocks identified by Diether, Lee, and Werner (2009a). In Panel A of Appendix Table B.4, we show that the result is robust to using different definitions of the treatment and control groups, corresponding to the different samples described in Appendix Table B.2. Moreover, Panel B of Appendix Table B.4 shows the result survives also when letting the sample start in 2004, which addresses the concern of sparser coverage by Markit in the early years (Drechsler and Drechsler, 2014). Diether, Lee, and Werner (2009a) find a positive, but statistically insignificant, increase in short interest during the implementation period for Pilot stocks. However, they measure short interest using the number of shares that are sold short, as reported by the exchanges once a month, without normalizing this variable by the company's shares outstanding or the average daily volume of the stock. This choice likely introduces measurement error as, for example, the sheer number of shares that are sold short may have different meanings for different companies depending on the

[9] In a steady-state equilibrium, short interest equals the flow of short sales times the duration of a short sale. Thus, the finding of an increase in short-sale volume in Diether, Lee, and Werner (2009a) makes it likely that equilibrium short interest also rose for Pilot stocks, which is what we find in Table 2.2, Panel A.

number of shares outstanding. In Panel C of Appendix Table B.4, we show that modifying the variable definition allows us to identify a statistically significant increase in short interest for Pilot stocks, even if we measure short interest with the variable reported from the exchanges, as in Diether, Lee, and Werner (2009a). In particular, we divide this variable by either shares outstanding (columns 1 and 2) or the average daily volume (columns 3 and 4) and we compute standard errors in the same way as in Diether, Lee, and Werner (2009a) – i.e., with the Newey and West (1987) correction for three lags of autocorrelation. Finally, in Panel D of the same table, we show that computing standard errors as in Diether, Lee, and Werner (2009a) does not affect the significance of the result when shares on loan from Markit are normalized by either total shares outstanding (columns 1-2) or average daily volume (columns 3-4). Next, we turn to the effect of Reg SHO on liquidity. In Table 2.2, Panel B, we confirm Diether, Lee, and Werner (2009a) finding of a significant increase in the effective and realized spreads for Pilot stocks during the program period (columns 1 and 3). In this case, our novel contribution is to show that the effects are significantly stronger on days with negative order imbalance on the stock (columns 2 and 4) – i.e., days when short sales are possibly more intense. The economic magnitude is large as the effective spread increases by almost 10% of a standard deviation, based on the estimate in column 2. Decreased liquidity provision and increased trade aggressiveness by short sellers should lead to a larger price impact of trades, especially if short sales are perceived as informed. To test this conjecture, we use two proxies of price impact from the literature. The first one –measuring the permanent move in price due to the trade – is defined as the difference in the mid-quote occurring five minutes after the trade and the mid-quote at the time of trade placement (Glosten, 1987). In columns 5 and 6 of Table 2.2, Panel B, we find an increase in permanent price impact, which is statistically significant on days of negative order imbalance (column 6). In columns 7-8, we focus on a trade-level estimate of Kyle (1985)'s lambda, obtained by regressing price impact on the square root of dollar volume. We find a significant increase in this measure of price impact for treated stocks during the experiment. Because Kyle (1985)'s lambda is closely related to the information asymmetry in the trade, the finding corroborates the view that short sellers acted more aggressively on their private information once the restrictions were lifted. A recent paper by Heath, Ringgenberg, Samadi, and Werner (2019) argues that reusing the same experiments to test effects on different dependent variables raises the probability of Type I errors because of multiple hypotheses testing. This issue is particularly relevant in the context of the Reg SHO, which has been used in several other studies focusing on many different dependent variables.[10] The authors recommend validating the significant relation between the experiment and the main explanatory variable – i.e., the "first

[10] Heath, Ringgenberg, Samadi, and Werner (2019) note that over the years the Reg SHO setting has been used to study a wide-variety of outcome variables including corporate investment, innovation, M& A, managerial myopia, payout policies, incentive contracts, corporate governance, SEO underpricing, CEO turnover, CEO compensation, employee relations, workplace safety, voluntary disclosure, reporting conservatism, disclosure of bad news, disclosure readability, analyst forecast precision,

stage" . Our approach addresses this concern. As we argued above, we do find a significant first-stage relationship – i.e., an increase in shares on loan as a fraction of shares outstanding, for Pilot stocks (Table 2.2, Panel A), as well as evidence of magnified price impact, especially on days with more intense selling (Table 2.2, Panel B). These effects are signals that are picked up by other market participants and can trigger their strategic behavior, which is the focus of this study. The same authors also recommend assessing the validity of the exclusion restriction against the existing findings in the literature. To address this concern, we note that prior papers using this experiment do not point out channels that could create potentially confounding effects on information impounding and strategic trading behavior, which we study here. Some of the existing studies focus on corporate finance or governance variables, which have no relation to our variables of interest. For example, Grullon, Michenaud, and Weston (2015) and De Angelis, Grullon, and Michenaud (2017) identify an effect of the Reg SHO experiment on capital expenditures and compensation policies, respectively. Neither dimension seems to have direct relevance for traders' behavior and price informativeness, especially given the drastically lower frequency of such corporate events. Possibly more relevant for our study, Massa, Zhang, and Zhang (2015) and Fang, Huang, and Karpoff (2016) find that Pilot firms reduced several measures of earnings management. This effect, if anything, is likely to make prices more revealing of information so that finding a decrease in price informativeness is less likely. Li and Zhang (2015) find that Pilot firms reduced the precision and readability of bad news releases. This behavior matters for price discovery around *bad* news, while our results emerge before the release of *good* news. Finally, Massa, Quian, Xu, and Zhang (2015) find that the presence of short sellers induces company insiders to trade more aggressively on negative information. While this behavior may impact price informativeness around *negative* news, it does not seem to matter for *positive* news, which is our focus. Boehmer, Jones, and Zhang (2020) argue that after the end of the Reg SHO experiment there was an indirect effect leading to an increase in short sellers' aggressiveness also on the original Pilot stocks. Our main sample terminates with the end of the Reg SHO experiment, but in robustness analysis, we find results consistent with these authors' claims. The same authors also point out an indirect effect of the opposite sign during the experiment: short sellers migrated from non-pilot stocks to Pilot stocks. This fact constitutes a violation of the stable unit treatment value assumption (SUTVA). This spillover is a concern if one wants to quantify the effect of the rule under consideration. Based on this interpretation, our identification strategy would then measure the overall increase in the exposure to short selling, *both the direct and indirect effects*, during the Reg SHO period. Boehmer, Jones, and Zhang (2020) suggest that the suspension of the uptick rule facilitated arbitrage-based short selling – e.g., index arbitrage – for Pilot stocks, while fundamental-based shorting was not necessarily affected, as

analysts rounding of forecasts, analyst forecast quality, banks' loan monitoring, and banks' loss recognition. Litvak and Black (2016) also raise concerns about the validity of the exclusion restriction for some studies utilizing the Reg SHO experiment.

these long-term strategies can patiently wait for execution on the limit order book. According to this interpretation, the Reg SHO experiment only led to an increase in non-fundamental short-selling. While plausible, this channel does not invalidate our identification strategy. Prior evidence still indicates that short interest, irrespective of its determinants, predicts future price declines (e.g. Boehmer, Jones, and Zhang, 2008; Engelberg, Reed, and Ringgenberg, 2012; Cohen, Diether, and Malloy, 2007; Diether, Lee, and Werner, 2009b) partly as a result of price pressure (Ringgenberg, 2014). To the extent that the rest of the market correctly expected negative price pressure for Pilot stocks, strategically delaying buy trades was still a rational strategy to pursue. Finally, in Appendix Table B.5, we show that the distribution of earnings announcement does not differ between Pilot and control stocks in terms of both the day of the week of the announcement and the size of the earnings surprise. This evidence is important to rule out lower investor attention (Della Vigna and Pollet, 2009) and informational content for the Pilot stocks' announcements.

2.3 Price Informativeness around Earnings Announcements

In this section, we study information impounding around earnings announcements and relate it to short-selling activity. This analysis is suggested by the conjecture that the combined presence in the market of informed investors with a positive view on the stock and short sellers may impact price informativeness. In particular, if the investors with positive information act strategically by delaying and concealing their trades, the presence of short sellers will reduce the impounding of positive information. To study information impounding, we estimate the following difference-in-differences specification:

$$y_{i,t} = \alpha_i + \delta_t + \beta Pilot_i \times Program\ Period_t + X'_{i,t}\gamma + \varepsilon_{i,t}, \tag{2.1}$$

where *Pilot* is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. *Program Period* is an indicator of the time in which the program took place (May 2005 – July 2007). The dependent variable measures price informativeness at the stock level. In our specifications, we include different sets of stock-level characteristics. The control variables are the log of market capitalization, the standard deviation of the previous-year return, Amihud (2002)'s illiquidity ratio, the number of analysts following the company, the previous-year average bid-ask spread, and the size of the earnings surprise relative to analysts' forecasts. All the specifications include stock and time fixed effects. For this reason, the levels of *Pilot* and *Pilot Period* do not appear in the regressions. We double cluster the standard errors at the stock and time level to allow for arbitrary correlation along these two dimensions. We separately study positive and negative earnings surprises. To capture information impounding before earnings announcements, our preferred variable comes from Weller (2018) and is defined as the ratio of cumulative-abnormal returns (CAR)

– i.e., the ratio CAR[-10, -2]/CAR[-10, 1], where day 0 is the day of the earnings release and the abnormal returns are computed relative to the DGTW benchmark (Daniel, Grinblatt, Titman, and Wermers, 1997). Fitting the need to measure information impounding, this variable quantifies the fraction of the total information released during the event that is incorporated in the pre-event period. For robustness, we also consider two variables that, more generally, capture price efficiency in the pre-event window. The first one is the variance ratio (e.g. Lo and MacKinlay, 1988; O'Hara and Ye, 2011) defined as $|1 - \frac{\sigma^2_{weekly}}{5\sigma^2_{daily}}|$. Both the daily and weekly return variances are estimated in the weeks [-5, -2] before an earnings announcement. Higher values of the variance ratio signal deviations from a random walk process, i.e. they correspond to less efficient prices. The second variable is defined as $|\ln\left[\frac{(1+\rho)}{(1-\rho)}\right]|$, where ρ is the cross-correlation between the contemporaneous stock return and the lagged market return (Bris, Goetzmann, and Zhu, 2007; Saffi and Sigurdsson, 2011). The cross-correlation is also estimated in the weeks [-5, -2] before the earnings announcement. Higher values of this variable suggest that stock prices are slow in impounding aggregate information, i.e. they are less informative.[11]

2.3.1 First Evidence on Price Informativeness

Table 2.3, Panel A, reports the estimates of Equation (2.1) using the ratio of CARs as a measure of price informativeness. The main finding is that stock prices impound significantly less information before *positive* earnings releases when short sellers are more likely to trade the stock (columns 1-4). The magnitude is economically large. Specifically, we find that the ratio of pre-period CAR to total CAR is lower by 7.2% before the release of positive news. Given that the average ratio is about 39.5% , this decline represents a sizeable 18% decline relative to the mean. Moreover, given the level of the t-statistics, most of these specifications satisfy the higher critical value (2.82) that Heath, Ringgenberg, Samadi, and Werner (2019) recommend for reusing the Reg SHO experiment, based on the Romano and Wolf (2005) sequential ordering approach that adjusts for multiple hypotheses testing. One could expect that short sellers improve information impounding ahead of *negative* earnings surprises. If this effect more than offsets the loss of information ahead of positive news, it would further strengthen the view that unrestricted short selling leads to an indisputable improvement in price efficiency. However, we do not find evidence of an improvement in price informativeness ahead of negative announcements (columns 5 and 8). The lack of an effect on price efficiency at a point when information is still private is consistent with the results in Engelberg, Reed, and Ringgenberg (2012). These authors show that the informational advantage of short sellers consists mostly of their ability to interpret public information after its release. Going into more detail, Figure 2.1 plots the differential cumulative abnormal return of Pilot stocks relative to

[11] The variance ratio has a correlation of about 11% with the CAR ratio and 5% with the cross-correlation, while the latter has a correlation close to zero with the CAR ratio. These low correlations reassure us that we are gaining independent evidence from the three variables.

non-Pilot stocks on days [-10, 5] around a positive earnings announcement during the Reg SHO experiment.[12] We observe a significant divergence between Pilot and non-Pilot stocks with the former exhibiting significantly lower returns. Hence, it appears that positive information impounding is significantly lower for stocks with lower constraints on short selling. The evidence of a relative price decline for Pilot stocks before the announcement provides the rationale for positively informed investors to act strategically and wait before placing their orders, as in the *waiting game* scenario from Foster and Viswanathan (1996). The divergence persists until the information becomes public. After the announcement, we observe some overshooting for Pilots stocks, followed by a reversal, but this pattern is not statistically significant. Appendix Table B.6 reports the day-by-day abnormal returns and cumulative abnormal returns used in Figure 2.1.

2.3.2 Evidence from Other Measures of Price Efficiency

In what follows, we provide additional evidence on price informativeness ahead of earnings releases using different measures of price efficiency. This analysis allows us to address a potential alternative interpretation of the evidence in Table 2.3, Panel A. If the presence of short sellers improved information impounding irrespective of the direction of news, rather than harming it, we would expect all the private information to be already incorporated by day -10. In this case, the CAR ratio would be very close to zero for Pilot stocks in the two-week pre-announcement period explaining the evidence. With this objection in mind, we directly measure the extent of price efficiency. The variance ratio studies the autocorrelation of returns and the cross-correlation studies the stock price reaction to aggregate information; both variables capture information incorporation. Moreover, we extend the estimation window farther back in time to cover a month before the week in which earnings are announced. In Table 2.3, Panel B, we report estimates of Equation (2.1) using the variance ratio as dependent variable. In this case, a positive and significant slope on the interaction in columns 1-4 suggests that Pilot stocks' prices are less efficient during the program period ahead of positive surprises, confirming the results in Panel A. The magnitude is sizeable as the 0.143 estimate corresponds to about 23% of the mean of the variance ratio (0.629). The insignificant effect for negative earnings releases is also consistent with Panel A. In Panel C of Table 2.3, we repeat the exercise using the cross-correlation as the measure of price informativeness. Again, the positive and significant slope on the interaction in columns 1-4 suggests that prices of Pilot stocks are slower in incorporating aggregate information ahead of positive news – i.e., they are less informative. The economic magnitude of the slope is somewhat lower but still significant at 10% of the mean. As in the other panels, we find no effect in the case of negative earnings surprises. In sum, the results in Table 2.3 reveal that stocks that are more exposed to short selling display lower price informativeness ahead

[12] The plot is based on estimates from a regression specification similar to the one in Table 2.3, Panel A, but run on daily observations and using the DGTW-abnormal returns as a dependent variable.

of positive earnings releases. This evidence is consistent with the hypothesis that the interaction between short sellers and investors with positive information deters information impounding. This result, which is novel in the literature, is not in conflict with previous findings that short selling improves price efficiency. Our analysis focuses on periods before the release of public information. Previous studies (Saffi and Sigurdsson, 2011; Beber and Pagano, 2013; Boehmer and Wu, 2013), instead, focus on the unconditional effect of short selling on price efficiency. Short sellers may improve efficiency over long horizons, especially after the release of public information. At the same time, during concentrated periods when private information is dispersed across several investors, other market participants may possess more informative signals. In these circumstances, the short-selling activity can deter information impounding by positively informed investors and price informativeness declines.

2.3.3 Can Price Pressure from Short Selling Explain the Result?

Our prior results document an increase in short selling activity (Table 2.2, Panel A) as well as an increase in price pressure for Pilot stocks during the experiment period, especially on days of negative order imbalance (Table 2.2, Panel B). Therefore, the price pressure from increased and more aggressive short sales could provide an alternative explanation for the finding in Table 2.3 and Figure 2.1 that Pilot stocks' returns are lower ahead of earnings announcements. To investigate this possibility, we provide an estimate for the price impact of short sales using data on actual sales on the NYSE available through the TAQ database. We estimate the permanent price impact per share using the following regression:

$$Price\ Impact_{i,t} = \alpha_{i,t} + \beta_{i,t} \times Short\ Volume_{\mathrm{i,\,t}} + \varepsilon_{i,t}, \tag{2.2}$$

where *Short Volume* is the number of shares short sold in one transaction, and *Price Impact* is the permanent price impact of the trade computed as the percentage change between the stock price just before execution and the stock price five minutes after the execution of the short sale. We run this regression for the four groups in our Reg SHO experiment; that is, Pilot stocks before and after the implementation of the Program, and Control stocks before and after. β is our estimate of price impact per share. The subscript i suggests that the short sale is executed for a stock in either the Pilot or Control group; while t indicates whether the trade takes place before or after the implementation of the Reg SHO Pilot Program Given the definition of price impact per share in Equation (2.2), to assess the potential price impact of short sales on Pilot stocks during the Reg-SHO period, we compute the following double-difference

$$
\begin{aligned}
&(Price\ Impact_{P,a} - Price\ Impact_{P,b}) - (Price\ Impact_{C,a} - Price\ Impact_{C,b}) = \\
&(\beta_{P,a} \times Short\ Volume_{P,a} - \beta_{P,b} \times Short\ Volume_{P,b}) \\
&- (\beta_{C,a} \times Short\ Volume_{C,a} - \beta_{C,b} \times Short\ Volume_{C,b})
\end{aligned}
\tag{2.3}
$$

where, P and C identify Pilot and Control stocks, respectively, while a and b stand for the period after and before the start of the Reg-SHO Program. Short Volume is the average total daily short-selling volume, expressed in shares. Effectively, using the estimated price impact per share, we compare price impact for Pilot stocks to that for the control group, before and after the start of the program. Appendix Table B.7 reports the quantities that are used in the estimation and the results of the computations. This exercise shows that a price decline of 3.07 bps *per day* can originate from the increase in short sales due to Reg-SHO. This estimate represents an upper bound because it assumes that there is no reversal of this price impact from the closure of the short positions (this could happen, e.g., if short sellers place limit buy orders to close their positions). Based on this daily estimate, in the 10 days before a positive earnings announcement, short selling can reduce the price of Pilot stocks relative to Control stocks by about 31 bps (i.e., 10×3.1 bps). This effect is roughly *20%* of the estimate that we report in Figure 2.1 (i.e., 150 bps). Thus, we conclude that increased price pressure from short sellers is not sufficient to explain the lower returns on Pilots stocks ahead of earnings announcements. Importantly, the reduction in liquidity in Pilot stocks that follows from the shift of short sellers from liquidity providers to liquidity demanders, which is evident in Table B.6 from the fact that the permanent price impact for Pilot stocks turns positive during the program period, allows us to discuss another potential channel for the main evidence in this section. In particular, positively-informed market participants, facing lower liquidity on the ask side, can decide to trade more cautiously to avoid large price impact. This strategic response of buyers can lead to slower information impounding in Pilot stocks. This alternative explanation differs from the buyers' intention to wait for a price decline to buy the asset at a lower price, which is the main testable conjecture in the paper. The evidence in the next section will allow us to tease these motives apart.

2.4 A Potential Channel for Reduced Information Impounding

Next, we study the conjecture that the observed slow-down in information impounding in Pilot stocks is driven by the strategic behavior of positively informed investors. To test for this conjecture, in this section, we analyze investors' behavior along two dimensions. First, we investigate investors' trading speed, as our working hypothesis implies that investors slow down their trades by shifting the bulk of their trading volume closer to the news release. This strategy would allow them to profit from the price decline induced by the short sellers. At the same time, this behavior would also reduce price impact as a response to the decreased liquidity on the ask-side of the book resulting from more aggressive short selling. Second, because order flow information can be disseminated by brokers (Barbon, Di Maggio, Franzoni, and Landier, 2019; Di Maggio, Franzoni, Kermani, and Sommavilla, 2019), investors might decide to break up their trades across multiple brokers to prevent information leakage. According to this hypothesis, dissemination of positive information should be avoided because it would counteract the negative price pressure imparted by short selling and, possibly, would induce short sellers to revise their priors and trade less aggressively, or it would convince other investors to purchase the stock. This behavior is also useful to mitigate price impact in response to decreased liquidity, which is the other potential motive for strategic behavior that we entertain.

2.4.1 Trading Speed

We start our analysis by testing the conjecture that short-selling activity slows down the trades of informed investors. We adopt the same specification as in Equation (2.1). Our first measure of trading speed is the ratio between the total dollar volume executed by active managers in a given stock in the days $[-10, -2]$ before the announcement to total active managers' volume in that stock over the days $[-10, 1]$. Intuitively, a lower value of this variable implies that investors' trading activity is less intense in the days before the announcement, consistent with a slowdown in trading speed. Table 2.4 reports the results of this analysis. We analyze the behavior of positively and negatively informed investors by looking at the estimates for buy trades (Panel A) and sell trades (Panel B). The separation between positive (columns 1-4) and negative news (columns 5-8) allows us to relate this analysis to the result in Table 2.3 that the reduction in information impounding occurs before positive news. In Panel A, we find a significantly negative difference-in-differences coefficient, consistent with a strategic delay in trading by privately informed investors before the release of positive news for stocks more exposed to short selling. The economic magnitude is sizeable, as the estimated 2% decrease corresponds to about 7% of a standard deviation. There is no effect in the case of negative news, consistent with the findings in Table 2.3. In Panel B, we find no evidence of a change in trading speed for sell trades. This result corroborates the view that only investors with positive priors have an incentive to slow down their trades to profit from the price decline induced by short sellers. Still, one may conjecture that negatively informed investors have the opposite incentive – i.e., they may want to rush their trades to preempt the price decline. The fact that we do not find supporting evidence for this conjecture may indicate that the sell trades that we analyze in Table 2.5 are not a good proxy for the behavior of negatively informed investors. In general, given the small cash buffers that institutions hold, sell trades are more likely than buy trades to reflect a liquidity motive (Chen, Goldstein, and Jiang, 2010). That is, while institutions can allocate new money slowly and wait for attractive opportunities, they are often in a rush to liquidate their positions if they need to respond to redemptions. In section 5, we account for this possibility by re-running this analysis for a subset of investors, hedge funds, whose trades are less constrained by a liquidity motive and find significance on sell trades as well. Providing more granular evidence, Figure 2.2 plots the cumulative buy volume ratio of Pilot stocks relative to non-Pilot stocks on days [-10, 1] around a positive earnings announcement, during the Reg SHO experiment. The line is based on estimates from a regression specification similar to the one reported in Table 2.5, but estimated on daily observations and using the fraction of total period dollar volume executed in one day. We observe a significant divergence between Pilot and non-Pilot stocks, with the former exhibiting a significantly lower fraction of volume executed before the announcement. Similar to the price pattern of Figure 2.1, this divergence persists until one day before the announcement, after which there is a sudden convergence between Pilot and non-Pilot stocks. The daily estimates together with the t-statistics for this figure are reported in Appendix Table B.8.[13] As a final piece of evidence, we provide a different perspective on trading speed that relies entirely on the trades before the announcement. We measure the average size of each buy trade in a given stock, either in dollars or in number of shares (both in logs), in the days $[-10, -2]$ before the announcement. Trade size is an alternative proxy for trading speed because smaller trades can lead to slower executions for a given amount of total volume. This alternative measure also addresses the concern that the denominator of the first trading speed variable includes trades occurring on the day of the announcement, which can be bigger following positive announcements – e.g. because of positive-feedback trading – leading to a mechanically lower trading speed variable.[14] These measures have a low correlation, about 18% , with trading speed. In Table 2.5, we find that the average size of the trade, measured in both dollars and shares, decreases significantly ahead of positive news. These result confirms the evidence in Table 2.5 and is consistent with the intent of positively-informed investors to slow down their executions. Analogously to prior results, we find no effect for negative news.

2.4.2 Strategic Use of Brokers

Next, we bring to the data the conjecture that informed investors' reaction to short selling also involves a hiding behavior. In particular, informed investors with positive information may decide to make their trades less visible fearing that short sellers

[13] Note that we do not necessarily expect positively informed investors to increase their trading speed in Pilot stocks before the announcement. In fact, in Figure 2.1, the price of Pilot stocks on day 0 is still lower than for non-Pilot stocks. This fact may provide an incentive to positively informed investors to postpone their buying activity until days 0 and 1.

[14] This mechanical explanation, however, does not generate a lower the trading speed variable for Pilot stocks during the program period.

will update their priors if they observe concentrated buying activity. Moreover, according to an additional and complementary interpretation, buyers can decide to conceal their trades to reduce price impact after observing lower liquidity in the market as short sellers become more aggressive. Given the recent evidence that brokers collect and spread information in the market (Di Maggio, Franzoni, Kermani, and Sommavilla, 2019; Barbon, Di Maggio, Franzoni, and Landier, 2019), informed investors can prevent information leakage by splitting the trade through multiple brokers. Hence, we study whether hiding behavior implies an increase in broker splitting for Pilot stocks after the Reg SHO Pilot Program is in place. We measure broker splitting with a dummy variable equal to one if the average number of brokers used by ANcerno managers to trade a stock in the window [-10, -2] before an earnings announcement is above the median value of the sample distribution. From Table 2.1, the median number of brokers across managers at the stock level is about 1.7. Table 2.6, Panel A, shows the results of a difference-in-differences specification similar to Equation (2.1). Again, to better convey the evidence on the mechanism that we conjecture, we distinguish between positive (columns 1-4) and negative news (columns 5-8). We find a significant increase in the number of brokers that managers use to execute trades on Pilot stocks during the experiment. The coefficient in column 1 suggests that managers buying Pilot stocks during the Reg SHO Pilot Program period have 4% more chances of using an abnormal number of brokers in the two weeks preceding an earnings announcement. Moreover, our inference falls within a more conservative confidence interval for multiple hypothesis testing (Romano and Wolf, 2005). The t-statistics of 2.88 is above the adjusted critical value of 2.82 for multiple hypotheses testing applied to the Reg SHO experiment (see Heath, Ringgenberg, Samadi, and Werner, 2019). There is no evidence of an increase in broker splitting ahead of negative news, consistent with the evidence in Table 2.3 that reduction in information impounding occurs only for positive news. We can further qualify the strategic behavior in terms of the types of brokers that are chosen. Given that central brokers are more likely to engage in information leakage (e.g. Di Maggio, Franzoni, Kermani, and Sommavilla, 2019), we conjecture that informed investors wishing to prevent information leakage should turn to more peripheral brokers. Like these authors, we use the notion of eigenvector centrality to define the "central" brokers applying it to the ANcerno data (Bonacich, 1972, 1987; Katz, 1953; Bonacich and Lloyd, 2001). Our definition of centrality takes into account all direct and indirect institutional trading partners of a given broker and is computed by assigning scores to all brokers and managers (i.e. institutional investors) in the bipartite network of trading relations. A broker-manager connection is weighted by the fraction of the total volume of the broker that is executed with the manager, where the volumes are computed over the prior six months.[15] The results in Panel B of Table 2.6 confirm the conjecture that informed investors respond strategically

[15] To limit noise in the definition of the broker network, we focus on the trades executed through the top 30 brokers by volume in the prior six months. These brokers intermediate more than 80% of the whole volume in ANcerno. Moreover, to use the centrality measure in our setting, we define an

to the stock's exposure to the short-selling activity. Before positive news, buy trades are 2% less likely to be directed towards central brokers for Pilot stocks. Given the average level of this probability is about 5% , the magnitude of this effect is economically significant. Consistent with the pattern in the rest of the analysis, we find no effect ahead of negative news. We can qualify the choice of brokers even further. If informed investors deviate from their typical trading patterns to hide from short sellers, they will turn to brokers with whom they had less frequent interactions in the past. To test this conjecture, we construct a measure of broker familiarity. In particular, we compute the share of dollar volume traded by a manager with each broker in the last year; then we adjust this value and multiply by the average number of brokers used by the investor in the same period. Intuitively, high values of this variable suggest that the broker intermediates a higher fraction of the manager's trades. Finally, we compute the volume-weighted average of our broker-manager familiarity proxy at the stock level for the trades occurring in the usual pre-announcement window (days [-10, -2]). Panel C of Table 2.6 reports the estimates from the difference-in-differences specification using broker familiarity as the dependent variable. Investors appear to trade with significantly less familiar brokers when buying a Pilot stock before a positive earnings announcement in the Reg SHO period. As usual, we find no effect ahead of negative news. Moreover, in some specifications, the t-statistics exceed the more restrictive statistical significance threshold suggested by Heath, Ringgenberg, Samadi, and Werner (2019). In terms of economic magnitude, the coefficients in columns 1-4 suggest a decrease in familiarity of about 9% of one standard deviation for Pilot stocks during the Program period.

2.5 Alternative Hypothesis, Further Evidence, and Robustness

2.5.1 Learning from Short Sellers

An alternative interpretation of the observed slowdown in the speed of buy trades is that investors revise their priors about fundamentals downwards for stocks with more short-selling activity, as investors find these assets less attractive. In other words, informed investors learn from short sellers about the poor fundamentals of stocks and reduce their long exposure. A similar prediction appears in Diamond and Verrecchia (1987) and finds confirmation in Senchack and Starks (1993). The test we propose to separate this alternative explanation from the conjecture of strategic behavior is based on the following argument. If positively informed investors

indicator for the centrality of brokers used by the managers buying the stock in the window [-10, -2]. From the raw measure computed from past trades, we compute the percentiles of the distribution across brokers. We finally volume weight the percentile at the stock-announcement level and define the dummy equal to 1 if the volume-weighted percentile is greater or equal to 75. Intuitively, a broker is central if it trades with many institutions, which in turn trade with many brokers, and so on.

aim to take advantage of the temporary reduction in price due to the potential exposure to short-selling activity, we expect them to buy a larger total amount of the stock, as the stock price becomes more attractive thanks to the action of the short sellers, than they would have done for another stock with a less prominent presence of short sellers. Instead, if investors are less convinced of their positive information after observing short selling, we would expect a decrease in the overall buy volume. We tease out these hypotheses in Table 2.7, using the Reg SHO experiment as a source of exogenous variation for potential short-selling activity. In particular, we examine whether the total (log) dollar volume traded by a manager in the window [-10, 1] around an earnings release is higher for Pilot stocks during the experiment. Again, our hypothesis concerns investors with a positive view on the stock, thus we focus on buy trades only and provide information on sell trades in later analysis. Columns 1-4 focus on positive news, while columns 5-8 focus on negative news. The dependent variable is aggregated at the stock level by summing volumes across active investors. We find that, around positive news, overall buy volume increases by about 7% for Pilot stocks. The coefficients for negative news are smaller in magnitude and not statistically significant in the fully-fledged specification with all the stock-specific control variables. Overall, the trading behavior we uncover corroborates the hypothesis of a strategic response by informed traders, who delay and hide their trades when short selling is more likely. Finally, this evidence contributes to give more credibility to the strategic timing of trades, as opposed to the mere containment of the total price impact, which is another potential explanation of the evidence of more cautious trading, as the main motive for the strategic slowdown and breakup of trades. Indeed, the wish to reduce the total price impact is hardly consistent with the evidence that total buy volume increases for Pilot stocks.

2.5.2 Hedge Funds' Informed Trading

One may wonder whether any strategic behavior takes place in case the informed investors hold a negative view of the firm's fundamentals. The analysis in Table 2.5 does not uncover any evidence of strategic behavior in sell trades. This might appear counterintuitive as a rational response to short selling for an investor who intends to sell the stock may be to speed up the liquidation, ahead of the price decline that short sellers induce. We point out that the lack of any effect for the sample of ANcerno informed investors may be because sell trades, more than buy trades, reflect a liquidity motive –i.e., the need to return cash to the fund investors, as opposed to an information motive. To address this limitation, we focus on a particular subset of investors whose sell trades are more likely to reflect negative information than a liquidity motive: the hedge funds. Indeed, redemption restrictions allow hedge funds more leeway in their liquidation decisions. More explicitly, if hedge funds need to return cash to their investors, their trading horizon is sufficiently long to allow them to choose the stocks they liquidate based on information reasons. Among the ANcerno institutions, we select 96 hedge funds following the procedure in Çötelioğlu, Franzoni, and Plazzi (2020). To strengthen this identification further, we select those hedge fund trades that are more likely to reflect an information motive – i.e., they are in the same direction of the earnings surprise and they involve a limited fraction of the portfolio positions. A fund selling a large fraction of its portfolio is more likely to be involved in a liquidity-driven fire sale (e.g. Barbon, Di Maggio, Franzoni, and Landier, 2019). We define an informed hedge fund buy (sale) by focusing on positive (negative) earnings releases and selecting those transactions for which the total imbalance for the manager in the stock over the window [-10, -2] is positive (negative) and less than 50% of the stocks in the hedge fund portfolio are exchanged.[16] Mirroring the analysis in Table 2.4, we run a difference-in-differences regression in which the dependent variable is our proxy for trading speed based on the ratio of pre-announcement to total period dollar volume. Table 2.8 shows the results. Columns 1-4, focusing on buy trades, report a significant decrease in trading speed of a similar magnitude to the one in Table 2.4. Our main interest is in columns 5-8, which focus on informed sell trades. We find a significant rise in trading speed for sell trades in Pilot stocks during the Reg SHO period. As for the magnitude, the increase in speed is 4.5% , which corresponds to a sizeable increase of roughly 15% of a standard deviation for this variable. Therefore, the evidence suggests that informed investors' strategic behavior is not peculiar to buy trades, but it shows up also when informed investors and short sellers compete in the same direction of the trade. However, the strategic interaction between short sellers and negatively informed investors is not novel to the literature. Massa, Quian, Xu, and Zhang (2015) show that company insiders liquidate their shares faster when short sellers are around. For this reason, the main focus of our paper remains the interaction between short sellers and positively informed investors.

2.5.3 Further Evidence and Robustness

In the Internet Appendix, we provide further evidence corroborating the main results of the paper. Table B.9 supports the conclusion that the evidence of order splitting across multiple brokers reflects strategic behavior by showing that this behavior halts after the information becomes public. Table B.10, Panel A, studies sell trades focusing on the dependent variables from Tables 5-7 finding no significant effect for Pilot stocks during the Program period. Panels B, C, and D of Table B.10 show that when the main analysis is conducted on alternative samples – that is, either before or after the actual implementation of the Reg SHO Pilot Program – the interaction coefficients are overall statistically insignificant. This evidence reassures us about the validity of our identification strategy.

[16] Following Barbon, Di Maggio, Franzoni, Landier (2019), we construct portfolio holdings from ANcerno trades by cumulating the trades in each stock over a two-year rolling window.

2.6 Conclusions

It is a commonly held view that short selling improves the informational content of asset prices. However, the presence of short sellers in the market can modify the behavior of other informed investors. We conjecture that positively informed investors react strategically to the presence of short sellers in the market. In particular, they may slow down and conceal their trades to let the short sellers push down prices. This behavior would allow positively informed investors to purchase the asset at a lower price. Because of this decrease in trading aggressiveness, prices can incorporate positive information more slowly for stocks that are more exposed to the short-selling activity. In this paper, we study how stock price informativeness before earnings announcements varies as a result of an exogenous variation in the exposure to short selling, as generated by the Reg SHO experiment. We also describe the trading behavior of informed investors as a response to short selling, using institutional trading data (ANcerno). We show that prices are less reflective of positive information for stocks more exposed to short selling during the Reg SHO experiment. Studying the channel, we show that this finding cannot be the mere result of increased price pressure by short sellers. Instead, we find that positively-informed traders react to short-selling activity by delaying their trades. Moreover, investors spread their trades across multiple brokers when short interest is higher, arguably to let short sellers run their course. Our results have implications for the debate around the role of short selling in financial markets. Our findings point out a setting in which short selling does not improve price efficiency. In particular, when some investors in the market receive a more precise signal than short sellers, short activity can deter traders with positive views from timely impounding their information into prices. We wish to stress that the setting that we focus on, i.e. the period before earnings announcements, is a convenient laboratory to study these issues. However, the evidence is likely to generalize to instances when different investors in the market receive different private signals. In this sense, the tests in our paper can be construed as validating the theoretical predictions of Foster and Viswanathan (1996). In sum, while short selling can improve price efficiency unconditionally, it can slow down information impounding when other investors hold competing beliefs about fundamentals and receive more informative signals. Drawing regulatory implications is beyond the scope of this work. In particular, the beneficiary role of short selling in preventing overvaluation and asset price bubbles is well-established in the literature and our results do not contradict this point of view. Thus, there is a clear tradeoff between discouraging uninformed short selling ahead of information releases, which according to our evidence seems to be detrimental for information impounding and allowing the short seller to prevent overvaluation unconditionally. This tradeoff can, perhaps, be resolved with regulation that is contingent on the extent of information dispersion across investors and on the timing of specific information releases. Arguably, the practical implementation of this policy presents many difficulties, not least the measurement of information dispersion and the design of state-contingent rules for short selling. Thus, we refrain from making policy recommendations and defer this task to future research.

2.7 Tables

Table 2.1: Variables description and summary statistics. In Panel A we show the definition of the variables used in the analysis, while Panels B-D report their mean, standard deviation, 25th, 50th and 75th percentiles, and the number of observations. In Panel B, we report statistics for the sample of earnings announcements. The sample is at the stock-announcement level. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. We report statistics for positive and negative news separately. When we consider price informativeness, we restrict the sample to the earnings announcements for which $|CAR_{(i,t)}[-10,1]| > \sqrt{12 \times \sigma_{(i,t)}}$ for stock i around time t earnings announcement (Weller, 2018). Where $\sigma_{(i,t)}$ is the return standard deviation computed in the previous month starting on day -11 before the announcement, and the CAR is computed with respect to the DGTW benchmark. Panel C reports statistics for the short-selling variables that we use as dependent variables for our first stage regressions. In this case, the sample is at the stock-month level. Finally, Panel D shows statistics for the TAQ variables at the stock-day level. We distinguish between the whole sample and the restricted sample of days in which the sell volume is greater than the buy volume. In all Panels, we focus on the period between May 2002 and July 2007 and consider Russell 3000 stocks as described in Table **??**.

Panel A	Description of main variables
ANcerno variables	
Trading speed	The fraction of total dollar volume executed in the window $[-10,-2]$ before the release of earnings information. The total dollar volume is computed in the window $[-10,1]$. We sum volume across active managers for a given stock.
Trade size (log)	Size of a single execution measured using either the dollar or the share volume. This variable is averaged across all active investors for each stock in the window $[-10,-2]$ before an earnings announcement.
High number of brokers	Dummy equal to 1 if the average number of brokers used to trade a stock before an earnings announcement is above the sample median.
(log) Dollar volume	Log-dollar volume executed in a stock in the window $[-10,1]$ around an earnings announcement.
Broker Familiarity	The ratio of the manager's dollar volume with a broker to the total manager's volume (scaled by the average number of brokers used). The measure is computed in the past year and averaged (volume-weighted) at the stock level for the trades occurring before an earnings announcement.
Broker Centrality	Indicator for high centrality of brokers used by managers to trade a stock in the window $[-10,-2]$ before an earnings announcement. We compute this measure as the (volume-weighted) eigenvector centrality in the six months before the earnings announcement. Then, from this row measure, we compute the percentiles of the distribution across brokers. We finally compute the volume-weighted percentile at the stock-announcement level and define the dummy equal to 1 if the volume-weighted percentile is greater or equal to 75.
Markit variables	
Shares on loan	Monthly average of the value of shares on loan as a percentage of market cap (Markit) .
Shares on loan (% of average volume)	Monthly average of the value of shares on loan as a percentage of average dollar-volume (Markit).
Short interest (% of average volume)	Monthly short interest in shares as a percentage of average share volume (Compustat Supplemental Short Interest file).
CRSP and IBES variables	
CAR Ratio	Defined as $CAR_{[t-j,t]}/CAR_{[t-j,t+h]}$, where CAR is the cumulative (DGTW) abnormal return. We choose t-j equal to day -10, t equal to day -2, and t+h equal to day +1.
Variance Ratio	$\lvert 1 - \frac{\sigma^2_{weekly}}{5\sigma^2_{daily}} \rvert$, where σ^2_{weekly} is the variance of weekly returns, while σ^2_{daily} is the variance of daily returns. Variances are computed in weeks $[-5,-2]$ before an earnings announcement.
Cross-correlation	$\lvert ln[\frac{(1+\rho)}{(1-\rho)}] \rvert$, where ρ is the correlation between weekly stock returns at time t, and the value-weighted market returns at time t-1, computed in weeks $[-5,-2]$ before an earnings announcement.
Market capitalization	Log-market capitalization lagged one period.
Amihud illiquidity	Previous year average of $10^6 \times \lvert ret \rvert / \$\,Volume$.
Number of analysts	Log-number of analysts recorded in I/B/E/S that issue earnings forecast within 90 days before the report date.
Bid-ask spread	Previous year average daily bid-ask spread.
Stock volatility (log)	The previous year (log) standard deviation of daily stock returns.
TAQ variables	
Note on symbols	P_k = price of trade k, M_k = bid-ask mid-price at time k, $DVol_k$ = \$-volume of trade k, D_k= 1 (-1) if trade k is a buy (sell)
Effective spread	Daily average of $2D_k(P_k - M_k)/M_k$.
Realized spread	Daily average of $2D_k(P_k - M_{k+5})/M_k$, where M_{k+5} is the bid-ask mid-price 5 minutes after the k-th trade.
Permanent price impact	Daily average of $2D_k(M_{k+5} - M_k)/M_k$, where M_{k+5} is the bid-ask mid-price 5 minutes after the k-th trade.
Kyle lambda	Coefficient of the following regressions: $Ln\frac{M_{i,t}}{M_{i,t-300}} = \alpha + \lambda \times SSqrtDVol + \varepsilon$, where $SSqrtDVol = Sgn(\sum_{(t-300)}^{t} DVolBuy - \sum_{(t-300)}^{t} DVolSell) \times \sqrt{(\lvert(\sum_{(t-300)}^{t} DVolBuy - \sum_{(t-300)}^{t} DVolSell)\rvert)}$, and $M_{i,t}$ is the bid-ask mid-price for stock i at second t.

Panel B	Summary statistics for earnings announcement analysis					
	Positive news					
	Mean	SD	p25	p50	p75	N
CAR Ratio	0.395	0.477	0.094	0.382	0.695	8,454
Variance Ratio	0.629	0.719	0.252	0.508	0.791	8,454
Cross-correlation	1.359	1.148	0.500	1.080	1.903	8,454
Trading speed	0.661	0.277	0.462	0.722	0.903	23,240
Trading speed (Hedge funds)	0.845	0.277	0.793	1.000	1.000	13,323
Trade Size (log-Dollars)	11.358	1.301	10.527	11.470	12.306	23,240
Trade Size (log-Shares)	8.287	1.343	7.463	8.400	9.223	23,240
Number of brokers	1.827	0.667	1.333	1.750	2.200	23,240
Broker centrality	0.051	0.219	0.000	0.000	0.000	23,240
Broker familiarity	5.763	4.932	2.480	4.432	7.360	23,240
(log) Dollar volume	1.768	1.377	0.564	1.539	2.719	23,240
Market capitalization	21.146	1.488	20.066	20.950	22.024	23,240
Stock volatility (log)	-3.805	0.438	-4.102	-3.815	-3.520	23,240
Amihud illiquidity	0.018	0.091	0.000	0.002	0.008	23,240
Bid-Ask spread	0.044	0.037	0.024	0.035	0.052	23,240
Number of analysts	1.530	0.899	0.693	1.609	2.197	23,240
	Negative news					
	Mean	SD	p25	p50	p75	N
CAR Ratio	0.399	0.459	0.100	0.368	0.702	2,850
Variance Ratio	0.711	1.263	0.265	0.519	0.808	2,850
Cross-correlation	1.385	1.219	0.509	1.086	1.907	2,850
Trading speed	0.664	0.288	0.457	0.725	0.922	9,398
Trading speed (Hedge funds)	0.844	0.282	0.811	1.000	1.000	3,746
Trade Size (log-Dollars)	11.024	1.395	10.088	11.123	12.034	9,398
Trade Size (log-Shares)	8.015	1.496	7.052	8.137	9.064	9,398
Number of brokers	1.789	0.712	1.250	1.667	2.138	9,398
Broker centrality	0.058	0.235	0.000	0.000	0.000	9,398
Broker familiarity	6.218	5.537	2.421	4.638	8.117	9,398
(log) Dollar volume	1.405	1.296	0.317	1.048	2.192	9,398
Market capitalization	20.790	1.384	19.762	20.584	21.613	9,398
Stock volatility (log)	-3.807	0.453	-4.112	-3.819	-3.518	9,398
Amihud illiquidity	0.021	0.118	0.001	0.003	0.014	9,398
Bid-Ask spread	0.039	0.029	0.023	0.032	0.046	9,398
Number of analysts	1.343	0.884	0.693	1.386	1.946	9,398

Panel C	Correlations					
	Positive news			Negative news		
	CAR Ratio	Variance Ratio	Cross Correlation	CAR Ratio	Variance Ratio	Cross Correlation
CAR Ratio	1.000			1.000		
Variance Ratio	0.103	1.000		0.138	1.000	
Cross Correlation	-0.008	0.050	1.000	0.014	0.044	1.000
	Trading speed	Trade size (Dollars)	Trade size (Shares)	Trading speed	Trade size (Dollars)	Trade size (Shares)
Trading speed	1.000			1.000		
Trade size (log-Dollars)	0.181	1.000		0.149	1.000	
Trades size (log-Shares)	0.179	0.801	1.000	0.171	0.803	1.000

Panel D	Summary statistics for short selling variables					
	Markit variables					
	Mean	SD	p25	p50	p75	N
Shares on loan	2.406	3.636	0.114	0.851	3.051	132,464
Shares on loan (% of volume)	15.750	20.346	1.182	6.785	22.765	131,809
	Compustat variables					
	Mean	SD	p25	p50	p75	N
Short interest (% of volume)	28.727	20.018	13.428	23.776	39.699	136,077

Panel E	Summary statistics for TAQ variables					
	All sample					
	Mean	SD	p25	p50	p75	N
Effective spread (bps)	19.210	22.763	5.850	10.925	22.740	1,883,729
Realized spread (bps)	6.981	14.941	0.469	2.566	7.636	1,883,607
Price impact (bps)	11.892	13.749	3.694	7.259	14.515	1,883,731
Kyle lambda ($\times$ 10,000)	0.015	0.037	0.001	0.007	0.022	1,831,843
	Sell volume > Buy volume					
	Mean	SD	p25	p50	p75	N
Effective spread (bps)	22.766	25.148	6.956	13.714	28.234	751,593
Realized spread (bps)	8.578	16.885	0.605	3.184	10.100	747,194
Price impact (bps)	13.838	15.124	4.326	8.849	17.381	748,193
Kyle lambda ($\times$ 10,000)	0.018	0.042	0.001	0.009	0.028	719,278

Table 2.2: Short-Selling Activity and Price Impact around Reg SHO. This table reports results for the difference-in-differences analysis of key variables around Reg SHO. In Panel A, we report estimates for a difference-in-differences and a generalized difference-in-differences model for the value of shares on loan as a percent of market cap (monthly average). We retrieve the short selling variable from Markit. The sample is at the stock-month level and spans the period between May 2002 (when the Markit sample begins) to July 2007 (when the Pilot Program ends). In columns (2) and (4) we include as controls the stock volatility, the market cap, the bid-ask spread and the Amihud illiquidy. In Panel B, we use TAQ data to compute the dependent variable. The sample is at the stocks-day level and considers averages of the dependent variables, which are defined at the trade level. The only exception is the proxy for Kyle lambda, which is obtained at the stock-day level from an OLS regression. Effective spread is defined as 2 times the difference between the price of a trade and the bid-ask mid quote at the time of the trade, scaled by the latter. This amount is then multiplied by 1 if the trade is a buy and -1 if it is a sell. Realized spread is defined similarly with the exception that the difference is taken with respect to the bid-ask mid quote 5 minutes after the trade (the scaling variable is the same). Permanent price impact is defined as the affected spread, but the mid quote at 5 minutes after the trade is used instead of the trade price. Finally, Kyle lambda is defined from a regression (with or without intercept) of the log difference between the mid quote at second t and that at second t-300 onto the signed squared root of the difference between the buy and sell volume between second t-300 and second t. In all the Panels, standard errors are double-clustered at the stock and month level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A				
Dependent variable	Shares on loan (%)			
	(1)	(2)	(3)	(4)
Pilot × Program Period	0.348***	0.373***	0.323**	0.390***
	(2.580)	(2.681)	(2.252)	(2.689)
Pilot	-0.038	-0.005		
	(-0.662)	(-0.087)		
Program Period	3.146***	3.368***		
	(14.806)	(15.110)		
Constant	0.991***	0.539***		
	(9.431)	(4.846)		
Controls	No	Yes	No	Yes
Stock FE	No	No	Yes	Yes
Time FE	No	No	Yes	Yes
Observations	132,464	125,092	132,464	125,090
R-squared	0.198	0.229	0.578	0.596

Panel B

	Effective spread (bps)		Realized spread (bps)		Permanent price impact (bps)		Kyle lambda (× 10,000)	
	All sample	Sell > Buy	All sample	Sell > Buy	All sample	Sell > Buy	All sample	Sell > Buy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	1.550***	2.145***	1.196***	1.540***	0.300	0.620**	0.001**	0.001***
	(2.999)	(3.378)	(3.650)	(3.756)	(1.372)	(2.293)	(2.511)	(2.640)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,883,729	751,592	1,883,607	747,193	1,883,731	748,193	1,831,843	719,278
R-squared	0.661	0.653	0.414	0.417	0.457	0.433	0.192	0.181

Table 2.3: Earnings announcements and price informativeness around Reg SHO. This table reports results for the following diff-in-diff regression around Reg SHO:

$$y_{i,t} = \alpha_i + \delta_t + \beta(Pilot_i \times Program\ Period)_t + X'_{i,t}\gamma + \varepsilon_{i,t}\ ,$$

where, $y_{i,t}$ is a variable measuring price informativeness, using different definitions. In Panel A, it is defined as the ratio of CAR[-10, -2] to CAR[-10, 1], with day 0 being the day of the earnings release. Abnormal returns are computed with respect to the DGTW benchmark. Panel B display results for variance ratios computed using weekly and daily returns. The variance ratio is defined as the absolute value of one minus the ratio of weekly to daily return variance, where the daily variance is multiplied by 5. We consider weeks [-5, -1] before the earnings announcements instead. In Panel C we use the cross-correlation between the current stock returns and the lagged market returns. For each earnings announcement, we compute the correlation, ρ, between weekly stock returns at time t, and the value-weighted market returns at time t-1. Following Saffi and Sigurdsson (2011), since correlations are bounded between -1 and 1, we apply the transformation $ln[(1+\rho)/(1-\rho)]$. To compute the correlations we use data from weeks [-5, -2] before and earnings announcement. Pilot is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. Program Period is an indicator for the time in which the program took place (May 2005 – July 2007). The vector of control variables, $X'_{i,t}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, previous year average bid-ask spread, and the earnings surprise. In columns (1)-(4), we show estimates for positive earnings surprises, while in columns (5)-(8), we focus on negative news. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. As in Weller (2018), we consider the domain on which our dependent variable has a meaningful distribution and focus on the subsample for which the total information release, as proxied by the total event CAR, is high enough (see the caption on Table 2.1). The sample is at the stock-event level and spans the period between May 2002 and July 2007. We consider the subsample of announcements in which active managers are present in the market. A manager is active if the adjusted R-squared of the regression of next month trading in a stock (as a % of total volume treaded next month) onto current stock holdings (as a percentage of portfolio holdings) ranks below the median of the across-managers R-squared distribution. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Panel A	Dependent variable: CAR Ratio							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	-0.072***	-0.072***	-0.071***	-0.070***	0.001	0.003	0.001	-0.001
	(-2.849)	(-2.832)	(-2.800)	(-2.772)	(0.015)	(0.058)	(0.014)	(-0.011)
Return Volatility		-0.010	-0.009	-0.016		-0.017	-0.032	-0.037
		(-0.604)	(-0.580)	(-0.942)		(-0.543)	(-1.021)	(-1.106)
Market Cap			0.036	0.034			-0.165***	-0.151***
			(1.439)	(1.220)			(-3.294)	(-2.774)
Amihud Illiquidity				0.008				0.050*
				(1.215)				(1.746)
Bid-Ask spread				0.035***				0.015
				(2.974)				(0.446)
Number of Analysts				0.021**				0.029
				(2.212)				(1.505)
Surprise				-0.017				-0.002
				(-1.098)				(-0.139)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,454	8,454	8,454	8,454	2,850	2,850	2,850	2,850
R-squared	0.375	0.375	0.375	0.377	0.551	0.551	0.554	0.556

Panel B	Dependent variable: Variance Ratio							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	0.143***	0.141***	0.138***	0.138***	0.043	0.041	0.038	0.035
	(2.767)	(2.734)	(2.686)	(2.692)	(0.324)	(0.305)	(0.286)	(0.258)
Return Volatility		0.047*	0.046*	0.046*		0.018	0.001	-0.004
		(1.936)	(1.893)	(1.870)		(0.243)	(0.020)	(-0.045)
Market Cap			-0.126***	-0.128**			-0.191	-0.069
			(-2.697)	(-2.524)			(-1.053)	(-0.374)
Amihud Illiquidity				-0.004				-0.001
				(-0.264)				(-0.020)
Bid-Ask spread				-0.017				-0.001
				(-1.009)				(-0.014)
Number of Analysts				0.035**				-0.025
				(2.414)				(-0.435)
Surprise				0.018				-0.175***
				(1.268)				(-2.922)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,454	8,454	8,454	8,454	2,850	2,850	2,850	2,850
R-squared	0.354	0.355	0.355	0.356	0.492	0.492	0.493	0.500

Panel C	Dependent variable: Cross-correlation							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	0.140**	0.143**	0.143**	0.145**	0.022	0.032	0.034	0.034
	(2.297)	(2.334)	(2.335)	(2.372)	(0.185)	(0.267)	(0.282)	(0.287)
Return Volatility		-0.053	-0.053	-0.062*		-0.079	-0.067	-0.084
		(-1.444)	(-1.441)	(-1.677)		(-0.964)	(-0.818)	(-0.905)
Market Cap			-0.001	0.064			0.137	0.215
			(-0.012)	(0.860)			(0.953)	(1.436)
Amihud Illiquidity				0.008				0.048
				(0.304)				(0.766)
Bid-Ask spread				0.036				0.065
				(1.030)				(0.608)
Number of Analysts				-0.051**				-0.054
				(-2.189)				(-1.181)
Surprise				0.020				-0.041
				(1.224)				(-1.484)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,454	8,454	8,454	8,454	2,850	2,850	2,850	2,850
R-squared	0.369	0.369	0.369	0.370	0.542	0.542	0.543	0.544

Table 2.4: Earnings announcements and trading speed around Reg SHO. This table reports results for the following diff-in-diff regression around Reg SHO:

$$y_{i,t} = \alpha_i + \delta_t + \beta(Pilot_i \times Program\ Period)_t + X'_{i,t}\gamma + \varepsilon_{i,t}\,,$$

where, $y_{i,t}$ is a variable measuring trading speed. We proxy trading speed with the ratio between the total dollar volume executed by active managers in a stock in the window [-10, -2] before the announcement to the total active managers' volume in that stock over the window [-10, 1]. In Panel A, we report results when trading speed is computed for buy trades. While sell trades are shown in Panel B. Pilot is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. Program Period is an indicator for the time in which the program took place (May 2005 – July 2007). The vector of control variables, $X^{'}_{i,t}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, previous year average bid-ask spread, and the earnings surprise. For each panel, we show estimates for positive (columns (1)-(4)) and negative (columns (5)-(8)) earnings surprises (news), separately. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. The sample is at the stock-event level and spans the period between May 2002 and July 2007. We consider the subsample of announcements in which active managers are present in the market. A manager is active if the adjusted R-squared of the regression of next month trading in a stock (as a % of total volume treaded next month) onto current stock holdings (as a percentage of portfolio holdings) ranks below the median of the across-managers R-squared distribution. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Panel A: buy trades								
Dependent variable	Trading speed							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	-0.019**	-0.019**	-0.019**	-0.020**	-0.014	-0.014	-0.012	-0.012
	(-2.256)	(-2.251)	(-2.258)	(-2.334)	(-0.893)	(-0.895)	(-0.757)	(-0.762)
Return Volatility		-0.001	-0.001	0.000		0.003	0.000	0.003
		(-0.258)	(-0.242)	(0.065)		(0.405)	(0.031)	(0.358)
Market Cap			-0.022**	-0.027**			-0.072***	-0.077***
			(-2.141)	(-2.505)			(-5.059)	(-5.064)
Amihud Illiquidity				0.008**				0.005
				(2.562)				(1.183)
Bid-Ask spread				-0.003				-0.007
				(-0.648)				(-0.820)
Number of Analysts				0.003				-0.000
				(0.818)				(-0.004)
Surprise				-0.006**				0.005
				(-2.061)				(0.846)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,240	23,240	23,240	23,240	9,398	9,398	9,398	9,398
R-squared	0.197	0.197	0.197	0.198	0.345	0.345	0.347	0.347

Panel B: sell trades

Dependent variable	Trading speed							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	0.009	0.009	0.009	0.008	-0.011	-0.010	-0.008	-0.009
	(0.954)	(0.970)	(0.958)	(0.898)	(-0.629)	(-0.621)	(-0.481)	(-0.552)
Return Volatility		-0.004	-0.003	-0.002		-0.008	-0.011	-0.014
		(-0.696)	(-0.645)	(-0.303)		(-0.847)	(-1.180)	(-1.422)
Market Cap			-0.049***	-0.057***			-0.074***	-0.064***
			(-4.499)	(-4.953)			(-4.194)	(-3.492)
Amihud Illiquidity				0.009***				0.015***
				(3.641)				(2.921)
Bid-Ask spread				-0.005				0.011
				(-0.983)				(1.122)
Number of Analysts				0.006*				0.006
				(1.656)				(0.954)
Surprise				-0.008**				-0.001
				(-2.551)				(-0.412)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,133	22,133	22,133	22,133	8,667	8,667	8,667	8,667
R-squared	0.199	0.199	0.200	0.201	0.359	0.359	0.361	0.362

Table 2.5: Earnings announcements and trade size around Reg SHO. This table reports results for the following diff-in-diff regression around Reg SHO:

$$y_{i,t} = \alpha_i + \delta_t + \beta(Pilot_i \times Program\ Period)_t + X'_{i,t}\gamma + \varepsilon_{i,t}\,,$$

where, $y_{i,t}$ is a variable measuring trade size. The dependent variable is the (log) size of a single execution measured using either the dollar or the share volume. This variable is averaged across all active investors for each stock in the window [-10, -2] before an earnings announcement. Pilot is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. Program Period is an indicator for the time in which the program took place (May 2005 – July 2007). The vector of control variables, $X'_{i,t}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, previous year average bid-ask spread, and the earnings surprise. For each panel, we show estimates for positive (columns (1)-(4)) and negative (columns (5)-(8)) earnings surprises (news), separately. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. The sample is at the stock-event level and spans the period between May 2002 and July 2007. We consider the subsample of announcements in which active managers are present in the market. A manager is active if the adjusted R-squared of the regression of next month trading in a stock (as a % of total volume treaded next month) onto current stock holdings (as a percentage of portfolio holdings) ranks below the median of the across-managers R-squared distribution. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Dependent variable	Trade size (buy trades)							
	Positive news				Negative news			
	Dollars		Number of shares		Dollars		Number of shares	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	-0.056**	-0.053**	-0.060**	-0.065**	0.005	-0.004	-0.028	-0.014
	(-2.245)	(-2.123)	(-1.967)	(-2.451)	(0.096)	(-0.076)	(-0.557)	(-0.278)
Total volume traded	1.125***	1.111***	0.989***	1.102***	1.200***	1.172***	1.041***	1.156***
	(78.530)	(75.713)	(66.185)	(74.092)	(59.427)	(55.646)	(48.948)	(54.421)
Return Volatility		-0.013		-0.005		0.006		-0.012
		(-0.790)		(-0.254)		(0.222)		(-0.419)
Market Cap		0.120***		-1.020***		0.249***		-0.884***
		(3.171)		(-25.414)		(4.344)		(-14.631)
Amihud Illiquidity		-0.031***		-0.028*		0.009		0.006
		(-3.116)		(-1.910)		(0.443)		(0.296)
Bid-Ask spread		0.052***		0.044*		0.086***		0.115***
		(3.570)		(1.757)		(3.291)		(4.086)
Number of Analysts		0.006		0.010		0.000		0.013
		(0.601)		(0.984)		(0.029)		(0.712)
Surprise		-0.012		-0.037***		-0.002		-0.016
		(-0.907)		(-3.259)		(-0.173)		(-1.353)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,240	23,240	23,240	23,240	9,398	9,398	9,398	9,398
R-squared	0.696	0.697	0.678	0.696	0.741	0.742	0.759	0.773

Table 2.6: Earnings announcements and broker splitting around Reg SHO. This table reports results for the following diff-in-diff regression around Reg SHO:

$$y_{i,t} = \alpha_i + \delta_t + \beta(Pilot_i \times Program\ Period)_t + X'_{i,t}\gamma + \varepsilon_{i,t}\,,$$

where for $y_{i,t}$ we use different proxies for how ANcerno managers trade with their brokers. In Panel A, the dependent variable is a dummy equal to one if the average number of brokers used by managers to trade a stock in the window [-10, -2] before an earnings announcement is above the median value of the sample distribution. In Panel B, we use is an indicator for the high centrality of brokers used by managers to trade a stock in the window [-10, -2] before an earnings announcement. We compute this measure as the (volume-weighted) eigenvector centrality in the six months before the earnings announcement. Then, from this raw measure, we compute the percentiles of the distribution across brokers. We finally compute the volume-weighted percentile at the stock-announcement level and define the dummy equal to 1 if the volume-weighted percentile is greater or equal to 75. In Panel C, we use a proxy for the familiarity of brokers that traders use to place their orders. For each Ancerno manager, we compute the share of dollar volume traded with each broker in the last year; then we adjust this value by taking into account the average number of brokers used by the trader in the same period. Finally, we compute the volume-weighted average of our broker/trader familiarity proxy at the stock level for the trades occurring in the pre-announcement window (days [-10, -2]). Pilot is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. Program Period is an indicator for the time in which the program took place (May 2005 – July 2007). The vector of control variables, $X'_{i,t}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, previous year average bid-ask spread, and the earnings surprise. For each panel, we show estimates for positive (columns (1)-(4)) and negative (columns (5)-(8)) earnings surprises (news), separately. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. The sample is at the stock-event level and spans the period between May 2002 and July 2007. We consider the subsample of announcements in which active managers are present in the market. A manager is active if the adjusted R-squared of the regression of next month trading in a stock (as a % of total volume treaded next month) onto current stock holdings (as a percentage of portfolio holdings) ranks below the median of the across-managers R-squared distribution. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Panel A	Dependent variable: High number of brokers							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	0.040***	0.039***	0.040***	0.039***	0.016	0.016	0.010	0.010
	(2.883)	(2.833)	(2.916)	(2.851)	(0.623)	(0.628)	(0.422)	(0.414)
Return Volatility		0.023***	0.022***	0.026***		-0.008	-0.000	-0.002
		(2.734)	(2.745)	(3.134)		(-0.618)	(-0.026)	(-0.129)
Market Cap			0.244***	0.240***			0.180***	0.177***
			(15.640)	(14.623)			(8.493)	(7.853)
Amihud Illiquidity				0.011**				-0.004
				(2.483)				(-0.663)
Bid-Ask spread				-0.015*				0.006
				(-1.781)				(0.380)
Number of Analysts				0.010**				0.003
				(2.076)				(0.324)
Surprise				0.003				0.005
				(0.766)				(0.722)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,240	23,240	23,240	23,240	9,398	9,398	9,398	9,398
R-squared	0.419	0.419	0.427	0.428	0.506	0.506	0.511	0.511

Panel B	Dependent variable: Broker centrality							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	-0.018***	-0.018***	-0.018***	-0.018***	-0.009	-0.009	-0.008	-0.008
	(-2.738)	(-2.675)	(-2.672)	(-2.678)	(-0.711)	(-0.691)	(-0.666)	(-0.620)
Return Volatility		-0.013***	-0.013***	-0.008*		-0.016**	-0.017**	-0.013*
		(-3.134)	(-3.108)	(-1.960)		(-2.226)	(-2.264)	(-1.681)
Market Cap			-0.024***	-0.031***			-0.010	-0.010
			(-2.877)	(-3.516)			(-0.738)	(-0.691)
Amihud Illiquidity				0.002				0.001
				(0.454)				(0.186)
Bid-Ask spread				-0.017***				-0.014*
				(-4.177)				(-1.799)
Number of Analysts				-0.001				-0.001
				(-0.501)				(-0.161)
Surprise				0.002				-0.006
				(0.853)				(-1.039)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,240	23,240	23,240	23,240	9,398	9,398	9,398	9,398
R-squared	0.193	0.194	0.194	0.195	0.334	0.334	0.334	0.335

Panel C	Dependent variable: Broker familiarity							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	-0.442***	-0.445***	-0.446***	-0.433***	-0.115	-0.113	-0.086	-0.057
	(-2.814)	(-2.833)	(-2.871)	(-2.798)	(-0.402)	(-0.398)	(-0.303)	(-0.201)
Return Volatility		0.084	0.087	0.101		-0.077	-0.116	0.016
		(0.886)	(0.940)	(1.058)		(-0.472)	(-0.717)	(0.095)
Market Cap			-1.013***	-1.092***			-0.922***	-1.033***
			(-4.971)	(-5.286)			(-3.401)	(-3.646)
Amihud Illiquidity				-0.160*				-0.150**
				(-1.934)				(-2.167)
Bid-Ask spread				-0.087				-0.434**
				(-1.116)				(-2.494)
Number of Analysts				-0.033				-0.129
				(-0.688)				(-1.405)
Surprise				0.027				-0.069
				(0.772)				(-0.995)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,240	23,240	23,240	23,240	9,398	9,398	9,398	9,398
R-squared	0.337	0.337	0.338	0.338	0.452	0.452	0.453	0.454

Table 2.7: Earnings announcements and trading volume around Reg SHO. This table reports results for the following diff-in-diff regression around Reg SHO:

$$y_{i,t} = \alpha_i + \delta_t + \beta(Pilot_i \times Program\ Period)_t + X'_{i,t}\gamma + \varepsilon_{i,t}\,,$$

where, $y_{i,t}$ is the total ANcerno active managers (log) dollar volume executed in a stock in the window [-10, 1] around an earnings announcement. Pilot is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. Program Period is an indicator for the time in which the program took place (May 2005 – July 2007). The vector of control variables, $X'_{i,t}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, previous year average bid-ask spread, and the earnings surprise. For each panel, we show estimates for positive (columns (1)-(4)) and negative (columns (5)-(8)) earnings surprises (news), separately. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. The sample is at the stock-event level and spans the period between May 2002 and July 2007. We consider the subsample of announcements in which active managers are present in the market. A manager is active if the adjusted R-squared of the regression of next month trading in a stock (as a % of total volume treaded next month) onto current stock holdings (as a percentage of portfolio holdings) ranks below the median of the across-managers R-squared distribution. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Dependent variable	Log-dollar volume							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	0.069**	0.066**	0.068**	0.066**	0.084*	0.085*	0.053	0.049
	(2.127)	(2.064)	(2.546)	(2.477)	(1.734)	(1.740)	(1.230)	(1.142)
Return Volatility		0.079***	0.074***	0.054***		-0.018	0.027	0.010
		(4.182)	(4.856)	(3.482)		(-0.588)	(1.109)	(0.376)
Market Cap			1.183***	1.239***			1.082***	1.108***
			(37.743)	(37.081)			(25.994)	(25.295)
Amihud Illiquidity				0.021***				0.020
				(3.316)				(1.485)
Bid-Ask spread				0.072***				0.042*
				(4.810)				(1.687)
Number of Analysts				0.031***				0.055***
				(3.517)				(3.504)
Surprise				0.016**				-0.026***
				(2.464)				(-2.602)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,240	23,240	23,240	23,240	9,398	9,398	9,398	9,398
R-squared	0.738	0.738	0.763	0.764	0.757	0.757	0.782	0.783

Table 2.8: Hedge funds' trades around Reg SHO. This table reports results for the following diff-in-diff regression around Reg SHO:

$$y_{m,i,t} = \psi_m + \alpha_i + \delta_t + \beta(Pilot_i \times Program\ Period)_t + X'_{i,t}\gamma + \varepsilon_{m,i,t}\,,$$

where, $y_{i,t}$ is a variable measuring trading speed. We define trading speed as the ratio between the dollar volume executed by manager m in stock i in the window [-10, -2] and the total manager-stock dollar volume in the window [-10, 1] around time-t earnings announcement. Pilot is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. Program Period is an indicator for the time in which the program took place (May 2005 – July 2007). $\psi_m, \alpha_i, \delta_t$ represent manager, stock, and time fixed effects, respectively. The vector of control variables, $X'_{i,t}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, previous year average bid-ask spread, and the earnings surprise. For each panel, we show estimates for positive (columns (1)-(4)) and negative (columns (5)-(8)) earnings surprises (news), separately. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. We consider the subsample of informed trades by hedge funds before positive news if the trade is a buy, or negative news if it is a sell. In particular, we define informed buy (sell) trades by selecting those transactions for which the total imbalance for the hedge fund manager in the stock over the window [-10, -2] is positive (negative) and only if less than 50% of the stocks in the portfolio of a hedge fund are exchanged. The sample is at the manager-stock-event level and spans the period between May 2002 and July 2007. Standard errors are clustered at the manager and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Dependent variable	Trading speed							
	Buy trades on positive news				Sell trades on negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	-0.020**	-0.021**	-0.023**	-0.024**	0.045**	0.045**	0.043**	0.041**
	(-2.191)	(-2.206)	(-2.426)	(-2.548)	(2.317)	(2.278)	(2.196)	(2.134)
Return Volatility		0.007*	0.010***	0.009**		0.007	0.007	0.009
		(1.824)	(2.726)	(2.572)		(0.389)	(0.416)	(0.535)
Market Cap			-0.075***	-0.073***			-0.090**	-0.100**
			(-5.420)	(-5.271)			(-2.045)	(-2.215)
Amihud Illiquidity				0.004**				0.001
				(2.036)				(0.219)
Bid-Ask spread				0.008***				-0.003
				(2.748)				(-0.187)
Number of Analysts				0.003				0.004
				(1.002)				(0.357)
Surprise				-0.004				0.008**
				(-1.652)				(2.669)
Manager FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,323	13,323	13,323	13,323	3,746	3,746	3,746	3,746
R-squared	0.256	0.256	0.258	0.258	0.422	0.422	0.423	0.424

2.8 Figures

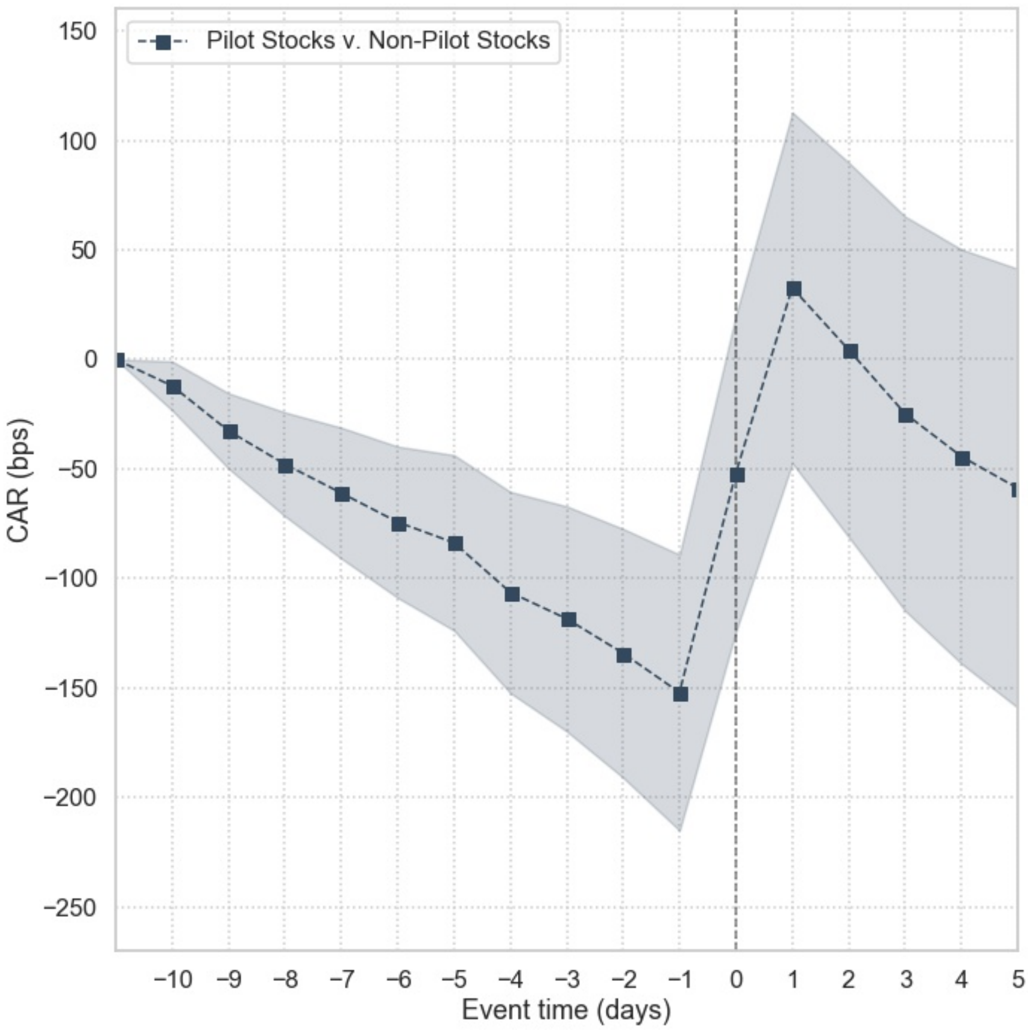


Figure 2.1: Positive earnings announcements and cumulative abnormal returns during the Reg SHO Pilot program. We graph the cumulative coefficients for the following regression:

$$AR_{i,t}^{t+s} = \alpha_i + \theta_t + \sum_{s=-10}^{5} \beta_{t+s}^{Pilot} \times Pilot_i \times Program\ Period_t^{t+s} + \sum_{j=1}^{N} \gamma_j X_{i,t}^{j} + \varepsilon_{i,t}\,,$$

where $AR_{i,t}^{t+s}$ is the stock i DGTW-adjusted return on day t+s around time-t for positive earnings announcement, with $s \in [-10, 5]$. α_i, θ_t are stock and time fixed effects, respectively; Pilot is a dummy equal to 1 if the stock is included in the Reg SHO Pilot Program, $Program\ Period_t^{t+s}$ is a dummy equal to one if day t+s around an earnings announcement scheduled on day t falls within the Reg SHO Program Period (May 2005-July 2007). Finally, $X_{i,t}^{j}$ represents control variables (market cap, Amihud illiquidity, stock volatility, bid-ask spread, number of analysts following the company). The graph reports cumulative $\hat{\beta}_{t+s}^{Pilot}$, and 95% confidence intervals for standard errors double clustered at the stock and day level (shaded area). Our sample spans the period between May 2002 and July 2007.

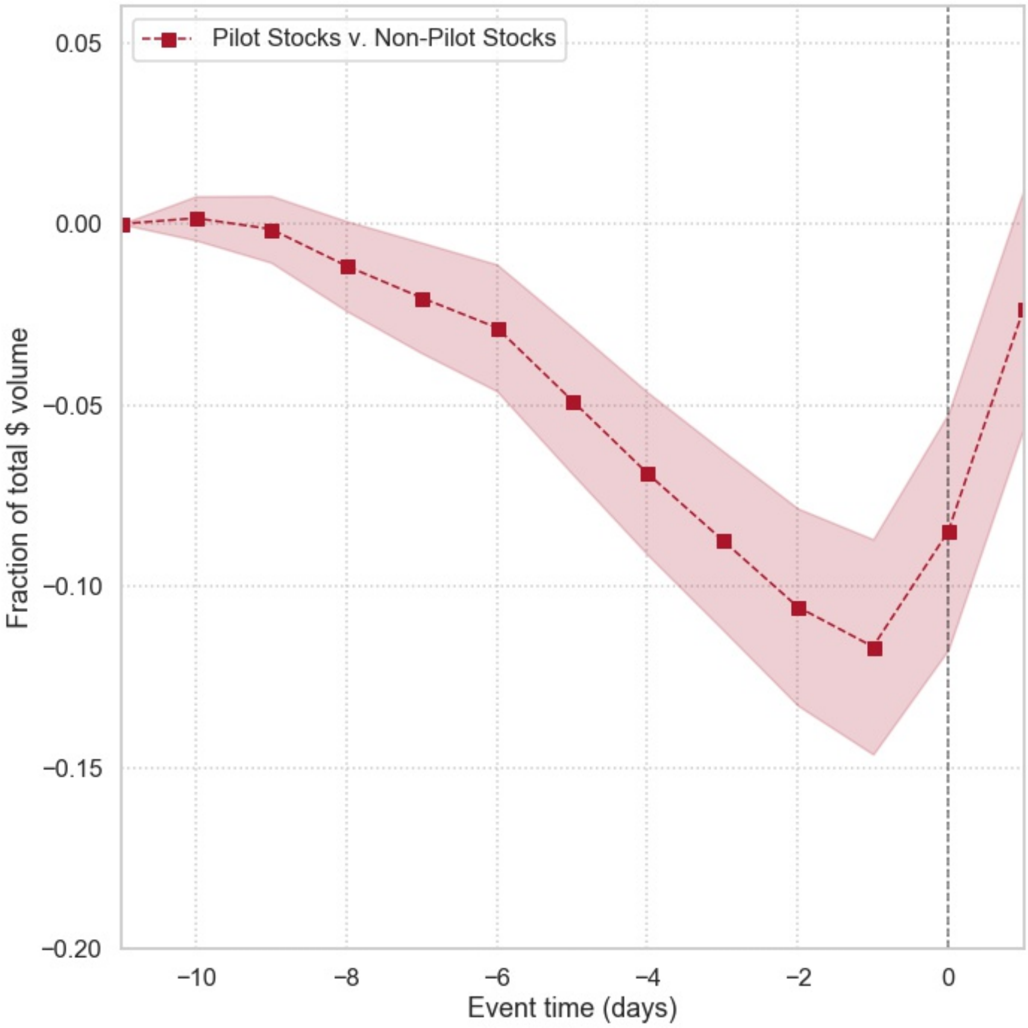


Figure 2.2: Earnings announcements and cumulative volume during Reg SHO Pilot program. We graph the cumulative coefficients for the following regression:

$$Trading\ Speed_{i,t}^{t+s} = \alpha_i + \theta_t + \sum_{s=-10}^{1} \beta_{t+s}^{Pilot} \times Pilot_i \times Program\ Period_t^{t+s} + \sum_{j=1}^{N} \gamma_j X_{i,t}^j + \varepsilon_{i,t},$$

where $Trading\ Speed_{i,t}^{t+s}$ is the stock i ratio of daily dollar volume on the buy-side to total event volume on the buy side computed on day t+s around time-t for positive earnings announcement, with $s \in [-10, 1]$. We compute the total event volume in the window [-10, 1]. The aggregate stock level volume considers only ANcerno traders that we define active. A manager is active if the adjusted R-squared of the regression of next month trading in a stock (as a % of total volume treaded next month) onto current stock holdings (as a percentage of portfolio holdings) ranks below the median of the across-managers R-squared distribution. α_i, θ_t are stock and time fixed effects, respectively; Pilot is a dummy equal to 1 if the stock is included in the Reg SHO Pilot Program, $Program\ Period_t^{t+s}$ is a dummy equal to one if day t+s around an earnings announcement scheduled on day t falls within the Reg SHO Program Period (May 2005-July 2007). Finally, $X_{i,t}^j$ represents control variables (market cap, Amihud illiquidity, stock volatility, bid-ask spread, number of analysts following the company). The graph reports cumulative $\hat{\beta}_{t+s}^{Pilot}$, and 95% confidence intervals for standard errors double clustered at the stock and day level (shaded area). Our sample spans the period between May 2002 and July 2007. Our sample spans the period between May 2002 and July 2007.

Chapter 3

Institutional Investors and the Announcement of Share Repurchases

3.1 Introduction

Over the last decades share repurchases have became a more common way for firms to distribute cash to shareholders (Fama and French, 2001; Grullon and Michaely, 2002, 2004). One often overlooked aspect of share repurchases regards the identity of the investors who are the counterparties of the firms buying back their own stocks. Our paper studies the trading of institutional investors around share repurchases to better understand the impact of share repurchases on the ownership of corporate equities.

In the frictionless environment of Miller and Modigliani (1961), investors are indifferent whether a firm distributes cash through dividends or share repurchases. However, the distribution method can have a differential impact on firm valuations in an environment with frictions. Investors may have different tax preferences toward the means of payout (Allen, Bernardo, and Welch, 2000). Whereas all taxable investors pay taxes on dividend payments, only taxable investors with embedded gains will pay taxes on the capital gains when they liquidate their positions while firms repurchase their shares. On the other hand, investors with embedded losses will realize a capital loss when they sell their securities, which can reduce their overall tax burden. Furthermore, share repurchases could provide a signal of the valuation of firms, as firms repurchase their shares after a period of poor stock performance (Vermaelen, 1981; Ikenberry, Lakonishok, and Vermaelen, 1995). The identity of the counterparty is important as the previous literature has documented that the stocks of repurchasing firms tend to outperform the market over the long-term (Ikenberry, Lakonishok, and Vermaelen, 1995, 2000). Thus, the investors who sell their securities to the repurchasing firms may forgo some profitable investment opportunities. Furthermore, the tax status and the capital gains overhang of the liquidating investors influences the tax burden by investors. Whereas pension funds are typically exempt from dividend and capital gains taxes, a significant portion of mutual fund investors are taxable (Sialm and Starks, 2012; Sialm and Zhang, 2020).

This paper studies share buybacks by focusing on 2,117 open market repurchase programs of US firms during the period from 2000 to 2014. For each repurchasing (treated) firm, we select a control firm by matching on industry, size, and past returns. In all tests we contrast the trading behavior of mutual funds and pension funds in the treated firms to the trades in the control firms on the same days.

First, we set the stage by confirming existing findings that firms actually commit to their share repurchase announcements by buying back most of the announced amount (Stephens and Weisbach, 1998; Chemmanur, Li, Xie, and Zhu, 2018). We add to the literature by showing that firms repurchase their shares with the highest intensity during the first month after the repurchase and subsequently continue to gradually repurchase their shares over the subsequent quarters. Moreover, consistent with Vermaelen (1981) and Ikenberry, Lakonishok, and Vermaelen (1995), we report cumulative market-adjusted returns of around 2% in the 10-day window starting immediately prior to the public announcement.

Second, we test whether different institutional investors display different propensity to sell when a firm announces a share buyback. We find that mutual funds are significantly more likely to liquidate their positions. In particular, our analysis shows that, in the aggregate, mutual funds sell about 1% of a firm's share outstanding in the year after the announcement. This figure is substantially smaller for pension funds.

Selling upon the announcement of a share repurchase program might not be optimal, given the positive long-run performance of repurchasing stocks in the subsequent period. However, investors can avoid this inefficiency by delaying the trades until after the stock prices have appreciated. Consistent with this view, we find that, in the week after the announcement when the stock price increases the most, mutual funds have a lower propensity of selling the announcing firm compared to similar non-announcing firms. We don't find such behavior for pension funds.

Our paper also sheds some light on the tax burden of liquidating investors. Investors in firms that repurchase their shares tend to liquidate the positions with relatively low embedded capital gains, reducing the tax burden of taxable investors. Around 40% of the liquidated positions are liquidated at capital losses. This proportion increases to around 50% when we condition on positions purchased within the last year, which are taxed at the higher short-term capital gains tax rate. Thus, the liquidation behavior reduces the tax burden of taxable investors in contrast to the uniform taxation of dividend income.

The paper proceeds as follows. Section 3.2 discusses the relevant literature on share repurchases. Section 3.3 describes the data used in the analysis. Section 3.4

studies the timing of share repurchases and the market reaction to their announcement. Section 3.5 analyzes the trading of institutional investors around the announcement of share repurchases. Section 3.6 focuses on the role played by embedded gains in the decision to trade during a share repurchase program. Section 3.7 concludes.

3.2 Related Literature

The vast literature on payout policies, and share repurchases in particular, is summarized by three comprehensive reviews (Allen and Michaely, 2003; DeAngelo, DeAngelo, and Skinner, 2008; Farre-Mensa, Michaely, and Schmalz, 2014), to which we refer for a complete treatment of the topic. In what follows, we limit the discussion to papers that are relevant to our work.

Early papers study the motives for repurchases. There are two main reasons why firms buy back their own securities. Firms repurchase stocks to signal positive information (Vermaelen, 1981, 1984) and to distribute free cash flows to shareholders (Jensen, 1986). Stephens and Weisbach (1998) reconcile both motives by confirming the findings in Vermaelen (1981) that firms repurchase after a period of undervaluation and by showing a positive relation between repurchases and the level of cash flows. Dittmar (2000) empirically re-evaluates several motives for repurchase and finds evidence consistent with firms repurchasing to take advantage of potential undervaluation and distribute excess capital. Dittmar (2000) also shows that share buybacks are a way to bring leverage back to its optimal level (Hovakimian, Opler, and Titman, 2001). Moreover, Dittmar finds that in periods when the takeover market is active, or the use of management stock options increases, share repurchases become a means to avoid takeover attempts and decrease the dilution effects of options. More recently, Massa, Rehman, and Vermaelen (2007) suggest that in concentrated industries, firms buy back shares to strategically mimic competitors, in order to avoid being perceived as "lemons". Finally, Brav, Graham, Harvey, and Michaely (2005) survey financial executives and find that repurchases are preferred because they are more flexible than dividends and can be used to time the stock market or to increase earnings per share.[1] This body of work informs our analysis, because institutional investors might take these motives into account when deciding whether to enter the market after the announcement of a share repurchase.

Another stream of literature looks at the interplay between shareholders and executives for the decision to distribute cash to shareholders. In a world without frictions, dividends and share repurchases are perfect substitutes and do not influence the value of the firm, as stated by Miller and Modigliani (1961). Brennan and Thakor (1990) theoretically show that, when information acquisition is costly, shareholders

[1] Similar results are found by Jagannathan, Stephens, and Weisbach (2000) in an empirical paper. Ma (2019) shows that firms time the market by acting as cross-market arbitrageurs in their own securities. When credit markets are cheap, firms not only issue additional debt, but also repurchase more equity, and vice-versa.

are no longer indifferent between dividends and share repurchases. In particular, share repurchases imply a redistribution of wealth from uninformed to informed investors. Hence, as long as the dividend tax is not too high, small distributions come in the form of dividends, while share repurchases are more likely for larger distributions. The choice will always be dependent on the distribution of shareholders. However, different investors have different tax treatments. The theory of Allen, Bernardo, and Welch (2000) suggests that dividend-paying firms attract more institutional investors, provided that they are less taxed than individual investors. This was confirmed empirically by Grinstein and Michaely (2005), who show that while institutions avoid firms that do not pay dividends, they prefer low dividends and share repurchases. In general, however, a concentration of institutional holdings does not seem to increase a firm's cash distributions. Additional and more conclusive evidence that tax preferences drive the payout policy can be found in Desai and Jin (2011).[2] Gaspar, Massa, Matos, Patgiri, and Rehman (2013) look at investors' horizons and find that the frequency of share repurchases increases when the shareholder base is dominated by short-term investors. Consistent with a tax preference story, Sikes (2017) finds that, since tax-sensitive investors are reluctant to sell stocks in which they have unrealized capital gains, firms with greater capital gains lock-in spend significantly more on capital expenditures and research and development than repurchases. We contribute to this literature by showing that while mutual funds are more likely to sell after a share repurchase announcement when compared to pension funds, they refrain to do so when capital gains are higher, provided their different tax treatment.

The focus of our paper is on open market share repurchases. The literature has shown that the announcement of these distributions is followed by long-lasting positive abnormal returns (Ikenberry, Lakonishok, and Vermaelen, 1995, 2000). These findings have been challenged because they might be sample-specific or suffer the joint-hypothesis problem (Fama, 1998), or be driven by the cross-correlation of a firm's abnormal returns (Mitchell and Stafford, 2000). Peyer and Vermaelen (2009) propose an updated study that takes into account the criticism and confirms the findings of Ikenberry, Lakonishok, and Vermaelen (1995). A recent paper by Manconi, Peyer, and Vermaelen (2018) finds that share repurchases are followed by significant positive short- and long-term excess returns in 31 non-U.S. countries.[3]

The growing importance of share repurchases led researchers to study the real effects of this type of distribution. In particular, Almeida, Fos, and Kronlund (2016) show that firm managers are willing to trade off investments and employment for stock repurchases that allow them to meet analyst earnings forecasts.

[2] More recently, Crane, Michenaud, and Weston (2016) show that variation in institutional ownership driven by Russell index weights helps explain higher dividend payments. In this context, bigger distributions are a compensation for better monitoring by institutions.

[3] Using a sample of firms from nine European countries, Anolick, Batten, Kinateder, and Wagner (2021) show that abnormal returns around share repurchases are tightly linked to market uncertainty, with a stronger signalling effect when uncertainty increases.

We close this review by discussing recent papers that are closely related to our work. Henry and Koski (2017) use ANcerno data to show that 15% of the overall abnormal returns of skilled investors is obtained using dividend capture strategies. Chemmanur, Li, Xie, and Zhu (2018) use trade-level data to study the institutional trading before and immediately after the announcement of open market share repurchases. They find that pre-event volume has predictive power for the post-announcement return. Moreover, in contrast to our work, the authors focus on buy trades and show that institutional investors' post-event purchases predict actual repurchases and both firms and investors performance around the event. Huang and Zhang (2017) report that institutions appear not concerned with the long-run positive price drift that follows share repurchase announcements and sell in the days and quarters immediately after the announcement. They find evidence consistent with the hypothesis that firms buy-out investors with non-aligned beliefs. Differently from both Huang and Zhang (2017) and Chemmanur, Li, Xie, and Zhu (2018), Jain, Mishra, and Nguyen (2020) show that the performance of institutional investors around share repurchases is weak due to the fact that these type of corporate events cannot be really anticipated, and hence institutions are unable to overcome the informational advantage of firms. We add to these studies by showing that the sell-off in the quarter after the announcement comes mostly from mutual funds that enter the market with a long position before the price starts moving upwards.

3.3 Data

3.3.1 Share Repurchase Announcement

We start with the CRSP-Compustat annual file and select firms with non-negative and non-missing total assets, that trade ordinary shares (CRSP share code of 10 or 11), and that are traded on NYSE, Nasdaq or Amex (CRSP exchange codes 1, 2, 3, 31, 32, 33). Next, using the Fama-French NYSE size breakpoints, available on Kenneth French's website, we exclude firm-year observations that fall in the first size decile.

We retrieve share repurchase announcements from Thomson One for the period spanning April 1999 to September 2014.[4] We consider open market share repurchases only with available information on the size of the program. We start with 6,897 announcements. We then match with the CRSP-Compustat database first by the 6-digit cusip code and, then, by name. When we match with the firm sample described above, we end up with 3,649 events. Finally, we require that the firm has non-missing buy-and-hold market-adjusted returns in the quarter and year before the share repurchase announcement. We compute previous quarter (year) market-adjusted return over the window [-63, -1] ([-252, -1]) and retain announcements with

[4]The time-period of the analysis is constrained by the availability of ANcerno data, which begins in January 1999 and ends in December 2014. By choosing share repurchase announcements from April 1999 to September 2014, we allow for a pre- and post- event window.

at least 31 (126) observations in this window. We use the CRSP value-weighted index as market benchmark.

Next, we select a candidate control firm, using the criteria described in Table 3.2. For each event, we select all firms that are in the same Fama-French 12 industry classification as the announcing firm. We exclude those firms that have a share repurchase announcement in the prior, current, and subsequent quarters. We then select as control firms those that have a market capitalization at the beginning of the event quarter between half and two times the market capitalization of the treated firm, and require that the absolute difference in the market-adjusted returns during the previous quarter and previous year are smaller than 2.5% and 10%, respectively. If there are multiple candidates, we keep the one for which the market capitalization is closest to that of the announcing firm. The matching rules are selected in an effort to balance the need of selecting a similar firm, while keeping a representative sample.

The final sample consists of 2,117 share repurchase events. Table 3.1 describes the sample. In Panel A, we report statistics for the full sample, while Panel B shows the sub-sample 2004-2014, when actual share repurchases are available in Compustat.[5] On average, the sample firm announces roughly 10.4% of market capitalization in repurchased securities, and in the first year after the announcement it buys back around 87% of the announced amount. Consistent with the share repurchase literature (e.g. Ikenberry, Lakonishok, and Vermaelen, 1995; Grullon and Michaely, 2002; Chemmanur, Li, Xie, and Zhu, 2018), we estimate a sizeable announcement price effect of 1.8% on days [0, 2] around the announcement, which follows a period of low returns in the quarter and year before the event. Notably, the firm characteristics do not seem to change in the most recent sub-sample (Panel B). Moreover, the t-test for the difference in means, reported in Table 3.3, shows that the treatment and the control firms do not differ along most dimensions. The only exception is the probability of paying dividends, but the economic magnitude is quite small.

3.3.2 Institutional Trades

We draw institutional trades from Abel Noser Solutions, formerly known as Ancerno Ltd. (we retain the name of "ANcerno", commonly used in the literature; see Hu, Jo, Wang, and Xie (2018b), for a detailed description of this data set). ANcerno provides consulting services for transaction cost analyses to institutional investors and made these data available for academic research. The previous literature has shown that the characteristics of stocks traded and held by ANcerno institutions

[5]Share repurchases are regulated by the Exchange Act of 1939. Rule 10b-18 of the Exchange Act outlines specific requirements for repurchases to receive safe harbor protection from price manipulation claims. The safe harbor under Rule 10b-18 was amended in November 2003 (effective December 2003). Firms must now disclose quarterly the number of shares repurchased, the average repurchase price, and whether the repurchase was part of a publicly announced open market repurchase program. See Banyi, Dyl, and Kahle (2008) for a complete discussion of the consequences of this change in rule.

and the return performance of the trades are comparable to those in 13F mandatory filings Puckett and Yan (2011) and Anand, Irvine, Puckett, and Venkataraman (2013b). This mitigates concerns related to a possible self-reporting bias of the data, provided that, while some ANcerno clients submit information to obtain objective evaluations of their trading costs, other institutions voluntarily report to ANcerno. The information provided in ANcerno regards the details of each single trade execution. In particular, we have access to the transaction date; the execution price; the number of shares that are traded; the side (buy or sell); the broker who intermediates the trade; and the management company originating the trade.

We match ANcerno to our share repurchases sample using *permno* as the stock identifier. Moreover, for each announcement we select the managers that trade any stock, at least once, in the month in which the event takes place. We set the volume traded to zero in those trading days that a manager does not send any orders to ANcerno.

A key information for our analysis is whether the manager is a mutual fund or a pension fund. ANcerno provides a variable, *clienttype*, that helps us distinguish between mutual funds ($clienttype = 2$) and pension funds ($clienttype = 1$).[6]

In our analysis, we consider 973 managers, of which 612 are pension funds and 361 mutual funds. Panel C of Table 3.1 shows the average number of traders per event. On average, there are 395 manager active in each event. The number of institutions across events is quite stable, as the minimum is 323 and the maximum 454. The average number of mutual funds is 138, while there are 257 pension funds.

3.4 Timing of Actual Repurchases and Price Reactions

In this section we study the timing of repurchases by firms during the quarters following the announcement and the stock price reaction to the repurchase announcement. This analysis will inform the subsequent tests on institutional trading.

3.4.1 Repurching Time

Stephens and Weisbach (1998) and Chemmanur, Li, Xie, and Zhu (2018) show that firms complete a significant portion of the repurchase programs within the one-year period after the announcement. However these studies do not provide higher frequency evidence of the timing of share repurchases. Understanding this patterns is important in order to make sense of the daily institutional trading that we study in the next section.

We start by analyzing the probability that a firm buys back shares in the quarters surrounding the announcement. To do so we consider actual share repurchases available in Compustat starting in 2004.

[6]As common in the ANcerno literature (Hu, Jo, Wang, and Xie, 2018b), we discard trades executed brokers ($clienttype = 3$), which represent a residual category.

Panel A of Figure 3.1 displays the coefficients of a linear probability model for actual share repurchases, where the main explanatory variables are a set of indicators for the quarters surrounding the announcement. Actual share repurchases are available at the end of the quarter; hence, we call quarter zero the one in which the firm announces the buyback, regardless of where in the quarter the event takes place. The figure shows a sharp discontinuity in the probability of repurchasing shares that coincides with quarter zero. The probability of buybacks remains high in the first quarter after the announcement and starts decreasing during the second quarter.

Confirming this result, Panel B shows the percentage of the program size that is repurchased during quarters [-4, 4]. More than 25% of the announced amount is repurchased in the first quarter, while the repurchasing activity decreases in the later quarters. We also see some repurchasing activity in the quarter prior to the announcement.

In order to provide a higher frequency estimate of the repurchasing activity using quarterly data, we take advantage of the timing of the repurchase announcement within a quarter. If an announcement occurs at the beginning of a quarter, then the repurchase amount during the announcement covers the repurchasing activity over most of the quarter after the announcement. On the other hand, if the announcement occurs at the end of the quarter, then the repurchase amount will primarily reflect repurchases immediately after the announcement. In Panels C and D of Figure 3.1 we zoom in on the first weeks and months after the announcement. Panel C plots the estimated average percentage repurchased during the three months after the announcement. In particular, we ascribe to month one quarterly repurchases that happen during the last four weeks of a quarter.[7] This ensures that the quarterly figure that we observe is mainly related to actual repurchases during the first month after the announcement. Since Panels A and B suggest that there is some repurchasing activity also in the period before the announcement, we take this into account by subtracting (13-i)/13 of the repurchases in the quarter prior to the announcement to the observed figure for the *i*th week of the quarter. In other words, we assume that what happens in the calendar quarter before the announcement is similar to what happens in the weeks that precede the announcement. We adopt a similar approach for the second and third months in a quarter. We use the middle five and first four weeks for repurchases made during the second and third month after the announcement, respectively. Again, we subtract (13-i)/13 of the repurchases in the quarter prior to the announcement to the observed figure for the *i*th week of the quarter.

We find that the cumulative estimated repurchasing activity is increasing during the first quarter after the announcement. Using a similar procedure to zoom in on the first four weeks after the announcement (Panel D), suggests that less than 5% of the program size is repurchased during the first week. Then, the repurchasing schedule is steadily increased to reach 15% at the end of the first month. Thus, we estimate

[7] For simplicity, we assume 13 weeks in a calendar quarter.

that while repurchases are especially pronounced during the first three weeks after an announcement, they steadily increase in the first quarter after the announcement.

3.4.2 Price Reaction

The share repurchases literature finds that firms' buybacks usually take place after a period of poor stock performance and that the announcements of share repurchases are usually followed by positive returns (Ikenberry, Lakonishok, and Vermaelen, 1995, 2000). This evidence is key in our setting as, taken at face value, it would imply that investors who take the other side of the transaction in a share repurchases - those who sell - would, most likely, forgo the positive return they would have achieved if they kept the long position.

We provide evidence that the stock price response to an announcement of share repurchases by plotting market-adjusted[8] cumulative abnormal returns during the window [-10, 10] around the event. Figure 3.2 shows the results. We confirm that the share repurchase announcement is preceded by a drop in the stock price and is followed by a sharp positive drift. During the 10 days before the announcement, we estimate a cumulative abnormal return of less than -1% and a positive jump of about 2% when the program is made public. The announcement effect is spread over two days (i.e., day 0 and day 1), as some of the announcements are made before the close of the market on day 0 and others are made after the close of the market on day 0. The stock price then keeps increasing steadily during the 10 days after announcement. Ikenberry, Lakonishok, and Vermaelen (1995, 2000) also show that the overperformance persists in the years after the announcement.

3.5 Institutional Trading in Repurchasing Firms

In this section we analyze trading patters of mutual funds and pension funds around share repurchase announcements. We present results both at the firm and manger-firm level.

3.5.1 Evolution of Stock-Level Volumes

We start by examining the aggregate volume in firms that announce a share repurchase (treated firms) and their match (control firms). For each announcing firm and match, we calculate the total signed share volume traded by all managers in Ancerno as a percentage of total shares outstanding in each month during the window [-6, 12] around the announcement. Month zero represents the 20-trading-day period starting with the announcement day. Similarly, we aggregate mutual fund and pension fund volumes for each event and analyze the trading patterns of these investors. We estimate average share volume using a regression of firm-month volume onto event-month dummies and then cumulating the coefficients. We take into account

[8] We use the CRSP value-weighted index as benchmark.

time invariant firm characteristics and time-specific confounding effects by including firm and time fixed effects in the regressions. We cluster standard errors at the firm and time level.

Panel A of Figure 3.3 displays cumulative signed volume as a percentage of shares outstanding for all managers trading in the treated and control firms. The figure shows that the trading pattern in treated firms is indistinguishable from that in control firms during the pre-event period. However, imbalances in treated firms become progressively more negative as we move away from month zero. We note that both for the announcing and matched firms volume shows a downward trend. This is a byproduct of the fact that treated and control firms both experience negative returns prior to the announcement month, as suggested by Table 3.3. However, the negative trend is more pronounced for firms that announce a share repurchase, suggesting that institutional investors take the other side of the transaction when firms repurchase their shares.

We next move on to study whether different institutional investors behave differently upon the announcement of a share repurchase program. Panels B and C of Figure 3.3 show the cumulative volume for pension funds and mutual funds, respectively. While pension funds sell after the announcement of share repurchases, their behavior on announcing firms are not significantly different than that of control firms after the announcement. On the other hand, mutual funds display an abnormal selling behavior after the announcement of a share repurchase program.

3.5.2 Probability of Selling after a Repurchase Announcement

We first test for the probability of selling in the period immediately after a share repurchase announcement. In particular, using a sample at the manager-stock-day level, we keep observations during the window [0, 60] after the repurchase announcement for trades of both the treated and control firms and test whether the probability of selling is higher for treated firms and how this probability changes as we move away from the event day. We define a manager-stock-day observation as a sell (buy) trade if the end-of-day net share volume is negative (positive). In order to take into account no trades, we consider trades by all managers that appear in Ancerno in the month when the firm announces the share repurchase, regardless of whether the manager trades the announcing firm. Table 3.4 shows the results.

In Panel A, we run a linear probability model where the outcome variable is the difference between the probabilities of selling and buying. We show results for the entire sample in Columns (1)-(3), for the subsample of mutual fund in Columns (4)-(6), and for pension funds in Columns (7)-(9). The main explanatory variable is either a dummy for treated firms (Columns (1)-(4)-(7)), the interaction between *Treated* and *Announcement*, an indicator for days $[0, 5]$ after the announcement (columns (2)-(5)-(8)), or the interaction between *Treated* and *Month* 0 (i.e., days $[0, 20]$) or *Month* 1 (i.e., days $[21, 40]$) (Columns (3)-(6)-(9)). In the specifications with the interactions, the levels of the variables are subsumed by the stock and day fixed-effects. In each specification, the standard errors are clustered at the manager and day level. In Panels B and C we repeat the exercise using as the dependent variable either the probability of selling or the probability of buying.

We find that in general, institutional investors are more likely to sell during the 61 days after the share repurchase announcement (Column (1)), but the magnitude of the effect is substantially stronger for mutual funds (Column (4)) than for pension funds (Column (6)). In particular, for mutual funds we find that the marginal probability of selling increases by 7 basis points, which is more than seven times bigger than the coefficients found for pension funds. Moreover, we find that mutual funds are more likely to buy during the six-day period immediately after the announcement when the stock price mostly appreciates (Column (5)).

Overall, mutual funds tend to enter the market as sellers only when the stock price has already moved upward, while pension funds seem to trade less intensively around announcements. This is consistent with the fact that mutual funds might anticipate the price movement and build a trading strategy that exploits the sharp stock price increase after the announcement.

3.6 Selling and Embedded Gains

This section focuses on the subsample of traders that hold a long position at the beginning of the announcement day and studies whether embedded gains affect their trading behavior. We define embedded gains as the raw return between the volume-weighted average price paid to build the position and the closing price on the day prior to the event.

3.6.1 Univariate Analysis

We start by showing average embedded gains and selling intensity for mutual funds and pension funds. We split the sample into two groups: *sellers*, that is, those with a negative net share volume at the end of the 61-day window previously considered; and its complement, *non-sellers*. We report results in Table 3.5. Panel A shows estimates for the full sample, while Panel B and C focus on positions held for less than one year, and those held for more than one year, respectively.[9]

We find that sellers have systematically lower embedded gains than non-sellers, and this difference is mainly driven by long-term positions. The difference is slightly more pronounced for pension funds than for mutual funds, which seems at odds with a tax-efficient story which would suggest the mutual funds, which tend to have some taxable investors, would have a higher incentive to sell positions with capital

[9] This distinction is driven by the different tax rules for short-term and long-term capital gains. Typically, long-term gains are taxed at lower tax rates.

losses than pension funds, which tend to have tax-exempt investors. However, pension funds tend to have longer investment horizons (and lower turnovers), which explains their relatively higher embedded capital gains.[10]

3.6.2 Embedded Gains and Probability of Selling

We next test whether embedded gains can affect the probability of selling in the cross-section of institutional investors. Similar to the analysis in Table 3.4, we run a linear probability model where the left-hand-side is the indicator for the negative net imbalance at the end of the day. The main explanatory variable is now the triple interaction with embedded gains, mutual fund indicator, and an indicator variable for treated firms. We use a manager-stock-day level panel and focus on the trades in the window [0, 60] for investors with a long position in the treated and control stock at the start of the announcement day.

Table 3.6 shows that, when compared to pension funds, mutual funds are less likely to sell the higher their capital gain on the treated stock. This seems to be consistent with a tax story, where pension funds care less about embedded gains, provided their better tax treatment.

Next, we test how the selling schedule changes at different level of past gains/losses. We start by looking at the cumulative density function of sales between embedded gains in the range [-100%, 100%]. Figure 3.4 shows the results. Overall, mutual funds seem to display a kink in the probability of selling around zero returns, suggesting that the sign of past gains/losses play a role in their decision to trade. To shed further evidence on this, we look at the probability of selling at different bins of embedded gains and report our findings in Figure 3.5. In each figure, the x-axis displays equally-spaced bins of embedded gains between -100% and 100%, while the y-axis shows the average probability of having a negative net imbalance at the end of the period [0, 60] around the share repurchase announcement. This analysis is similar in spirit to that of Ben-David and Hirshleifer (2012). However, our results differ substantially from theirs. In particular, for mutual funds trading the announcing firm, we find a reversed V-shaped selling schedule, that is the probability of selling is low at extreme realizations of past gains/losses, while is highest at near zero returns. The discontinuity around zero, when the selling probability is highest, is stronger for positions held for less than a year and seems to suggest that the sign realization is more important than the magnitude of the gains/losses. The results are likely driven by differences in the holding horizon across investors. Positions with small gains or losses are held for a shorter time period and high-turnover investors are more likely to liquidate these positions.

[10] Sikes (2017) shows that, since tax-sensitive investors are reluctant to sell stocks in which they have unrealized capital gains, firms repurchase fewer shares the greater the unrealized capital gains of their tax-sensitive investors relative to those of their tax-insensitive investors. This interpretation might explain why we find that traders who sell after a share repurchase are those that experience the bigger capital losses.

3.7 Conclusions

In this paper we study the trading by institutional investors around the announcement of share repurchases. We contrast mutual funds and pension funds and find that while mutual funds generally sell following the announcement. They do so only when the stock price already drifted upwards. Possibly consistent with tax-efficient trading, mutual funds are less likely to sell upon a gain, when compared to pension funds. The higher selling propensity of mutual funds seems to be concentrated on existing positions where they accumulated near-zero returns.

3.8 Tables

Table 3.1: Summary statistics. This table reports summary statistics at the firm level for the sample of share repurchase announcement. We report statistics for the full sample in Panel A. Panel B focuses on the subsample starting in 2004, when an amendmend to Rule 10b-18 of the Exhange Act required firms to disclose actual share repurchases. For each variable we report the number of observations, mean, standard deviation, minimum, median and maximum. In Panel C we report the average number of traders by investor type that are active in the market during a share repurchase.

Panel A: 1999-2014

	N	Mean	SD	Min	Median	Max
Program Size	2,117	0.104	0.143	0.008	0.063	0.997
Actual Completion	1,486	0.870	0.962	0.000	0.617	5.803
Log Size	2,117	7.673	1.707	4.196	7.562	12.265
Log Book-to-Market	2,117	-0.496	0.494	-1.958	-0.374	0.197
Cash Holdings	2,117	0.104	0.123	0.000	0.052	0.584
R&D Expenses	2,117	0.024	0.048	0.000	0.000	0.234
Dividend dummy	2,117	0.597	0.491	0.000	1.000	1.000
Buy-and-Hold Abnormal Return [0, 2]	2,114	0.018	0.059	-0.149	0.013	0.274
Previous Quarter Abnormal Return	2,117	-0.058	0.138	-0.501	-0.042	0.227
Previous Year Abnormal Return	2,117	-0.072	0.289	-0.746	-0.075	0.776
Previous 3-Year Abnormal Return	2,003	0.119	0.778	-1.346	0.005	3.449
No. firms: 1,369						

Panel B: 2004-2014

	N	Mean	SD	Min	Median	Max
Program Size	1,486	0.108	0.142	0.008	0.067	0.997
Actual Completion	1,486	0.870	0.962	0.000	0.617	5.803
Log Size	1,486	7.942	1.673	4.196	7.825	12.265
Log Book-to-Market	1,486	-0.514	0.477	-1.958	-0.431	0.197
Cash Holdings	1,486	0.109	0.118	0.000	0.065	0.584
R&D Expenses	1,486	0.024	0.046	0.000	0.000	0.234
Dividend dummy	1,486	0.600	0.490	0.000	1.000	1.000
Buy-and-Hold Abnormal Return [0, 2]	1,486	0.017	0.054	-0.149	0.013	0.274
Previous Quarter Abnormal Return	1,486	-0.040	0.114	-0.501	-0.031	0.227
Previous Year Abnormal Return	1,486	-0.046	0.234	-0.746	-0.054	0.776
Previous 3-Year Abnormal Return	1,427	0.148	0.683	-1.163	0.042	3.449
No. firms: 992						

Panel C: Number of traders

	All Institutions	MF	PF
Number of traders	395	138	257

Table 3.2: Matching criteria. This table summarizes the criteria we use to select the control firms used in the analysis. Section 3.3 extensively describes the matching algorithm.

	Matching criteria
1	Same Fama-French 12 industry
2	Ratio of market cap is between 0.5 and 2
3	Absolute difference in last quarter returns is ≤ 0.025
4	Absolute difference in last year returns is ≤ 0.1
	Events with a match: 2,117

Table 3.3: Treated vs. Control firms. This table reports t-tests for the difference in means between the treated and control firms along different firm characteristics. T-statistics for White standard error are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Treated	Control	Difference
Log Size	7.671	7.613	0.058
			(1.11)
Log Book-to-Market	-0.497	-0.501	0.004
			(0.26)
Cash Holdings	0.105	0.103	0.001
			(0.39)
R&D Expenses	0.024	0.025	-0.001
			(0.83)
Dividend Dummy	0.597	0.562	0.035**
			(2.30)
Previous Quarter Abnormal Return	-0.058	-0.057	-0.000
			(0.10)
Previous Year Abnormal Return	-0.072	-0.071	-0.001
			(0.12)
Previous 3-Year Abnormal Return	0.122	0.109	0.014
			(0.53)
Total Events	2,117		

Table 3.4: Selling behavior around share repurchases. This table reports results on the trading behavior of a manager in the quarter after a share repurchase announcement. The regressions are run at the manager-stock-day level, and we include all managers active in any stock during the event month. In Panel A, the dependent variable is either the difference between a dummy indicating sales and a dummy indicating buys, that is, it takes a value of 1 if the end of day imbalance is negative (i.e., a sell trade), is negative 1 if the imbalance is positive (i.e., a buy trade), and 0 if the manager is not trading that stock on that particular day. In Panels B and C, we focus on the probability of selling and buying, respectively. Treated is a dummy equal to 1 if the firm announces a share repurchase. Announcement is an indicator for days [0, 5] after the announcement. Month 0 (Month 1) is equal to one on days [0, 20] ([21, 40]). The control firms are those defined in Table 3.3. Fixed effects should be interpreted as cohort-group (e.g. manager fixed-effects looks at a manager within a specific event, only). Standard errors are clustered at the manager and day level (across cohorts). t-Statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A	Dependent variable: Probability of selling - Probability of buying (bps)								
	All managers			Mutual funds			Pension funds		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	2.996***			6.937***			0.877*		
	(4.402)			(4.121)			(1.890)		
Treated × Announcement		-2.799***			-6.714**			-0.695	
		(-2.593)			(-2.501)			(-0.928)	
Treated × Month 0			-2.265**			-5.411**			-0.573
			(-2.412)			(-2.257)			(-0.995)
Treated × Month 1			-0.122			-0.144			-0.109
			(-0.173)			(-0.080)			(-0.246)
Manager FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	102,063,370	102,063,370	102,063,370	35,684,390	35,684,390	35,684,390	66,378,980	66,378,980	66,378,980
R-squared	0.088	0.090	0.090	0.098	0.102	0.102	0.052	0.055	0.055

Panel B	Dependent variable: Probability of selling (bps)								
	All managers			Mutual funds			Pension funds		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	4.851***			10.438***			1.847***		
	(7.251)			(6.450)			(4.136)		
Treated × Announcement		1.392**			2.055			1.036**	
		(2.264)			(1.460)			(1.993)	
Treated × Month 0			0.141			-0.460			0.464
			(0.251)			(-0.344)			(1.063)
Treated × Month 1			0.229			-0.168			0.443
			(0.498)			(-0.156)			(1.155)
Manager FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	102,063,370	102,063,370	102,063,370	35,684,390	35,684,390	35,684,390	66,378,980	66,378,980	66,378,980
R-squared	0.179	0.181	0.181	0.200	0.204	0.204	0.077	0.080	0.080

Panel C	Dependent variable: Probability of buying (bps)								
	All managers			Mutual funds			Pension funds		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	1.855***			3.501***			0.970***		
	(4.531)			(3.737)			(2.727)		
Treated × Announcement		4.191***			8.769***			1.731***	
		(5.581)			(4.797)			(3.198)	
Treated × Month 0			2.406***			4.952***			1.037**
			(4.175)			(3.542)			(2.537)
Treated × Month 1			0.351			-0.024			0.552*
			(0.898)			(-0.025)			(1.888)
Manager FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	102,063,370	102,063,370	102,063,370	35,684,390	35,684,390	35,684,390	66,378,980	66,378,980	66,378,980
R-squared	0.259	0.261	0.261	0.301	0.305	0.305	0.092	0.095	0.095

Table 3.5: Average Embedded gains: Trades in [0, 60]. This table shows average embedded gains for mutual funds (MF), and pension funds (PF) compared to their trading behavior in the window [0, 60] after the share repurchase announcement. A fund sells if the end-of-period net share imbalance is negative. Embedded gains are computed for all managers that hold a long position in the announcing stock on day -1 and is defined as the raw return between the day when the position was opened and day -1 before the announcement. Panel A shows results for the all sample, while Panel B and C focus on positions held for less than a year and more than a year, respectively. The table also shows the proportion and number of institutions that sold. The sample is limited to managers with an open long position on day -1 before the event. We also show the t-tests for the differences in means. Standard errors are clustered at the stock-event day level. t-Statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Embedded Gains			Proportion			Institutions		
Panel A: All sample									
	Seller	Non-Seller	Diff	Seller	Non-Seller	Diff	Seller	Non-Seller	Diff
MF	11.686	27.583	-15.897***	0.206	0.794	-0.587***	4.221	16.236	-12.014***
			(8.62)			(32.24)			(60.76)
PF	14.062	32.430	-18.367***	0.166	0.834	-0.668***	4.064	20.441	-16.378***
			(9.52)			(39.82)			(64.10)
Panel B: Position held for less than a year									
	Seller	Non-Seller	Diff	Seller	Non-Seller	Diff	Seller	Non-Seller	Diff
MF	-0.521	-1.003	0.482	0.345	0.655	-0.310***	2.798	5.313	-2.516***
			(1.17)			(12.63)			(34.39)
PF	-1.675	-1.875	0.200	0.292	0.708	-0.417***	2.236	5.430	-3.195***
			(0.40)			(21.74)			(38.88)
Panel C: Postion held for more than a year									
	Seller	Non-Seller	Diff	Seller	Non-Seller	Diff	Seller	Non-Seller	Diff
MF	35.547	41.593	-6.046**	0.116	0.884	-0.769***	1.651	12.613	-10.962***
			(2.59)			(52.42)			(58.59)
PF	33.297	44.980	-11.684***	0.109	0.891	-0.782***	2.115	17.328	-15.213***
			(5.19)			(45.80)			(63.01)

Table 3.6: Embedded gains and selling behavior. This table reports results on the likelihood of a manager to sell in the quarter after the announcement of a share repurchase when she has a long position at the time of the event. The regressions are run at the manager-stock-day level, and we include all managers that trade the event-stock in the window [-30, -10] before the event. In columns (1) and (2), the dependent variable is the difference between a dummy indicating sales and a dummy indicating buys, that is, it takes a value of 1 if the end of day imbalance is negative (i.e., a sell trade), is negative 1 if the imbalance is positive (i.e., a buy trade), and 0 if the manager is not trading that stock on that particular day. In columns (3) and (4) we focus on the probability of selling alone. The main explanatory variables is the embedded gains of the position before the event. We compute embedded gains as the return between two dates, the volume weighted average purchase day of a long position and day -1 before the announcement. To compute embedded gains we use the volume-weighted average transaction price. The sample is restricted to managers that hold a long position at the beginning of the announcement month. We consider how embedded gains interact with the two groups of ANcerno investors (mutual funds - MF - and pension funds - PF). Horizon is the log-number of days since the position is open. The control firms are those defined in Table 3.3. All specifications include manager, and and day-stock fixed effects. Standard errors are clustered at the manager and day level. t-Statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Probability of selling (bps)			
	(1)	(2)	(3)	(4)
Embedded Gains × MF× Treated	-7.902***	-7.740**	-8.229***	-7.654**
	(-2.651)	(-2.538)	(-2.760)	(-2.459)
MF × Treated	14.172***	13.724**	14.627***	14.608***
	(2.658)	(2.538)	(2.655)	(2.590)
Embedded Gains × MF	-30.968***	-28.065***	-30.040***	-28.578***
	(-5.624)	(-4.756)	(-5.289)	(-4.887)
Embedded Gains × Treated	-0.621	1.828	-0.883	1.722
	(-0.526)	(1.059)	(-0.505)	(0.717)
Embedded Gains	37.088***	9.904**	22.667***	10.383**
	(9.078)	(2.438)	(6.919)	(2.262)
Horizon	-200.759***	-220.603***	-205.033***	-221.896***
	(-13.085)	(-12.639)	(-13.250)	(-12.767)
Treated	6.970***	10.863***	0.314	
	(3.050)	(4.363)	(0.098)	
Manager FE	Yes	Yes	Yes	Yes
Event FE	No	No	Yes	No
Stock-Day FE	No	No	No	Yes
Observations	11,282,926	11,282,926	11,282,926	11,279,134
R-squared	0.055	0.062	0.061	0.083

3.9 Figures

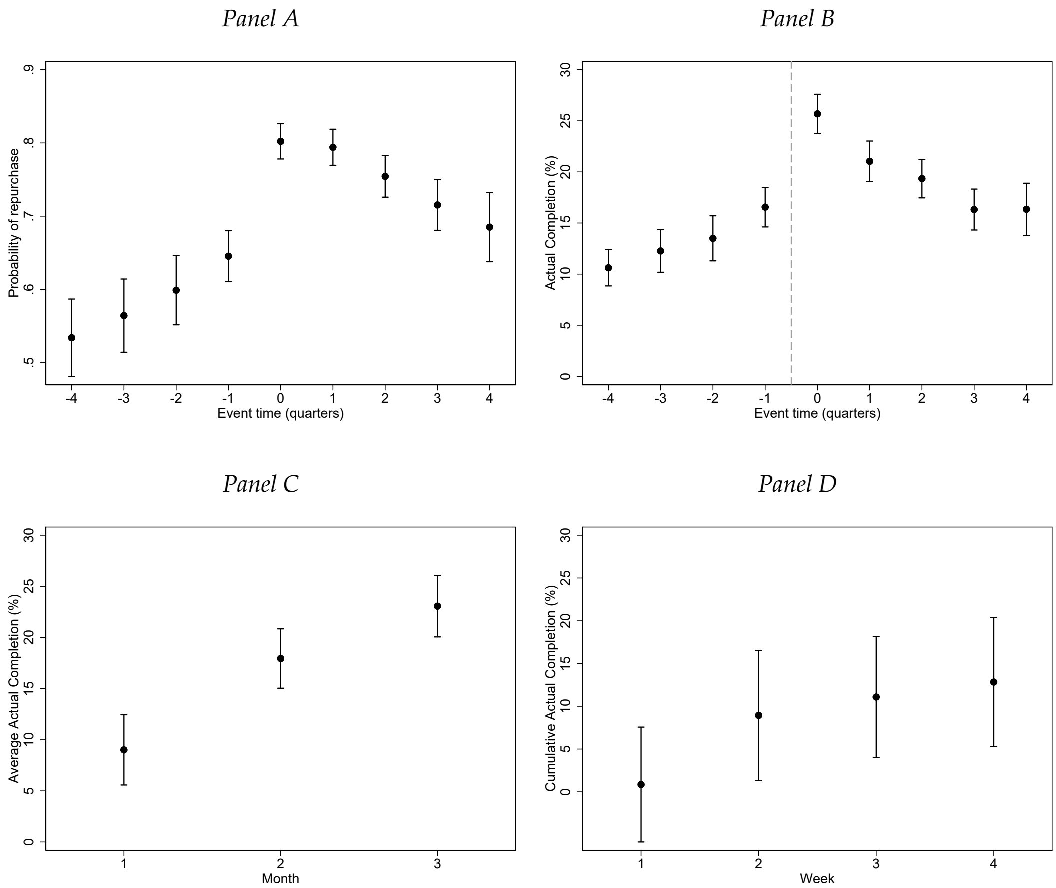


Figure 3.1: Actual share repurchases. Panel A shows coefficients and confidence interval bars from regressing a dummy equal to one if a firm reports actual repurchases in a given quarter in the window [-4; 4] around a share repurchase announcement taking place in quarter 0 onto event-time dummies. Panel B shows coefficients and confidence intervals from a regression of the percentage of target amount actually repurchased in a quarter onto event-time dummies. Panel C shows the cumulative completion rate for the three months after the announcement. To do so we assign repurchases observed at the end of the quarter when the announcement is made to month 1 when the announcement is made on the last four weeks of the quarter. Similarly, we assign total quarter repurchases to month 2 and 3 if the announcement is made on the middle five week and first four weeks of the quarter, respectively. In order to take into account that there might be share repurchases even in regular times we subtract to each quarterly observation an amount equal to (13-i)/13 of the repurchases made in the pre-announcement quarter, where i = 1,..., 13 is the week of the quarter when the announcement is made. To compute average actual share repurchases in the pre-announcement quarter, we focus on announcements made on the first month of the quarter. Panel D does the analogue exercise of Panel C by looking at the breakdown of the first 4 weeks after the announcement (i.e., week 13, 12, 11, 10 of the quarter). Confidence intervals are computed from standard errors clustered at the firm and time level. The sample starts in 2004, when actual share repurchases become available.

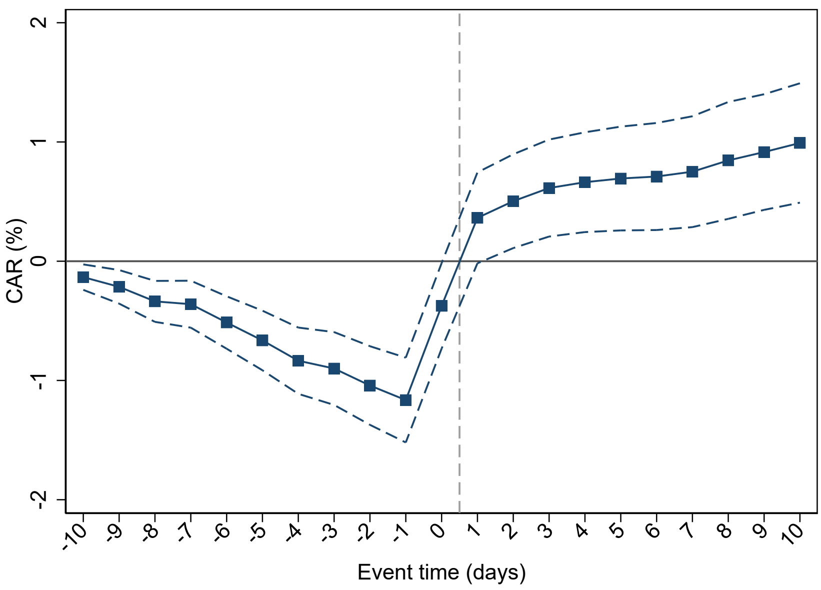


Figure 3.2: Cumulative average abnormal return around a share repurchase announcement. This figure displays the CAAR in the window [-10, 10] We cluster standard errors at the firm and time level. We report the point estimate (square) and the 95% confidence interval (dashed lines). The abnormal return is computed with respect to the CRSP value-weighted market returns.

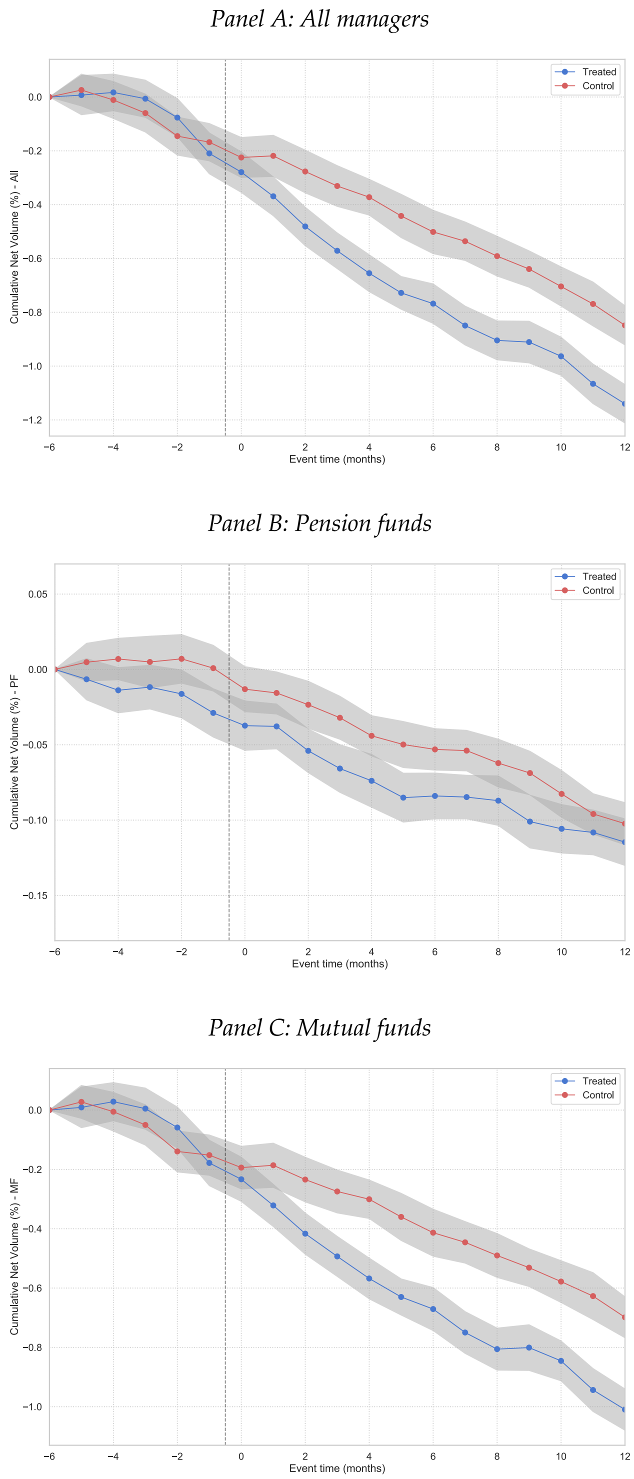


Figure 3.3: Cumulative dollar imbalance. This figure displays the cumulative signed dollar volume in the monthly window [-6, 12] around a share repurchase announcement. We cumulate monthly averages from a regression of monthly imbalance onto a set of event time dummies. In the regression we include stock and year-month fixed effects and cluster standard errors accordingly. We report the point estimate and the 95% confidence interval. We show results for all ANcerno managers, Pension funds and Mutual funds, separately.

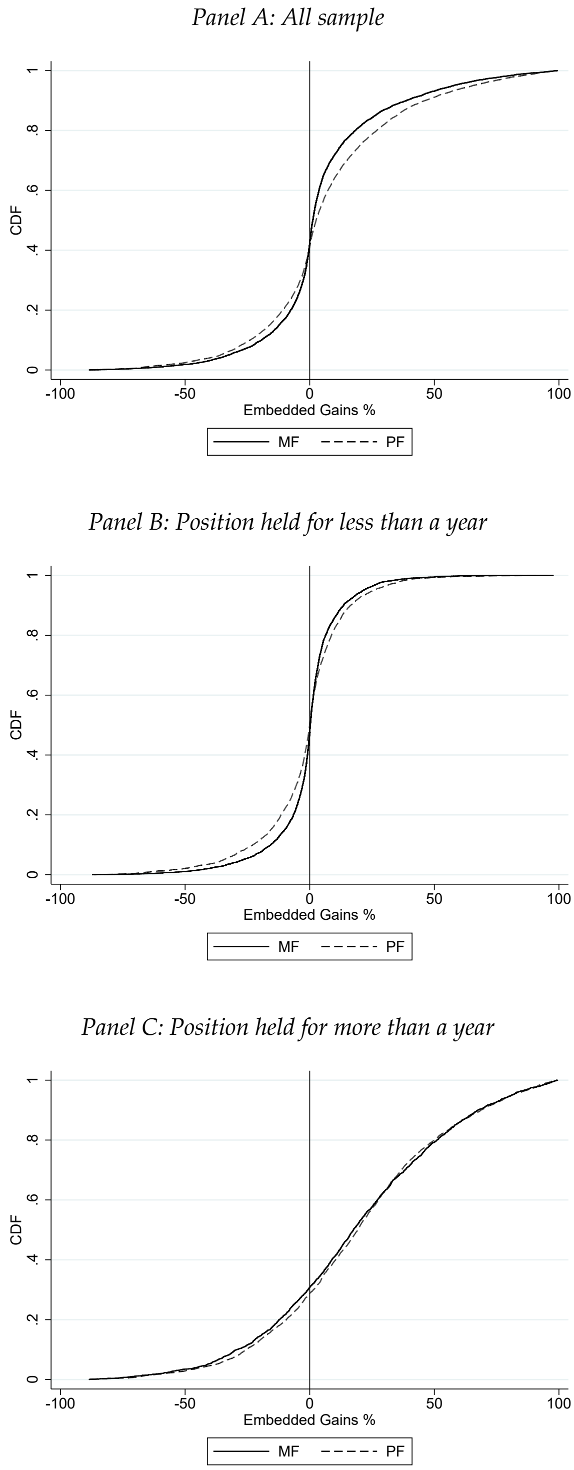
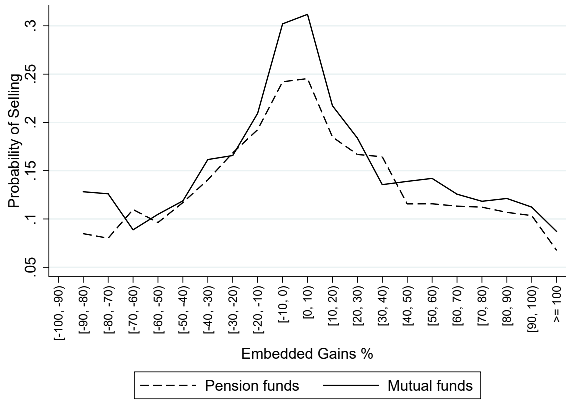
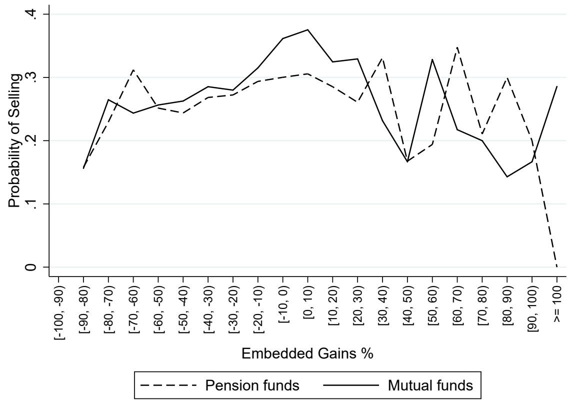


Figure 3.4: Cumulative distribution of Embedded Gains (MF v. PF). This figure displays the cumulative distribution of embedded gains for mutual funds (MF), and pension funds (PF) that sell in the window [0, 60] after the share repurchase announcement. A fund sells if the end-of-period net share imbalance is negative. Embedded gains are computed for all managers that hold a long position in the announcing stock on day -1 and is defined as the raw return between the day when the position was opened and day -1 before the announcement. Panel A shows results for the all sample, while Panel B and C focus on positions held for less than a year and more than a year, respectively. The sample is limited to managers with an open long position on day -1 before the event.

Panel A: All sample



Panel B: Position held for less than a year



Panel C: Position held for more than a year

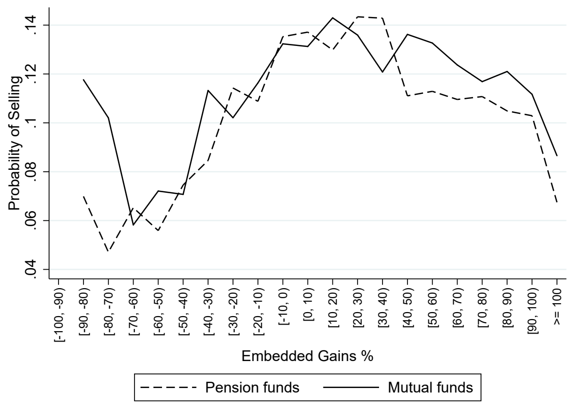


Figure 3.5: Proportion of sellers by bins of embedded gains (MF vs. PF). This figure displays, for different bins of embedded gains, the proportion of mutual funds (MF) sellers and that of pension funds (PF) sellers. In each figure, the x-axis displays equally-spaced bins of embedded gains between -100% and 100%, while the y-axis shows the average probability of having a negative net imbalance at the end of the period [0, 60] around the share repurchase announcement. Embedded gains are computed for all managers that hold a long position in the announcing stock on day -1 and is defined as the raw return between the day when the position was opened and day -1 before the announcement. Panel A shows results for the all sample, while Panel B and C focus on positions held for less than a year and more than a year, respectively. The sample is limited to managers with an open long position on day -1 before the event.

Appendix A

Mutual Funds' Fire Sales and the Real Economy

Sample construction

Mutual funds data We select the universe of domestic equity mutual funds, for which the holdings data are most complete and reliable from the CRSP Survivor-Bias-Free US Mutual Fund and Thomson Reuters (TR) s12 (formerly CDA/Spectrum) in the period 1980-2017. In particular, following Kacperczyk, Sialm, and Zheng (2008),we exclude funds in TR s12 that have the following Investment Objective Codes (variable IOC): International (ioc=1), Municipal Bonds (ioc=5), Bond and Preferred (ioc=6) and Balanced (ioc=7).

Similar to Kacperczyk, Sialm, and Zheng (2008) and Evans (2010), we screen the CRSP Mutual Fund database to remove all funds with "policy" variable in C & I, Bal, Bonds, Pfd, B & P, GS, MM and TFM. Next, we keep funds with Lipper Class (if available on CRSP Mutual Fund) equal to EIEI, G, LCCE, LCGE, LCVE, MCCE, MCGE, MCVE, MLCE, MLGE, MLVE, SCCE, SCGE, SCVE (Benos, Jochec, and Nyekel, 2010). If the Lipper Class code is unavailable, we use Strategic Insight Objective Code and include funds with SIOC in AGG, GMC, GRI, GRO, ING, SCG. If neither Lipper Objective Code nor Strategic Insight Objectives are available, we consider the Wiesenberger Fund Type Code and pick funds with the following objectives: G, G-I, AGG, GCI, GRI, GRO, LTG, MCG, and SCG. If Wiesenberger Fund Type Codes is also missing, but the fund has a CS policy (Common Stocks are the securities mainly held by the fund), then the fund is included.

Further, if "policy" variable is not available in CRSP, we exclude funds that on average hold less than 80% or more than 105% in stocks (Kacperczyk, Sialm, and Zheng, 2008).

Finally, we follow Kacperczyk, Nieuwerburgh, and Veldkamp (2014) and Franzoni and Schmalz (2017) and exclude observations for which the year of the observation is prior to the reported fund starting year, as well as observations for which the names of the funds are missing in the CRSP database. Because incubated funds tend to be smaller, we exclude funds before they pass the $5 million threshold for assets under management.

We then combine TR-s12 holdings data to CRSP Mutual Fund using MFLINKS, and select observations for which Total Net Assets in Thomson Reuters are not too different than Total Net Assets in CRSP, i.e. ratio $\in [0.5, 2]$ (Lou, 2012).

We perform the analysis at the portfolio (wficn) level. Data is aggregated by summing TNA across share classes, while for returns and expense ratio we take the TNA-weighted average. For all the other variable, we use the information available for the fund with largest TNA.

Finally, a further requirement is that the fund has non-missing headquarter information available in CRSP, and that it is located in one of the continental US states.

Firms data We start with CRSP MSF and CRSP-Compustat annual file from 1980 to 2017 to match the availability of mutual fund data. We select ordinary shares (shrcd 10 and 11) traded on the NYSE, NASDAQ, or AMEX stock exchange (exchcd 1, 2, 3, 31, 32, 33). Utilities (SIC 4900-4949) and financial firms (SIC 6000-6999) are excluded from the analysis. Finally, we exclude firms without information on the headquarter (7.56% of the sample) and those headquartered outside any of the continental US states.

Applying the filters for the treatment and control groups discussed in section 1.3, the final sample is made of 11,493 firms.

Geographic data We use the procedure outlined below to link county codes to zip and CBSA codes.[1] Linking zip code to county code is quite tricky as the former might span multiple counties.

We start with the list of county codes from Census Bureau and merge it with the U.S. Department of Housing and Urban Development (HUD) zip-county crosswalk file. After the merge we identify the county for which a given zip code has the largest share of total addresses, and residential addresses in.

However, the address count might not be enough to link zip codes to county codes. Hence, we next use the crosswalk provided by Census Bureau, which contains the county population percentage residing in that zip code. As before, we merge the crosswalk to the list of counties and keep the observations with largest population share.

We merge the two crosswalk files and proceed as follows. First, we rely on the Census Bureau's link, and then fill the missing values with HUD's residential address apportioned matches. When both are present but in conflict, we rely on Census Bureau's values which should be considered to have more integrity than HUD.

Finally, we use the Census Bureau's delineation file to assign each county to a CBSA.

[1] We adopt the methodology outlined by A.L. D'Agostino, and available at https://anthonylouisdagostino.com/a-better-zip5-county-crosswalk/.

Appendix Tables

Table A.1: Description of variables used in the analysis.

Variable	Description
Fund-level variables	
Flow (%)	$TNA_{j,t} - TNA_{j,t-1} \times (1 + R_{j,t})/TNA_{j,t-1}$.
Return	$R_{j,t}$, the quarterly fund raw return with monthly expenses added back (compounded from monthly CRSP MF data).
TNA	End of quarter fund Total Net Assets from CRSP MF database (original variable name: mtna).
Total Expense Ratio (TER)	Annual Total Expense Ratio from CRSP MF database (origina variable name: exp_ratio).
Turnonver	Fund Turnover defined in CRSP MF as the minimum (of aggregated sales or aggregated purchases of securities), divided by the average 12-month Total Net Assets of the fund (original variable name: turn_ratio).
Volatility	Standard deviation of past 12-month fund monthly returns.
Firm-level variables	
Q	$(at - ceq + (cshotimesprcc_f))/at$ (using variables names in CRSP-Compustat merged annual file).
Capex/PPE	$capx/l1.ppent$ (using variables names in CRSP-Compustat merged annual file).
CF/A	$(ib + dp)/at$ (using variables names in CRSP-Compustat merged annual file).
Size	End of year market capitalization expressed in log (using CRSP variables prc and shrout).
Turnover	Past 6-month average of volume per share (vol/shrout in CRSP)
Return	Monthly stock return from CRSP
DGTW-Adj. Return	Monthly DGTW-adjusted returns
Market-Adj. Return	Monthly market-adjusted returns using the value-weighted CRSP index as the benchmark
Payout ratio	$oibdp/at$ (using variables names in CRSP-Compustat merged annual file).
Tangibility	$ppent/at$ (using variables names in CRSP-Compustat merged annual file).
Profitability	$(dvp + dvc + prstkc)/oibdp$ (using variables names in CRSP-Compustat merged annual file).
Financial constraints (Kaplan-Zingales index)	Constructed using the specification in Lamont, Polk and Saa-Requejo (2001): KZ = -1.002CF + 3.139TLTD - 39.368TDIV - 1.315CASH + 0.282Q, where $CF = (ib + dp)/l.ppent$ is the cash flow, $TLTD = (dltt + dlc)/(dltt + dlc + seq)$ is the ratio of long term debt over assets, $TDIV = (dvc + dvp)/l.ppent$ is the dividend to capital ratio, $CASH = che/l.ppent$ is the cash to capital ratio, and Q is the market-to-book ratio.
Hurricane Hypothetical Sale (HHS)	See eq. (1.1)
Hurricane-Induced Flow (HIF)	See eq. (1.2)
Hurricane-Induced Surprise Flow (HISF)	See eq. (1.3)
CBSA-level variables	
Unemployement rate	From the Bureau of Labor Statistics
House Price Index (HPI)	From FRED database (variable ATNHPIUS)

Table A.2: Description of hurricane events. This table describes the 15 hurricanes used in the analysis. For each natural event, the table reports the name, the landfall date and year, the number of fatalities, the damages (in billions of US dollars both at the time of the event and adjusted for CPI in January 2020). Fatalities is the estimated total number of direct deaths in the US mainland due to the hurricane. Damages is the estimated value of total direct damages due to tropical storms in the US mainland expressed in billions of dollars. Damages (CPI adjusted) is the estimated value of total damages expressed in billions of dollars adjusted for the Consumer Price Index as of January 2020. Category measures the wind intensity according to the Saffir and Simpson Hurricane Wind Scale, which ranges from one (lowest intensity) to five (highest intensity). "TS" indicates Tropical Storm. The primary source of information is SHELDUS. Information about Start date, End date, Landfall date, Damages, and Fatalities comes from the tropical storm reports available in the archive section of the National Hurricane Center website. Information about Category comes from the NOAA Technical Memorandum (Blake, Landsea, and Gibney, 2011). The table also reports, for each hurricane, the number of treated and control funds/firm. The treatment and control group are based on the main definition as discussed in section 1.3.

Name	Landfall date	Year	Fatalities	Damages	Damages (CPI adj.)	Category	Funds		Firms	
							Treated	Control	Treated	Control
Hugo	22.09.1989	1989	21	7.00	14.52	4	4	285	150	3,480
Andrew	24.08.1992	1992	26	26.50	48.71	5	5	396	27	3,768
Opal	04.10.1995	1995	9	5.14	8.67	3	15	690	17	4,307
Fran	06.09.1996	1996	26	4.16	6.83	3	19	605	14	4,995
Floyd	14.09.1999	1999	56	6.90	10.64	2	485	761	1,462	1,928
Alison	05.06.2001	2001	41	9.00	13.11	TS	102	1,317	486	3,398
Isabel	18.09.2003	2003	16	5.37	7.51	2	531	1,262	1,470	873
Charley	13.08.2004	2004	10	15.11	20.67	4	56	2,116	91	2,974
Frances	05.09.2004	2004	7	9.51	12.96	2	218	1,954	1,708	1,120
Ivan	16.09.2004	2004	25	18.82	25.66	3	709	1,463	1,835	660
Jeanne	26.09.2004	2004	4	7.66	10.45	3	269	1,903	1,194	1,458
Katerina	25.08.2005	2005	1,500	108.00	142.54	3	123	1,948	201	2,748
Rita	24.09.2005	2005	7	12.04	15.67	3	62	2,009	48	2,698
Wilma	24.10.2005	2005	5	21.01	27.31	3	14	1,822	4	3,074
Ike	13.09.2008	2008	20	29.52	34.91	2	137	2,260	244	2,096

Table A.3: Predicting Hurricane Hypothetical Sale This table reports results for regressions of different versions of instruments based on hurricanes onto lagged quarterly stock returns. Columns (1)-(3) show estimates for *HHS*, the main instrument used in the analysis, while regressions for *HIF* and *HISF* are displayed in columns (4)-(6) and (7)-(9), respectively. Standard errors are clustered at the firm and time level. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Continuous instrument (firm-quarter level)								
	HHS			HIF			HISF		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Return (t-1)	0.013	-0.001	-0.003	-0.027	-0.004	0.001	-0.042	-0.000	0.004
	(0.695)	(-0.199)	(-0.837)	(-1.232)	(-0.939)	(0.240)	(-1.199)	(-0.028)	(0.899)
Return (t-2)	0.003	-0.001	-0.003	0.015	-0.000	0.005	0.020	0.003	0.008
	(0.200)	(-0.249)	(-1.021)	(1.192)	(-0.007)	(0.892)	(1.370)	(0.552)	(1.216)
Return (t-3)	0.052	0.008	0.006	-0.056*	-0.018*	-0.013	-0.049*	-0.019	-0.013
	(1.436)	(1.396)	(1.217)	(-1.847)	(-1.656)	(-1.444)	(-1.703)	(-1.566)	(-1.394)
Return (t-4)	0.002	0.004	0.002	0.027	-0.007*	-0.001	0.043	-0.007	-0.001
	(0.070)	(1.557)	(0.778)	(0.836)	(-1.753)	(-0.376)	(1.064)	(-1.401)	(-0.366)
Constant	0.006			-0.010			-0.011		
	(0.088)			(-0.191)			(-0.189)		
Stock FE	No	No	Yes	No	No	Yes	No	No	Yes
Time FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	498,638	498,638	498,303	498,638	498,638	498,303	498,638	498,638	498,303
Adjusted R-squared	0.003	0.715	0.717	0.004	0.386	0.392	0.006	0.450	0.457

Table A.4: Preference for proximity: Funds registered in one state. This table reports results for a linear probability model where the outcome variable is an indicator for outflows and the main explanatory variable is the interaction between *One State* and *Improved Economy*. *One State* is 1 if the funds operates in one US state only, and zero otherwise. *Improved Economy* takes values equal to 1 if the proxy for local economy improves across two adjacent quarters (decrease in unemployment rate or increase in HPI). Control variables include the total expense ratio, the fund turnover, previous quarter fund return, and the fund return volatility in the previous 12 months. Standard errors are clustered at the location-time level and t-statistics reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Outflow indicator							
Proxy for local economy condition	HPI				Unemployement rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
One State × Improved Economy	-0.034***	-0.034***	-0.034***	-0.035***	-0.022**	-0.018*	-0.021**	-0.018*
	(-2.786)	(-2.784)	(-2.816)	(-2.829)	(-2.110)	(-1.781)	(-2.046)	(-1.738)
Improved Economy	0.000	0.001	-0.001	-0.000	-0.004	-0.004	-0.004	-0.003
	(0.016)	(0.084)	(-0.140)	(-0.002)	(-0.812)	(-0.712)	(-0.798)	(-0.649)
Total Expense Ratio		-0.019***		-0.018***		-0.019***		-0.018***
		(-3.209)		(-3.025)		(-3.247)		(-3.063)
Turnover		0.007**		0.007**		0.007**		0.007**
		(2.396)		(2.362)		(2.398)		(2.366)
TNA		-0.082***		-0.082***		-0.081***		-0.082***
		(-17.859)		(-17.745)		(-17.862)		(-17.748)
Return		-0.151***		-0.151***		-0.151***		-0.151***
		(-29.321)		(-29.265)		(-29.319)		(-29.262)
Return Volatility		0.016***		0.016***		0.016***		0.016***
		(3.056)		(3.096)		(3.041)		(3.082)
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	No	No	Yes	Yes	No	No	Yes	Yes
Observations	73,898	73,898	73,898	73,898	73,898	73,898	73,898	73,898
Adjusted R-squared	0.183	0.207	0.185	0.208	0.183	0.207	0.185	0.208

Table A.5: Post-hurricane dollar outflows. This table reports the magnitude of the fund outflows in the 4 quarters following a hurricane event in million USD. For each quarter we report the outflow of the average fund and the total dollar outlay of the mutual fund industry. The cumulative industry effect is also reported in the third row. Dollar values are adjusted for inflation using January 2020 CPI. The computations consider only funds headquartered in the hurricane area in the quarter preceding the natural event. The estimates are from the regression in Table 1.4 with fund and quarter fixed effects.

	Event window (quarters)				
	0	1	2	3	4
Average fund $-outflow (mil.)	-16.15	-8.06	-10.71	-2.50	-11.72
Average industry $-outflow (mil.)	-2,480.48	-1,079.68	-1,347.50	-314.11	-1,488.91
Cumulative industry $-outflow (mil.)	-2,480.48	-3,560.17	-4,907.67	-5,221.77	-6,710.69

Table A.6: Hurricanes and fund flows: preference for proximity. This table reports triple differences estimates of the effects of hurricanes on funds located in the area affected by the adverse natural event. The triple difference focuses on affected funds which are most likely to have "local" clients. The proxy for local clientele are *(i)* the t-stat of a regression of outflows onto last quarter unemployment rate - run for each MSA separately - greater (smaller) or equal than 2 (-2) (columns 1-4); *(ii)* the fund operates only in the state in which it is headquartered (columns 5-8). The dependent variable is the fund flow, expressed in percentage points. Fund headquarters are identified in terms of Core-based Statistical Areas. *Disaster Zone* is a dummy variable equal to one if the CBSA of the fund headquarters is in the area hit by a hurricane over quarters [0, 12] after the hurricane. *Local* is an indicator for the two proxies of local clientele. The control variables are the Total Expense Ratio (TER), the log-TNA, the volatility of fund returns in the previous 12 months, and the fund return in quarter *q-1*. In all specifications, the level of *Local* and the other double-interaction terms are subsumed by the fixed-effects. Standard errors are clustered at the location-quarter level. Fixed-effects are interacted with *Local*. Moreover, when control variables are considered, the specification includes both the level of the variable and the interaction term. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Flow (%)							
Proxy for Local flows	Outflows correlated with unemployement				Fund operates in one state only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local × Disaster zone	-0.940**	-0.913*	-1.101**	-1.063**	-2.137***	-1.929***	-2.065***	-1.865***
	(-1.966)	(-1.914)	(-2.276)	(-2.203)	(-3.258)	(-2.935)	(-3.116)	(-2.806)
Disaster zone	-1.215***	-1.268***	-1.089***	-1.142***	-1.024***	-1.064***	-0.997***	-1.036***
	(-3.479)	(-3.618)	(-3.098)	(-3.240)	(-3.235)	(-3.390)	(-3.070)	(-3.218)
Control	No	Yes	No	Yes	No	Yes	No	Yes
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	No	No	Yes	Yes	No	No	Yes	Yes
Observations	122,975	122,975	122,975	122,975	75,638	75,638	75,638	75,638
Adjusted R-squared	0.115	0.135	0.116	0.135	0.110	0.131	0.112	0.133

Table A.7: Hurricanes: Treated v. Control funds. This table reports the sample mean and t-test for the difference in means of the group of funds hit by a hurricane at least once (Treated), and those always headquartered outside the hurricane area (Control). We consider observations in the pre-event window, only. Standard errors clustered at the fund and quarter level. T-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Treated	Control	Difference
Flow	-0.082	0.277	-0.359
			(1.16)
Return	0.021	0.019	0.002
			(0.97)
TNA	965.060	1,000.029	-34.968
			(0.38)
TER	0.013	0.012	0.001***
			(4.25)
Turnover	0.868	0.846	0.022
			(0.74)
Return volatility	0.045	0.045	0.000
			(0.46)
Stocks held	4.326	4.375	-0.049
			(1.35)
Stock size	8.488	8.431	0.058
			(1.12)
Stock turnover	0.002	0.002	-0.000
			(0.71)

** Observations recorded on pre-event window*

Table A.8: Fund style: treated v. control This table reports the fraction of funds in each of the 8 categories of fund style for the group of treated and control funds. Column 3 reports the t-test for the differences between the proportions in the two groups. *t*-statistics for standard errors double clustered at the fund and time level are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Treated	Control	Difference
Growth	0.392	0.387	0.005
			(0.24)
Growth-Income	0.172	0.170	0.002
			(0.11)
Hedged	0.001	0.002	-0.000
			(0.75)
Income	0.037	0.039	-0.002
			(0.28)
Large Cap	0.020	0.012	0.008
			(1.45)
Mid Cap	0.094	0.105	-0.011
			(0.87)
Small/Micro Cap	0.184	0.187	-0.003
			(0.17)
None	0.100	0.099	0.001
			(0.08)

Table A.9: Hurricanes and fund flows: matched sample. This table reports difference-in-differences estimates of the effects of hurricanes on funds located in the area affected by the adverse natural event. The dependent variable is the fund flow, expressed in percentage points. Fund headquarters are identified in terms of Core-based Statistical Areas (CBSAs). *Disaster Zone (q+i-j, q+i)* is a dummy variable equal to one for funds headquartered in any of the CBSAs hit by the hurricane in quarter (q) and the observation is recorded in quarters (q+i-j, q+i) around a hurricane event. The control variables are the Total Expense Ratio (TER), the log-TNA, the volatility of fund returns in the previous 12 months, and the fund return in quarter *q-1*. The control sample is made of funds matched to treated funds in quarter $q-1$ using the two nearest neighbors for each treated funds based on TNA, flows, expense ration, and fund return. Standard errors are clustered at the fund and quarter level. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Flows (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster zone (q-4, q-1)	-0.363	-0.325	-0.360	-0.304	0.074	-0.008
	(-0.925)	(-0.779)	(-0.917)	(-0.729)	(0.101)	(-0.012)
Disaster zone (q)	-1.842***	-1.561**	-1.861***	-1.554**	-1.454*	-1.780**
	(-2.729)	(-2.091)	(-2.731)	(-2.060)	(-1.797)	(-2.316)
Disaster zone (q+1, q+4)	-0.505	-0.340	-0.577	-0.397	-0.313	-0.385
	(-1.027)	(-0.679)	(-1.199)	(-0.811)	(-0.459)	(-0.563)
Disaster zone (q+5, q+8)	-0.794*	-0.697	-0.830*	-0.734	-0.560	-0.710
	(-1.676)	(-1.446)	(-1.768)	(-1.537)	(-0.964)	(-1.196)
Disaster zone (q+9, q+12)	-0.572	-0.378	-0.661	-0.486	-0.478	-0.955
	(-1.103)	(-0.704)	(-1.256)	(-0.898)	(-0.760)	(-1.514)
Total Expense Ratio		2.834***		2.937***		3.202***
		(4.874)		(5.109)		(5.389)
Turnover		0.180		0.166		0.096
		(0.731)		(0.678)		(0.397)
TNA		6.077***		6.164***		6.593***
		(9.017)		(9.127)		(8.945)
Return		3.336***		3.315***		1.156***
		(8.488)		(8.483)		(5.741)
Return volatility		0.132		0.109		0.162
		(0.340)		(0.281)		(0.524)
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	No	No
Location FE	No	No	Yes	Yes	No	No
State-Time FE	No	No	No	No	Yes	Yes
Observations	35,249	35,249	35,249	35,249	35,217	35,217
Adjusted R-squared	0.161	0.186	0.162	0.187	0.170	0.191

Table A.10: Hurricanes and fund flows: homogeneous sample. This table reports estimates of the effects of hurricanes on funds located in the area affected by the adverse natural event. The dependent variable is the fund flow, expressed in percentage points. Fund headquarters are identified in terms of Core-based Statistical Areas (CBSAs). *Disaster Zone (q+i-j, q+i)* is a dummy variable equal to one for funds headquartered in any of the CBSAs hit by the hurricane in quarter (q) and the observation is recorded in quarters (q+i-j, q+i) around a hurricane event. The control variables are the Total Expense Ratio (TER), the log-TNA, the volatility of fund returns in the previous 12 months, and the fund return in quarter *q-1*. The sample comprises of funds that are treated at least once during the sample. Hence, the same treated funds serve as control group when they are not affected by a hurricane. Standard errors are clustered at the fund and quarter level. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Flows (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster zone (q-4, q-1)	-0.168	-0.199	-0.095	-0.109	-0.038	-0.178
	(-0.349)	(-0.399)	(-0.199)	(-0.218)	(-0.060)	(-0.288)
Disaster zone (q)	-1.742**	-1.565*	-1.618**	-1.421	-1.039	-1.489*
	(-2.252)	(-1.806)	(-2.028)	(-1.618)	(-1.392)	(-1.827)
Disaster zone (q+1, q+4)	-0.450	-0.483	-0.400	-0.409	0.425	0.331
	(-0.841)	(-0.910)	(-0.740)	(-0.768)	(0.638)	(0.461)
Disaster zone (q+5, q+8)	-0.832	-0.825	-0.821	-0.790	0.334	0.159
	(-1.655)	(-1.626)	(-1.559)	(-1.488)	(0.625)	(0.295)
Disaster zone (q+9, q+12)	-0.725	-0.720	-0.706	-0.689	-0.394	-0.853
	(-1.243)	(-1.180)	(-1.220)	(-1.143)	(-0.627)	(-1.383)
Total Expense Ratio		1.490**		1.577**		1.594**
		(2.272)		(2.371)		(2.268)
Turnover		0.196		0.219		0.228
		(0.573)		(0.643)		(0.671)
TNA		4.657***		4.793***		4.973***
		(6.543)		(6.581)		(6.902)
Return		3.549***		3.515***		1.117***
		(9.653)		(9.563)		(5.724)
Return volatility		0.261		0.237		0.253
		(0.678)		(0.614)		(0.736)
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	No	No
Location FE	No	No	Yes	Yes	No	No
State-Time FE	No	No	No	No	Yes	Yes
Observations	20,403	20,403	20,403	20,403	20,395	20,395
Adjusted R-squared	0.175	0.200	0.177	0.202	0.184	0.202

Table A.11: Hurricane and stock price reversal: portfolio analysis. This table reports results for monthly calendar-time 4-factor regression for a long-short portfolio that buys stocks in the treated group in the previous 5-15 months and sells the control group. In column 1, standard errors are adjusted using Newey-West methodology with 6 lags, while column 2 uses weighted least squares using the monthly number of firms in the portfolio as weight. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable: Monthly return on long-short hurricane portfolio		
	Newey-West std errors	WLS regression
	(1)	(2)
Alpha	0.011**	0.011**
	(2.103)	(2.069)
Market Excess Return	0.091	0.040
	(0.805)	(0.296)
SMB	-0.194	-0.217
	(-1.607)	(-1.504)
HML	0.041	-0.100
	(0.168)	(-0.575)
UMD	-0.230***	-0.248**
	(-2.806)	(-2.601)
Observations	113	113

Table A.12: Hurricane and portfolio liquidation This table reports difference-in-differences estimates how treated stocks are traded in the two quarters after the hurricane. The sample is made of firms unrelated to the hurricane as described in section 1.3. The main treated group is made of firms held by funds hit by the natural event. The dependent variable is either *Trade* is the change in shares of a stock held by a fund, with split adjustment (columns 1-2), or an indicator for *Trade* being smaller than zero (columns 3-4). In the latter case the specification is a linear probability model for the probability that a treated stock is sold. *Disaster Zone (q, q+1)* is an indicator equal to one if the firm falls in the treated group and the observation is recorded in quarters $[0, 1]$ around the hurricane. The control variables are the fund's total expense ratio, its turnover, the fund log-TNA, the past quarter fund return, the past year fund return volatility, the firm's log-size, and past 6-month volume turnover. All variables are standardized. Standard errors are clustered at the firm and month level. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependend variable	Trade		Sell	
	(1)	(2)	(3)	(4)
Disaster Zone (q, q+1)	-0.016**	-0.017**	0.016*	0.017*
	(-2.389)	(-2.421)	(1.854)	(1.823)
Total Expense Ratio		0.018**		0.001
		(2.289)		(0.147)
Turnover (fund)		0.009		0.019***
		(1.368)		(4.762)
TNA		0.021***		-0.035***
		(2.980)		(-5.931)
Return (fund)		0.030***		-0.034***
		(3.746)		(-5.836)
Return volatility (fund)		-0.015***		0.016***
		(-2.620)		(3.646)
Size (firm)		0.018*		0.013***
		(1.969)		(3.887)
Turnover (firm)		-0.022***		0.014***
		(-7.305)		(13.235)
Fund FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	11,780,753	11,780,753	11,780,753	11,780,753
Adjusted R-squared	0.014	0.014	0.109	0.111

Table A.13: Hurricanes and stock returns: robustness This table reports results for a difference-in-differences regression equal to that used in Table A.5, but using firms with negative values of the measures in equations 1.2 and 1.3, instead. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	DGTW Adj. Ret		Market Adj.Ret	
	HIF	HISF	HIF	HISF
	(1)	(2)	(3)	(4)
Disaster Zone (t-6, t-1)	0.004	0.003	-0.001	-0.001
	(1.100)	(0.999)	(-0.129)	(-0.114)
Disaster Zone (t, t+5)	-0.011**	-0.011**	-0.021**	-0.021**
	(-2.278)	(-2.429)	(-2.444)	(-2.535)
Disaster Zone (t+6, t+15)	0.005**	0.005**	0.010*	0.008*
	(2.024)	(2.089)	(1.852)	(1.740)
Disaster Zone (t+16, t+24)	-0.000	0.000	-0.003	-0.002
	(-0.055)	(0.087)	(-0.537)	(-0.414)
Disaster Zone (t+25, t+48)	0.002	-0.000	-0.002	-0.003*
	(1.014)	(-0.144)	(-0.914)	(-1.915)
Size	-0.049***	-0.049***	-0.049***	-0.048***
	(-28.184)	(-28.161)	(-15.859)	(-15.651)
Turnover	-0.002***	-0.002***	-0.003***	-0.003***
	(-3.174)	(-3.164)	(-3.161)	(-3.138)
Stock FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	1,240,493	1,265,025	1,432,090	1,459,075
Adjusted R-squared	0.015	0.014	0.047	0.047

Table A.14: Hurricane and stock returns: stocks with institutional ownership. This table reports results for a difference-in-differences regression equal to that in table 1.5 except for the exclusion from the sample of firms with zero institutional ownership (columns 1-3), or with less than 1% of institutional ownership. The dependent variable is the DGTW-adjusted monthly return, and the control variables are the firm log-size and past 6-month volume turnover. Standard errors are clustered at the firm and month level. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	DGTW Adj. Monthly Returns					
	Institutional ownership > 0			Institutional ownership $> 1\%$		
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster Zone (t-6, t-1)	0.004	0.004	0.004	0.003	0.003	0.003
	(1.027)	(1.040)	(1.038)	(0.880)	(0.904)	(0.897)
Disaster Zone (t, t+5)	-0.013**	-0.013**	-0.013**	-0.012**	-0.012**	-0.012**
	(-2.563)	(-2.561)	(-2.559)	(-2.571)	(-2.564)	(-2.569)
Disaster Zone (t+6, t+15)	0.005**	0.005**	0.005**	0.004*	0.004*	0.004*
	(2.332)	(2.341)	(2.342)	(1.842)	(1.853)	(1.831)
Disaster Zone (t+16, t+24)		0.000	0.000		0.001	0.001
		(0.133)	(0.131)		(0.324)	(0.311)
Disaster Zone (t+24, t+48)			-0.000			-0.000
			(-0.044)			(-0.352)
Size	-0.051***	-0.051***	-0.051***	-0.049***	-0.049***	-0.049***
	(-26.169)	(-26.201)	(-26.173)	(-27.039)	(-27.031)	(-27.008)
Turnover	-0.002***	-0.002***	-0.002***	-0.003***	-0.003***	-0.003***
	(-3.130)	(-3.130)	(-3.129)	(-3.458)	(-3.459)	(-3.456)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,224,789	1,224,789	1,224,789	1,142,074	1,142,074	1,142,074
Adjusted R-squared	0.015	0.015	0.015	0.015	0.015	0.015

Table A.15: Real effects: robustness This table reports results for an IV-regressions equal to that used in Table 1.6, but where the instrument is changed to a dummy variable equal to 1 if *HHS* is in the top 75^{th} percent of the across hurricanes distribution (Panel A), *HIF* from equation 1.2 (Panel B), or *HISF* from equation 1.3. *T*-statistics are reported in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A	Instrument: HHS > 75th percentile											
Dependent variable	Capex/PPE											
	IV	1st Stage	RF	IV	1st Stage	RF	IV	1st Stage	RF	IV	1st Stage	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Q	0.597***			0.648***			0.501**			0.646**		
	(4.868)			(4.002)			(2.276)			(2.286)		
HHS		-0.156***	-0.093***		-0.132***	-0.085***		-0.140***	-0.070*		-0.121***	-0.078*
		(-4.411)	(-4.658)		(-4.397)	(-4.465)		(-3.284)	(-1.972)		(-3.423)	(-1.885)
Cash Flow	0.219***	-0.197***	0.101***	0.229***	-0.198***	0.101***	0.134*	-0.310***	-0.021	0.187*	-0.317***	-0.018
	(7.466)	(-7.169)	(9.942)	(6.313)	(-7.330)	(9.882)	(1.861)	(-8.989)	(-1.683)	(1.982)	(-8.506)	(-1.496)
Size	-0.113	0.647***	0.273***	-0.155	0.641***	0.261***	-0.094*	0.239***	0.025*	-0.140**	0.248***	0.020
	(-1.425)	(10.261)	(7.945)	(-1.516)	(10.805)	(8.221)	(-1.827)	(9.395)	(1.768)	(-2.081)	(9.273)	(1.425)
Firm FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Industry FE	No	No	No	No	No	No	Yes	Yes	Yes	No	No	No
Time FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Location-Time FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Industry-Location-Time FE	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Observations	105,519	105,519	105,519	105,408	105,408	105,408	105,411	105,411	105,411	94,368	94,368	94,368
Kleibergen-Paap F stat	19.460			19.340			10.780			11.710		
H_0: t-test size>10% (p-value)	0.022			0.023			0.189			0.154		
H_0: t-test size>25% (p-value)	0.000			0.000			0.005			0.003		
H_0: relative OLS bias>10% (p-value)	0.006			0.007			0.087			0.067		
H_0: relative OLS bias>30% (p-value)	0.001			0.001			0.017			0.012		
Adjusted R-squared		0.385	0.237		0.389	0.240		0.148	0.068		0.108	0.055

Panel B	Instrument: HIF											
Dependent variable	Capex/PPE											
	IV	1st Stage	RF	IV	1st Stage	RF	IV	1st Stage	RF	IV	1st Stage	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Q	0.745***			0.776***			0.627***			0.683**		
	(3.991)			(3.518)			(2.838)			(2.413)		
HIF		0.030***	0.023***		0.029***	0.022***		0.030**	0.019***		0.027**	0.019***
		(3.096)	(5.232)		(3.320)	(5.365)		(2.666)	(4.377)		(2.336)	(3.645)
Cash Flow	0.248***	-0.197***	0.101***	0.254***	-0.198***	0.100***	0.173**	-0.310***	-0.021	0.198**	-0.317***	-0.018
	(6.417)	(-7.172)	(9.968)	(5.666)	(-7.336)	(9.906)	(2.412)	(-8.992)	(-1.676)	(2.146)	(-8.510)	(-1.493)
Size	-0.209*	0.648***	0.274***	-0.236*	0.643***	0.262***	-0.124**	0.240***	0.026*	-0.149**	0.249***	0.021
	(-1.858)	(10.211)	(7.980)	(-1.783)	(10.735)	(8.252)	(-2.429)	(9.289)	(1.807)	(-2.210)	(9.111)	(1.456)
Firm FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Industry FE	No	No	No	No	No	No	Yes	Yes	Yes	No	No	No
Time FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Location-Time FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Industry-Location-Time FE	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Observations	105,519	105,519	105,519	105,408	105,408	105,408	105,411	105,411	105,411	94,368	94,368	94,368
Kleibergen-Paap F stat	9.585			11.02			7.105			5.459		
H_0: t-test size>10% (p-value)	0.244			0.179			0.396			0.526		
H_0: t-test size>25% (p-value)	0.00820			0.00434			0.0247			0.0514		
H_0: relative OLS bias>10% (p-value)	0.121			0.0817			0.230			0.341		
H_0: relative OLS bias>30% (p-value)	0.0271			0.0158			0.0675			0.122		
Adjusted R-squared		0.386	0.237		0.389	0.241		0.148	0.068		0.108	0.055

Panel C	Instrument: HISF											
Dependent variable	Capex/PPE											
	IV	1st Stage	RF	IV	1st Stage	RF	IV	1st Stage	RF	IV	1st Stage	RF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Q	0.678***			0.702***			0.599***			0.601***		
	(4.870)			(4.368)			(3.528)			(3.532)		
HISF		0.038***	0.026***		0.035***	0.025***		0.038***	0.023***		0.037***	0.022***
		(3.280)	(5.140)		(3.462)	(5.903)		(3.037)	(5.687)		(3.134)	(5.312)
Cash Flow	0.235***	-0.197***	0.101***	0.240***	-0.199***	0.100***	0.165***	-0.310***	-0.021	0.172***	-0.317***	-0.018
	(7.987)	(-7.192)	(9.981)	(7.183)	(-7.350)	(9.907)	(2.933)	(-9.000)	(-1.686)	(2.979)	(-8.517)	(-1.499)
Size	-0.166*	0.650***	0.275***	-0.189*	0.644***	0.263***	-0.118***	0.241***	0.027*	-0.129***	0.251***	0.022
	(-1.939)	(10.174)	(7.980)	(-1.933)	(10.691)	(8.242)	(-2.998)	(9.211)	(1.824)	(-3.189)	(9.039)	(1.476)
Firm FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Industry FE	No	No	No	No	No	No	Yes	Yes	Yes	No	No	No
Time FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Location-Time FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Industry-Location-Time FE	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Observations	105,519	105,519	105,519	105,408	105,408	105,408	105,411	105,411	105,411	94,368	94,368	94,368
Kleibergen-Paap F stat	10.760			11.980			9.222			9.821		
H_0: t-test size>10% (p-value)	0.190			0.145			0.263			0.232		
H_0: t-test size>25% (p-value)	0.005			0.003			0.010			0.007		
H_0: relative OLS bias>10% (p-value)	0.088			0.062			0.133			0.114		
H_0: relative OLS bias>30% (p-value)	0.018			0.011			0.031			0.025		
Adjusted R-squared		0.386	0.237		0.389	0.241		0.149	0.068		0.108	0.055

Appendix B

Strategic Trading as a Response to Short Sellers

Broker splitting when information is revealed

We can corroborate the conclusion that the evidence of order splitting across multiple brokers reflects strategic behavior by studying what happens after the information becomes public. If the increase in the number of brokers before an earnings announcement is due to strategic behavior, then rational investors should go back to their normal use of brokers after the fundamental has been revealed, given that trading with multiple and possibly unfamiliar brokers translates into higher costs. We bring this conjecture to the data by running a difference-in-differences analysis where the dependent variable is the probability that the managers use an abnormal number of brokers to buy a stock in a five-day window after the earnings announcement, i.e. days [1, 5]. In other words, we use the same dependent variable as in Table 2.6, but we study a period after the release of public information. Table B.9 reports the results. In brief, we find no significant difference between the use of brokers after the announcement in Pilot and non-Pilot stocks during the Reg SHO period. The outcome of this investigation further supports the view that the increase in the number of brokers before the earnings announcement is the result of a costly strategic behavior, in which positively informed investors engage to prevent information leakage and, ultimately, to profit from the price decline induced by short sellers.

Further evidence on the sell-side and Placebo tests

For completeness, we also report what happens in the case of sell trades for the dependent variables in Tables 5-8. As argued, in the extended sample of all the managers that we consider, sell trades are less likely to be information-driven. Hence, we do not expect any significant evidence of strategic behavior on the sell side. Consistent with this expectation, the estimates in Table B.10, Panel A, find no significant effect for Pilot stocks during the Program period, for the number of brokers, broker centrality, broker familiarity, and total sell volume in the period before the announcement. Next, we address the potential concern that the identification strategy based on the Reg SHO experiment captures a behavior that is not a response to the policy intervention but due to some correlated market movements. In particular, although unlikely given the random selection of the treated stocks, there could pre-trends in the variables of interest. To rule out this possibility, we conduct placebo difference-in-differences analyses focusing on the period that exactly precedes. Specifically, we use the period ranging from January 1999 (the date on which ANcerno starts) to July 2004 (the date on which the Reg SHO experiment was announced). In this case, the placebo-treated group contains the same stocks that are treated during the actual Reg SHO program, but in the period July 2002 – July 2004. In another set of specifications, we use the period after Reg RHO, but excluding the financial crisis, ranging from November 2010 (after the financial crisis and after the re-introduction of the uptick rule) to December 2014 (the end of the ANcerno sample). In this second case, the placebo-treatment occurs between November 2012 and December 2014. The set of treated stocks is the same as in the original analysis. We report the results in Panels B, C, and D of Table B.10. The coefficients on the interaction between Pilot and Program Period, which now refers to the placebo periods, are overall statistically insignificant and, sometimes, with a sign in the opposite direction relative to the main results. This evidence reassures us that we are indeed capturing behavior that is a specific response to the policy enacted during the Reg SHO experiment.

Appendix Tables

Table B.1: Main results using sample at intersection of Markit and ANcerno. This table replicates the main results of the paper using the most restricting sample in which we require the availability of both ANcerno and Markit data in the days around an earnings announcement. This sample consists of 2,273 stocks of which 704 are Pilot and 1,569 control. We provide results for positive news in Panel A, while negative news are shown in Panel B. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Panel A	Positive news									
Dependent variable	CAR Ratio	Variance Ratio	Cross correlation	Trading speed	Trade size (dollars)	Trade size (shares)	High n. of brokers	Broker centrality	Broker familiarity	Log-dollar volume
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pilot × Program Period	-0.063**	0.129***	0.140**	-0.016*	-0.039	-0.045	0.044***	-0.016**	-0.340**	0.067**
	(-2.390)	(2.692)	(2.184)	(-1.805)	(-1.504)	(-1.641)	(3.074)	(-2.381)	(-2.139)	(2.388)
Return Volatility	-0.010	0.068**	-0.042	0.000	-0.019	-0.010	0.021**	-0.008*	0.127	0.043**
	(-0.594)	(2.571)	(-1.058)	(0.004)	(-1.123)	(-0.559)	(2.467)	(-1.835)	(1.282)	(2.539)
Market Cap	0.038	-0.133**	0.109	-0.034***	0.123***	-1.051***	0.244***	-0.032***	-1.115***	1.235***
	(1.321)	(-2.398)	(1.359)	(-2.868)	(3.064)	(-24.520)	(13.948)	(-3.301)	(-4.933)	(34.988)
Amihud Illiquidity	0.005	-0.004	0.005	0.007**	-0.034***	-0.030*	0.012**	0.002	-0.137	0.022***
	(0.728)	(-0.247)	(0.198)	(1.980)	(-3.024)	(-1.709)	(2.412)	(0.538)	(-1.608)	(3.238)
Bid-Ask spread	0.033***	-0.019	0.054	-0.007	0.046***	0.028	-0.017*	-0.019***	-0.101	0.071***
	(2.686)	(-1.103)	(1.502)	(-1.392)	(3.157)	(1.107)	(-1.854)	(-4.399)	(-1.301)	(4.694)
Number of Analysts	0.021**	0.040**	-0.052**	0.004	0.003	0.006	0.004	-0.002	-0.036	0.022**
	(2.048)	(2.524)	(-2.068)	(1.286)	(0.307)	(0.570)	(0.839)	(-0.670)	(-0.723)	(2.333)
Surprise	-0.017	0.018	0.006	-0.004	-0.010	-0.040***	0.002	0.001	0.013	0.017**
	(-1.082)	(1.089)	(0.467)	(-1.490)	(-0.736)	(-3.321)	(0.715)	(0.379)	(0.389)	(2.472)
Total volume traded					1.114***	1.112***				
					(70.176)	(68.826)				
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,520	7,520	7,520	20,677	20,677	20,677	20,677	20,677	20,677	20,677
R-squared	0.382	0.377	0.379	0.205	0.701	0.695	0.434	0.197	0.343	0.772

Panel B	Negative news									
Dependent variable	CAR Ratio	Variance Ratio	Cross correlation	Trading speed	Trade size (dollars)	Trade size (shares)	High n. of brokers	Broker centrality	Broker familiarity	Log-dollar volume
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pilot × Program Period	-0.040	0.060	0.046	-0.014	0.033	0.025	0.001	-0.010	0.052	0.066
	(-0.747)	(0.392)	(0.346)	(-0.882)	(0.678)	(0.518)	(0.027)	(-0.748)	(0.173)	(1.445)
Return Volatility	-0.018	-0.015	-0.066	-0.003	0.005	-0.016	0.006	-0.012	0.128	0.012
	(-0.507)	(-0.215)	(-0.728)	(-0.370)	(0.170)	(-0.541)	(0.418)	(-1.443)	(0.714)	(0.423)
Market Cap	-0.148**	-0.024	-0.035	-0.076***	0.260***	-0.901***	0.192***	-0.014	-1.192***	1.101***
	(-2.560)	(-0.200)	(-0.223)	(-4.499)	(4.180)	(-14.024)	(7.618)	(-0.823)	(-4.087)	(22.865)
Amihud Illiquidity	0.033*	-0.007	-0.026	0.000	0.010	0.004	-0.004	0.006	-0.123	0.035***
	(1.844)	(-0.220)	(-0.602)	(0.052)	(0.304)	(0.128)	(-0.536)	(0.819)	(-1.212)	(3.951)
Bid-Ask spread	0.033	-0.027	0.007	-0.004	0.084***	0.113***	0.003	-0.015*	-0.487***	0.053**
	(0.938)	(-0.351)	(0.062)	(-0.490)	(3.192)	(4.040)	(0.201)	(-1.863)	(-2.739)	(1.978)
Number of Analysts	0.021	-0.044	-0.032	0.003	-0.006	0.006	-0.001	-0.003	-0.157	0.048***
	(1.046)	(-0.746)	(-0.639)	(0.462)	(-0.352)	(0.340)	(-0.152)	(-0.569)	(-1.562)	(2.771)
Surprise	-0.013	-0.136**	-0.015	0.001	-0.013	-0.029**	-0.001	-0.003	-0.033	-0.033***
	(-0.695)	(-2.406)	(-0.577)	(0.146)	(-0.927)	(-2.237)	(-0.154)	(-0.482)	(-0.444)	(-2.903)
Total volume traded					1.177***	1.166***				
					(52.912)	(51.893)				
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,449	2,449	2,449	8,290	8,290	8,290	8,290	8,290	8,290	8,290
R-squared	0.583	0.552	0.558	0.355	0.750	0.765	0.517	0.340	0.458	0.788

Table B.2: Markit sample: Sample Construction and Composition. Panel A describes the sample used in our main analysis, together with alternative samples stemming from filtering methodologies suggested by existing literature (Diether, Lee, and Werner (2009a); Heath, Ringgenberg, Samadi, and Werner, 2019). We also report a sample with no filtering (large sample) in which we use the 948 Pilot firms from the SEC list and use as controls the remaining firms that appeared in the Russell 3000 index both in May 2004 and after the reconstitution of May 2005. In the last two rows, in parentheses, we report the number of firms in the original paper and, out of the parentheses, the number of firms we obtain using the original paper's filters. Panel B reports the number of Treated (Pilot) and Control firms for the sample period used in Table 2.2, when we study how short selling reacted to the Reg SHO Pilot Program. The sample starts on May 2002, the first date at which Markit data is available (hence, the low coverage for that month), and ends in July 2007, when the Reg SHO Pilot Program expired. Note that even though we use the same sample construction technique for the entire analysis, we might have a different number of firms in different tests. For example, in our main tests, we need data availability in ANcerno and in the days surrounding an earnings announcement with a valid earnings surprise measure, which is not required when we use short selling variables from Markit. Panel B also reports, for comparison, the number of firms by month that appears in CRSP, using our sample or the large sample specification.

Panel A				
	Sample			
	Our sample	Diether et al. (2009)	Heath et al. (2019)	Large sample
Russell composition (control group)	Jun04 and Jun05	Jun04 and Jun05	Jun04 and Jun05	Jun04 and Jun05
Filter	*Is the filter applied?*			
Keep only ordinary shares	Yes	No	No	No
Drop if ticker changed	No	Yes	Yes	No
Drop NASDAQ small cap	No	Yes	Yes	No
Drop if changed listing venue	No	Yes	Yes	No
Drop AMEX stocks	No	Yes	Yes	No
Drop if merged/acquired/privitized	No	Yes	Yes	No
Drop if average price > 100	No	Yes	Yes	No
Drop if average quoted spread > $1	No	Yes	Yes	No
Keep if Compustat data available	No	No	Yes	No
	Number of firms before matching to Markit			
Pilot	823	768 (824 in paper)	571 (576 in paper)	948
Control	1,906	1,729 (1,661 in paper)	1,223 (1,132 in paper)	2,028

Panel B

	Sample Markit								Sample CRSP			
	Our sample		Diether et al. (2009)		Heath et al. (2019)		Large sample		Our sample		Large sample	
ym	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated	Control	Treated
2002m5	388	198	379	195	292	149	400	211	1,866	812	1,985	897
2002m6	1,155	544	1,099	521	788	398	1,225	588	1,873	812	1,994	897
2002m7	1,158	542	1,100	519	792	397	1,228	586	1,873	812	1,994	897
2002m8	1,184	556	1,127	532	819	405	1,257	600	1,874	812	1,995	897
2002m9	1,182	554	1,124	530	817	406	1,255	600	1,883	814	2,004	900
2002m10	1,171	553	1,113	528	809	405	1,242	601	1,890	815	2,011	902
2002m11	1,177	556	1,120	531	813	408	1,250	604	1,893	817	2,014	904
2003m1	1,207	567	1,146	542	830	416	1,281	617	1,894	820	2,016	907
2003m2	1,217	574	1,156	549	839	419	1,294	623	1,898	822	2,020	909
2003m3	1,449	644	1,358	615	983	465	1,544	713	1,901	822	2,023	909
2003m4	1,510	676	1,411	645	1,019	487	1,609	746	1,904	822	2,026	909
2003m5	1,501	669	1,402	639	1,014	481	1,599	738	1,906	823	2,028	910
2003m6	1,499	671	1,397	642	1,006	485	1,596	739	1,906	823	2,028	914
2003m7	1,522	683	1,419	653	1,014	491	1,621	752	1,906	823	2,028	916
2003m8	1,532	685	1,423	655	1,020	494	1,630	754	1,906	823	2,028	917
2003m9	1,553	695	1,445	663	1,033	500	1,649	771	1,906	823	2,028	919
2003m10	1,583	711	1,473	675	1,055	510	1,686	792	1,906	823	2,028	923
2003m11	1,586	712	1,473	675	1,059	509	1,688	794	1,906	823	2,028	926
2003m12	1,586	709	1,479	673	1,058	508	1,688	792	1,906	823	2,028	931
2004m1	1,618	720	1,501	682	1,072	513	1,722	809	1,906	823	2,028	934
2004m2	1,612	718	1,496	680	1,067	513	1,715	813	1,906	823	2,028	937
2004m3	1,622	720	1,503	683	1,072	514	1,729	815	1,906	823	2,028	941
2004m4	1,622	719	1,504	682	1,071	514	1,728	820	1,906	823	2,028	948
2004m5	1,626	722	1,509	685	1,075	517	1,732	826	1,906	823	2,028	948
2004m6	1,647	732	1,515	690	1,078	520	1,748	839	1,906	823	2,028	948
2004m7	1,627	728	1,508	689	1,074	519	1,706	836	1,906	823	2,028	948
2004m8	1,630	729	1,509	689	1,074	519	1,702	838	1,906	823	2,017	948
2004m9	1,638	730	1,514	689	1,078	519	1,703	840	1,906	823	1,996	948
2004m10	1,639	731	1,514	689	1,079	519	1,691	841	1,906	823	1,987	947
2004m11	1,648	731	1,519	689	1,081	519	1,692	841	1,906	823	1,978	947
2004m12	1,647	732	1,518	690	1,081	519	1,683	842	1,906	823	1,962	947
2005m1	1,650	733	1,519	691	1,081	520	1,677	842	1,906	823	1,955	947
2005m2	1,649	734	1,518	692	1,081	521	1,670	842	1,906	823	1,941	947
2005m3	1,655	734	1,521	692	1,083	521	1,669	842	1,906	823	1,931	947
2005m4	1,657	734	1,522	692	1,083	521	1,665	842	1,906	823	1,925	947
2005m5	1,657	735	1,522	693	1,082	522	1,658	840	1,906	823	1,919	947
2005m6	1,645	734	1,524	693	1,085	522	1,647	838	1,906	823	1,910	944
2005m7	1,632	730	1,513	690	1,085	521	1,634	834	1,889	822	1,894	942
2005m8	1,619	723	1,502	684	1,082	521	1,620	828	1,880	820	1,885	940
2005m9	1,610	719	1,495	680	1,084	521	1,611	824	1,869	814	1,873	935
2005m10	1,600	715	1,486	676	1,084	521	1,601	819	1,859	810	1,863	930
2005m11	1,589	713	1,477	674	1,084	521	1,591	816	1,851	807	1,855	927
2005m12	1,582	709	1,469	672	1,084	521	1,584	809	1,837	804	1,842	922
2006m1	1,577	706	1,464	667	1,087	522	1,578	806	1,830	799	1,835	915
2006m2	1,566	702	1,454	663	1,088	522	1,567	802	1,818	794	1,823	908
2006m3	1,560	701	1,448	662	1,089	523	1,562	798	1,809	789	1,814	903
2006m4	1,546	692	1,434	653	1,088	522	1,547	789	1,800	785	1,805	897
2006m5	1,545	694	1,431	654	1,089	523	1,548	789	1,790	781	1,795	892
2006m6	1,531	687	1,417	648	1,086	523	1,534	782	1,782	777	1,787	888
2006m7	1,520	686	1,406	647	1,086	523	1,523	781	1,771	773	1,776	884
2006m8	1,513	684	1,398	645	1,085	523	1,516	778	1,760	769	1,765	880
2006m9	1,508	678	1,394	641	1,087	523	1,512	772	1,751	766	1,756	875
2006m10	1,505	677	1,391	640	1,088	523	1,509	771	1,743	761	1,748	870
2006m11	1,492	675	1,379	638	1,087	523	1,496	768	1,741	759	1,746	868
2006m12	1,478	671	1,364	634	1,087	523	1,482	763	1,728	758	1,733	866
2007m1	1,465	666	1,351	629	1,084	522	1,469	758	1,714	756	1,719	863
2007m2	1,453	657	1,340	620	1,083	523	1,457	747	1,701	752	1,706	859
2007m3	1,445	655	1,332	618	1,083	523	1,449	744	1,692	744	1,697	849
2007m4	1,436	651	1,324	614	1,084	523	1,439	740	1,685	739	1,690	843
2007m5	1,433	651	1,322	614	1,089	524	1,436	739	1,675	735	1,679	839
2007m6	1,417	647	1,306	610	1,088	524	1,419	735	1,665	732	1,669	835
2007m7	1,406	642	1,295	605	1,088	524	1,408	727	1,651	730	1,655	832
2007m7	1,406	642	1,298	605	1,091	524	1,408	727	1,637	723	1,640	825

Table B.3: Representativeness analysis of the Markit sample. This table reports statistics for stock characteristics on Russell 3000 firms appearing in the Markit sample compared to those in CRSP (Panel A), or those in the Compustat supplemental short interest file (Panel B). In each panel, all variables are computed using CRSP daily data. We report results for the entire sample of Markit availability (May 22, 2002 – December 31, 2018) and the sample we use to run the difference-in-differences models around the Reg-SHO Pilot Program (May 22, 2002 – July 6, 2007). For each variable, we report the mean in each sample, together with a t-test for the difference in means between the samples. For the t-test, we report, in parentheses, the t-statistics for standard errors clustered at the firm and time level.

Panel A	Comparison with CRSP sample					
	Full sample (2002-2018)			Diff-in-diff sample (May 22, 2002 - July 6, 2007)		
	Markit	CRSP	Difference	Markit	CRSP	Difference
Market cap (mil)	3,307.581	3,196.002	111.579	3,269.039	3,102.237	166.803
			(0.64)			(0.90)
Return (bps)	5.608	5.718	-0.110	17.256	16.805	0.451
			(0.31)			(0.83)
Return volatility (bps)	253.569	256.044	-2.475	205.967	210.963	-4.996***
			(1.58)			(2.93)
Amihud Illiquidity	0.094	0.097	-0.004	0.043	0.051	-0.008**
			(0.66)			(2.13)
Turnover (%)	0.847	0.852	-0.005	0.803	0.807	-0.004
			(0.40)			(0.26)
Bid-Ask spread (bps)	28.304	28.347	-0.043	28.224	29.079	-0.855
			(0.07)			(1.31)
% listed in NYSE	43.188	42.626	0.562	45.475	44.448	1.027
			(0.50)			(0.83)
% listed in Amex	3.009	3.177	-0.168	2.689	2.869	-0.180
			(0.58)			(0.51)
% listed Nasdaq	53.795	54.101	-0.306	51.816	52.530	-0.713
			(0.27)			(0.58)

Panel B	Comparison with short interest sample					
	Full sample (2002-2018)			Diff-in-diff sample (May 22, 2002 - July 6, 2007)		
	Markit	Short Interest	Difference	Markit	Short Interest	Difference
Market cap (mil)	3,889.425	3,742.144	147.281	3,311.845	3,181.566	130.279
			(0.60)			(0.70)
Return (bps)	5.395	5.316	0.079	17.247	16.900	0.347*
			(0.22)			(1.80)
Return volatility (bps)	258.971	261.729	-2.758	204.796	208.154	-3.359**
			(1.52)			(2.08)
Amihud Illiquidity	0.215	0.213	0.001	0.024	0.026	-0.002
			(0.08)			(1.49)
Turnover (%)	0.875	0.882	-0.007	0.798	0.806	-0.008
			(0.54)			(0.59)
Bid-Ask spread (bps)	30.650	30.548	0.103	25.863	26.117	-0.254
			(0.14)			(0.48)
% listed in NYSE	43.530	43.179	0.351	46.670	46.225	0.445
			(0.31)			(0.36)
% listed in Amex	3.026	3.195	-0.169	2.760	2.955	-0.195
			(0.58)			(0.54)
% listed Nasdaq	53.438	53.616	-0.177	50.553	50.800	-0.247
			(0.16)			(0.20)

Table B.4: Short selling activity around Reg SHO: Robustness tests. This table reports a series of robustness tests for our first-stage result, in which we show that short selling activity increases for Pilot stocks during the Reg-SHO Program period. Panel A reports generalized difference-in-differences estimates around Reg-SHO for Markit variables (monthly average of shares on loan and shares lendable as a percentage of shares outstanding) on the alternative sample selections described in Table B.1. In Panel B, we re-run the analysis of Table 2.2 but with a sample starting in 2004 to have full coverage of Markit data. Finally, Panel C and D report difference-in-differences estimates around Reg-SHO, where the dependent variable is Compustat short interest and Markit shares on loan, respectively. In both Panels, columns (1)-(2) show results when the dependent variable is scaled by the shares outstanding from CRSP, while in columns (3)-(4) the scaling variable is the average daily volume computed in the previous year. In Panels A and B, standard errors are clustered at the stock and time level, while we follow Diether, Lee, and Werner (2009a) and use Newey-West standard errors with 3 lags in Panels C and D. T-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A	Different definitions of treatment and control groups					
	Diether et al. (2010)		Heat et al. (2019)		Large sample	
Dependent variable	Shares on loan (%)	Shares lendable (%)	Shares on loan (%)	Shares lendable (%)	Shares on loan (%)	Shares lendable (%)
	(1)	(2)	(3)	(4)	(5)	(6)
Pilot × Program Period	0.282*	0.815***	0.341**	0.704**	0.297**	0.776***
	(1.910)	(3.031)	(2.009)	(2.274)	(2.151)	(3.005)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	123,729	123,585	92,993	92,616	140,497	140,343
R-squared	0.576	0.857	0.582	0.867	0.577	0.848

Panel B	Restricted sample (2004-2007)			
Dependent variable	Shares on loan (%)		Shares lendable (%)	
	(1)	(2)	(3)	(4)
Pilot × Program Period	0.323**	0.299*	0.988***	0.892***
	(2.193)	(1.943)	(3.694)	(3.253)
Pilot	-0.020		0.256***	
	(-0.225)		(2.995)	
Program Period	2.946***		14.390***	
	(12.327)		(12.187)	
Constant	1.487***		2.525***	
	(12.841)		(8.012)	
Stock FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
Observations	95,810	95,810	95,810	95,810
R-squared	0.116	0.638	0.453	0.850

Panel C	Compustat short interest			
Scaling variable	Shares outstanding		Average daily volume	
	(1)	(2)	(3)	(4)
Pilot × Program Period	0.140*	0.133***	0.857**	0.673***
	(1.669)	(3.140)	(2.015)	(2.686)
Pilot	0.185***		0.653**	
	(3.544)		(2.553)	
Program Period	1.560***		7.095***	
	(33.212)		(29.395)	
Constant	3.378***		25.216***	
	(118.032)		(174.874)	
Stock FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
Observations	138,472	138,471	136,008	136,005
R-squared	0.042	0.674	0.034	0.560

Panel D	Markit shares on loan (Newey-West standard errors)			
Scaling variable	Shares outstanding		Average daily volume	
	(1)	(2)	(3)	(4)
Pilot × Program Period	0.348***	0.323***	1.246***	0.817***
	(4.541)	(7.155)	(3.097)	(3.167)
Pilot	-0.038		-0.414***	
	(-1.333)		(-2.754)	
Program Period	3.146***		21.448***	
	(72.987)		(92.989)	
Constant	0.991***		6.567***	
	(58.177)		(72.532)	
Stock FE	No	Yes	No	Yes
Time FE	No	Yes	No	Yes
Observations	132,464	132,464	131,809	131,809
R-squared	0.198	0.586	0.282	0.563

Table B.5: Size of earnings surprise and day-of-the-week distribution around Reg-SHO. This table reports average earnings surprise, as measured by the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement (Della Vigna and Pollet, 2009) for the Pilot and the Control groups of stocks. We also report the proportion of announcements for each day of the week. We show results for the period before (May 2003 – April 2005) and after (May 2005 – July 2007) the inception of the Reg SHO Pilot Program. We also report the differences (before vs. after and pilot vs. control) and the double-differences. When we compute differences and double-differences, we report t-statistics for standard errors clustered at the stock and time level. The surprise is expressed in percentage points.

	Surprise size				Monday		
	Before	After	Difference		Before	After	Difference
Pilot	5.217	2.976	-2.241	Pilot	0.101	0.109	0.008
			(-0.734)				(0.363)
Control	7.989	3.504	-4.485**	Control	0.118	0.123	0.005
			(-2.310)				(0.197)
Difference	-2.772	-0.528	2.244	Difference	-0.017**	-0.014	0.003
	(-1.111)	(-0.201)	(-0.627)		(-2.037)	(-1.472)	(0.325)
	Tuesday				Wednesday		
	Before	After	Difference		Before	After	Difference
Pilot	0.253	0.249	-0.004	Pilot	0.278	0.274	-0.004
			(-0.095)				(-0.088)
Control	0.243	0.240	-0.003	Control	0.267	0.260	-0.008
			(-0.066)				(-0.172)
Difference	0.010	0.009	-0.001	Difference	0.010	0.014	0.004
	(0.888)	(0.719)	(-0.107)		(0.828)	(1.030)	(0.296)
	Thursday				Friday		
	Before	After	Difference		Before	After	Difference
Pilot	0.322	0.320	-0.002	Pilot	0.046	0.049	0.003
			(-0.049)				(0.232)
Control	0.327	0.328	0.001	Control	0.045	0.049	0.005
			(0.016)				(0.447)
Difference	-0.005	-0.008	-0.003	Difference	0.001	-0.001	-0.002
	(-0.350)	(-0.542)	(-0.230)		(0.264)	(-0.145)	(-0.398)

Table B.6: Differential price pattern (Pilot v. Non-Pilot) around positive news. This table reports the coefficients (and the cumulative coefficients) for the following regression (plotted in Figure 2.1):

$$AR_{i,t}^{t+s} = \alpha_i + \theta_t + \sum_{s=-10}^{5} \beta_{t+s}^{Pilot} \times Pilot_i \times Program\ Period_t^{t+s} + \sum_{j=1}^{N} \gamma_j X_{i,t}^{j} + \varepsilon_{i,t}\,,$$

where $AR_{i,t}^{t+s}$ is the stock i DGTW-adjusted return on day t+s around time-t for positive earnings announcement, with $s \in [-10, 5]$. α_i, θ_t are stock and time fixed effects, respectively; Pilot is a dummy equal to 1 if the stock is included in the Reg SHO Pilot Program, $Program\ Period_t^{t+s}$ is a dummy equal to one if day t+s around an earnings announcement scheduled on day t falls within the Reg SHO Program Period (May 2005-July 2007). The vector of control variables, $X_{i,t}^{j}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, and previous year average bid-ask spread. In column AR we report, for each day in the pre-earnings announcement window, the estimates of the coefficient β_{t+s}^{Pilot}, while CAR is computed by cumulating $\hat{\beta}_{t+s}^{Pilot}$. When computing t-statistics for CARs, we take into account all the covariances of the type $Cov(\beta_{t+i}^{Pilot}, \beta_{t+j}^{Pilot})$, for $\forall i \neq j$. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. We report results for positive earnings surprise and consider the subsample for which the total information release, as proxied by the total event CAR, is high enough (Weller, 2018). The sample spans the period between May 2002 and July 2007. We cluster standard errors at the stock and day level. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

T	AR	t-stat	CAR	t-stat
-10	-12.076**	-2.113	-12.076**	-2.113
-9	-20.547***	-3.809	-32.623***	-3.725
-8	-15.232***	-2.753	-47.855***	-3.960
-7	-13.147**	-2.361	-61.001***	-4.006
-6	-13.276***	-2.767	-74.278***	-4.218
-5	-9.437*	-1.793	-83.715***	-4.101
-4	-22.674***	-4.488	-106.389***	-4.542
-3	-12.053**	-2.335	-118.442***	-4.522
-2	-15.718***	-2.965	-134.160***	-4.632
-1	-17.911***	-3.009	-152.071***	-4.736
0	100.087***	8.639	-51.984	-1.412
1	84.697***	7.152	32.713	0.801
2	-28.279***	-4.376	4.434	0.102
3	-29.130***	-5.060	-24.697	-0.538
4	-19.607***	-3.714	-44.304	-0.919
5	-14.506***	-2.761	-58.810	-1.151

Table B.7: Short selling induced price impact. This table reports averages for Pilot and Control groups in the period before and after the beginning of the Reg-SHO Pilot Program (May 2005) together with the single and double differences. In particular, we show statistics for the daily short volume in thousands of shares and the (permanent) price impact per one-thousand shares, estimated as per Equation (2). We use TAQ NYSE short-selling data. The calculation for short selling induced price impact is reported in the last row and is detailed in Equation (3).

	Pilot			Control			
	After	Before	Diff	After	Before	Diff	Diff-diff
Price impact per 1,000 shares (bps)	0.0327	-0.0041	0.0368	-0.0664	-0.0915	0.0251	0.0117
Short volume (1,000 shares)	190.32	183.65	6.67	174.84	169.52	5.31	1.36
Price impact (bps)	*6.22*	*-0.75*	*6.97*	*-11.61*	*-15.51*	*3.90*	***3.07***

Table B.8: Differential price pattern (Pilot v. Non-Pilot) around positive news. This table reports the coefficients (and the cumulative coefficients) for the following regression (plotted in Figure 2.1):

$$Trading\ Speed_{i,t}^{t+s} = \alpha_i + \theta_t + \sum_{s=-10}^{1} \beta_{t+s}^{Pilot} \times Pilot_i \times Program\ Period_t^{t+s} + \sum_{j=1}^{N} \gamma_j X_{i,t}^{j} + \varepsilon_{i,t},$$

where $Trading\ Speed_{i,t}^{t+s}$ is the stock i ratio of daily dollar volume on the buy-side to total event volume on the buy side computed on day t+s around time-t for positive earnings announcement, with $s \in [-10, 1]$. We compute the total event volume in the window [-10, 1]. α_i, θ_t are stock and time fixed effects, respectively; Pilot is a dummy equal to 1 if the stock is included in the Reg SHO Pilot Program, $Program\ Period_t^{t+s}$ is a dummy equal to one if day t+s around an earnings announcement scheduled on day t falls within the Reg SHO Program Period (May 2005-July 2007). The vector of control variables, $X_{i,t}^{j}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, and previous year average bid-ask spread. In column AR we report, for each day in the pre-earnings announcement window, the estimates of the coefficient β_{t+s}^{Pilot}, while CAR is computed by cumulating $\hat{\beta}_{t+s}^{Pilot}$. When computing t-statistics for CARs, we take into account all the covariances of the type $Cov(\beta_{t+i}^{Pilot}, \beta_{t+j}^{Pilot})$, for $\forall i \neq j$. We report results for buy trades on positive earnings surprise only. Following Della Vigna and Pollet (2009), we define the earnings surprise as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. We report results for positive earnings surprise and consider the subsample for which the total information release, as proxied by the total event CAR, is high enough (Weller, 2018). The sample spans the period between May 2002 and July 2007. We cluster standard errors at the stock and day level. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

T	Volume Ratio	t-stat	Cumulative	t-stat
-10	0.002	.5147955	0.002	0.515
-9	-0.003	-1.114818	-0.001	-0.305
-8	-0.010***	-3.595318	-0.012*	-1.833
-7	-0.009***	-2.914384	-0.020***	-2.621
-6	-0.008***	-3.295117	-0.029***	-3.212
-5	-0.020***	-7.239779	-0.049***	-4.733
-4	-0.020***	-6.959544	-0.069***	-6.023
-3	-0.018***	-6.88093	-0.087***	-6.943
-2	-0.018***	-6.697286	-0.106***	-7.625
-1	-0.011***	-3.688716	-0.117***	-7.712
0	0.032***	7.197648	-0.085***	-5.106
1	0.061***	12.7312	-0.024	-1.402

Table B.9: Broker splitting after the information is revealed. This table reports results for the following diff-in-diff regression around Reg SHO:

$$y_{i,t} = \alpha_i + \delta_t + \beta(Pilot_i \times Program\ Period)_t + X'_{i,t}\gamma + \varepsilon_{i,t}\,,$$

where, $y_{i,t}$ is a dummy equal to one if the average number of brokers used by managers to trade a stock in the window [1, 5] after an earnings announcement is above the median value of the sample distribution. α_i, θ_t are stock and time fixed effects, respectively; *Pilot* is a dummy equal to one if the stock is included in the Reg SHO Pilot Program. *Program Period* is an indicator for the time in which the program took place (May 2005 – July 2007). The vector of control variables, $X'_{i,t}$, comprises market capitalization, previous year's return standard deviation, Amihud illiquidity, number of analysts following the company, previous year average bid-ask spread, and earnings surprise. We show estimates for positive news and negative news, separately, and report results for buy trades, only. Following Della Vigna and Pollet (2009), we define the earnings surprise (news) as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. The sample is at the stock-event level and spans the period between May 2002 and July 2007. We consider the subsample of managers defined as being active. A manager is active if the adjusted R-squared of the regression of next month trading in a stock (as a % of total volume treaded next month) onto current stock holdings (as a percentage of portfolio holdings) ranks below the median of the across-managers R-squared distribution. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Dependent variable	High number of brokers [1, 5]							
	Positive news				Negative news			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pilot × Program Period	0.005	0.004	0.005	0.004	0.038	0.037	0.030	0.030
	(0.360)	(0.302)	(0.326)	(0.258)	(1.508)	(1.494)	(1.234)	(1.226)
Return Volatility		0.024***	0.023***	0.027***		0.022	0.034**	0.040***
		(2.984)	(2.946)	(3.418)		(1.576)	(2.580)	(2.852)
Market Cap			0.222***	0.218***			0.273***	0.263***
			(13.872)	(12.883)			(11.954)	(11.004)
Amihud Illiquidity				0.016***				0.003
				(2.758)				(0.411)
Bid-Ask spread				-0.016*				-0.020
				(-1.954)				(-1.487)
Number of Analysts				0.004				0.020**
				(0.725)				(2.258)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,837	22,837	22,837	22,837	9,128	9,128	9,128	9,128
R-squared	0.383	0.383	0.390	0.391	0.478	0.479	0.489	0.490

Table B.10: Further Evidence. This table reports statistics for difference-in-differences analysis of our main dependent variables when different samples are used. The sample of Panel A consists of sell trades and the regression is run around the period of application of the actual Reg SHO Pilot Program. Panel B and C, and D consider buy trades - for all variables excluding price informativeness - and set Pilot Period equal to 1 in dates not covered by the actual Reg SHO program. In Panel B and columns (1)-(8) of Panel D, Pilot Period is set to one in the period July 2002-July 2004, i.e., just before the Reg SHO Pilot Period was announced (July 28, 2004), and the sample is from January 1999 to July 2004. In Panel C and columns (9)-(16) of Panel D, Pilot Period is set to one in the period November 2012-December 2014, and the sample is from November 2010, i.e. right after the reintroduction of the uptick rule on November 10, 2010 to November 2014. The set of control variables comprises market capitalization, previous year return standard deviation, Amihud illiquidity, number of analysts following the company, and previous year average bid-ask spread. We report results for positive and negative news, separately. Following Della Vigna and Pollet (2009), we define the earnings surprise (news) as the difference between the actual earnings figure and the consensus forecast, scaled by the stock price 5 trading days before the announcement. Standard errors are clustered at the stock and time level, and t-statistics are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The control variables are standardized.

Panel A	Sell trades											
Dependent variable	Trade size (Dollars)		Trade size (Shares)		High n. Of brokers		Broker centrality		Broker familiarity		Log-dollar volume	
News	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pilot × Program Period	-0.031	-0.041	-0.023	-0.051	0.004	-0.010	0.014	0.031	0.001	-0.016	0.045	-0.277
	(-0.942)	(-0.592)	(-0.668)	(-0.756)	(0.153)	(-0.222)	(1.117)	(1.418)	(0.105)	(-1.292)	(0.265)	(-0.922)
Return Volatility	0.034	-0.039	0.034	-0.053	0.058***	0.033	0.021***	0.020	0.002	0.008	0.176*	0.017
	(1.548)	(-0.921)	(1.475)	(-1.276)	(3.416)	(1.130)	(2.587)	(1.450)	(0.347)	(1.051)	(1.676)	(0.087)
Market Cap	1.037***	1.167***	-0.040	0.134*	1.329***	1.239***	0.208***	0.185***	-0.027***	-0.016	-1.191***	-1.792***
	(22.225)	(16.801)	(-0.836)	(1.928)	(37.343)	(25.471)	(12.493)	(7.359)	(-3.067)	(-1.124)	(-5.586)	(-5.409)
Amihud Illiquidity	0.022**	0.025	-0.003	-0.012	0.017***	0.007	-0.002	-0.011*	0.002	-0.004	-0.068	-0.187***
	(2.483)	(1.235)	(-0.362)	(-0.636)	(2.907)	(0.598)	(-0.627)	(-1.679)	(0.691)	(-1.085)	(-1.168)	(-2.918)
Bid-Ask spread	0.107***	0.061	0.112***	0.096**	0.061***	0.039	-0.001	-0.006	-0.009**	-0.001	-0.389***	-0.299*
	(5.208)	(1.351)	(4.231)	(2.150)	(3.433)	(1.322)	(-0.109)	(-0.449)	(-2.170)	(-0.145)	(-4.684)	(-1.759)
Number of Analysts	0.041***	0.053**	0.054***	0.066**	0.029***	0.025	0.012***	0.011	-0.002	-0.010**	-0.118**	-0.068
	(3.178)	(1.997)	(4.294)	(2.441)	(2.966)	(1.446)	(2.608)	(1.261)	(-0.592)	(-1.987)	(-2.033)	(-0.694)
Surprise	0.001	-0.014	-0.021	-0.018	0.016**	-0.018*	-0.002	-0.003	0.000	0.006**	0.049	0.068
	(0.089)	(-0.774)	(-1.421)	(-1.006)	(2.117)	(-1.750)	(-0.567)	(-0.481)	(0.198)	(1.996)	(1.079)	(1.370)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,133	8,667	22,133	8,667	22,133	8,667	22,133	8,667	22,133	8,667	22,133	8,667
R-squared	0.458	0.532	0.465	0.589	0.745	0.770	0.412	0.510	0.182	0.349	0.302	0.443

Panel B	Before announcement of Reg-SHO Program: Price efficiency and trade speed											
Dependent variable	CAR Ratio		Variance Ratio		Cross-correlation		Trading speed		Trade size (Dollars)		Trade size (Shares)	
News	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pilot × Program Period	0.000	-0.069	0.015	0.111	0.101	0.678	0.004	0.005	0.004	-0.020	-0.017	-0.048
	(0.008)	(-0.941)	(0.212)	(0.855)	(0.836)	(1.480)	(0.406)	(0.285)	(0.126)	(-0.243)	(-0.532)	(-0.560)
Return Volatility	-0.009	-0.004	0.084	0.028	-0.082	-0.386	0.002	0.003	0.062***	0.062	0.070***	0.116**
	(-0.421)	(-0.079)	(1.377)	(0.278)	(-1.078)	(-1.130)	(0.439)	(0.223)	(2.759)	(1.105)	(3.115)	(1.995)
Market Cap	0.071**	-0.186***	-0.299***	-0.033	-0.330***	-1.659**	-0.031***	-0.086***	0.904***	0.614***	-0.361***	-0.547***
	(2.522)	(-2.695)	(-4.434)	(-0.214)	(-2.718)	(-2.015)	(-3.811)	(-5.010)	(27.364)	(7.822)	(-11.324)	(-6.914)
Amihud Illiquidity	0.003	0.007	0.010	-0.061	-0.039	-0.739	0.011***	0.004	0.000	0.065	-0.009	0.045
	(0.734)	(0.088)	(0.699)	(-0.624)	(-1.045)	(-1.535)	(3.862)	(0.411)	(0.016)	(1.141)	(-0.703)	(0.911)
Bid-Ask spread	-0.000	0.026	0.044	-0.169*	-0.048	-0.152	-0.003	-0.010	0.012	-0.083	-0.005	-0.099*
	(-0.021)	(0.531)	(1.008)	(-1.702)	(-0.722)	(-0.544)	(-0.561)	(-0.837)	(0.550)	(-1.502)	(-0.228)	(-1.763)
Number of Analysts	0.016	0.031	0.103***	0.058	0.080*	0.094	0.006*	0.001	0.025**	0.005	0.028**	0.023
	(1.293)	(1.056)	(4.002)	(0.944)	(1.931)	(0.405)	(1.801)	(0.138)	(2.235)	(0.133)	(2.561)	(0.610)
Surprise	-0.030**	-0.000	0.007	-0.172***	-0.014	-0.145	-0.007**	0.005	0.005	-0.042**	0.010	-0.053***
	(-2.048)	(-0.032)	(0.303)	(-3.180)	(-0.472)	(-0.803)	(-2.032)	(1.214)	(0.446)	(-1.993)	(0.840)	(-2.647)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,948	1,375	5,948	1,375	5,948	1,375	19,749	6,252	19,751	6,658	19,751	6,658
R-squared	0.434	0.643	0.372	0.634	0.466	0.684	0.223	0.425	0.535	0.492	0.537	0.577

Panel C	After reintroduction of uptick rule: Price efficiency and trade speed											
Dependent variable	CAR Ratio		Variance Ratio		Cross-correlation		Trading speed		Trade size (Dollars)		Trade size (Shares)	
News	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pilot × Program Period	0.016	-0.068	-0.061	0.833	-0.151	0.051	0.005	0.010	-0.056	-0.031	-0.056	-0.027
	(0.507)	(-1.154)	(-0.646)	(1.197)	(-1.591)	(0.217)	(0.503)	(0.545)	(-1.283)	(-0.350)	(-1.245)	(-0.309)
Return Volatility	0.048**	-0.000	0.050	-0.586	-0.014	0.157	-0.002	0.018	0.002	0.116*	0.008	0.104*
	(2.047)	(-0.007)	(0.796)	(-1.648)	(-0.187)	(1.045)	(-0.320)	(1.393)	(0.069)	(1.842)	(0.264)	(1.746)
Market Cap	-0.007	-0.336***	0.241	-1.578	-0.126	-0.967**	0.000	-0.077***	1.510***	1.471***	0.203**	0.177
	(-0.128)	(-3.880)	(1.424)	(-1.206)	(-0.663)	(-2.255)	(0.024)	(-2.766)	(17.686)	(9.946)	(2.389)	(1.252)
Amihud Illiquidity	-0.018**	-0.030	-0.000	0.002	0.006	-0.023	0.002	-0.004	0.019***	0.065***	0.018**	0.066**
	(-2.071)	(-0.531)	(-0.026)	(0.018)	(0.493)	(-0.337)	(0.685)	(-0.732)	(2.816)	(2.731)	(2.514)	(2.420)
Bid-Ask spread	-0.001	0.018	0.031	0.631	0.038	-0.118	0.010	-0.029**	0.089***	0.027	0.105***	0.069
	(-0.044)	(0.448)	(0.353)	(1.226)	(0.428)	(-0.702)	(1.314)	(-2.290)	(2.835)	(0.449)	(3.453)	(1.198)
Number of Analysts	0.012	0.054**	0.025	0.028	0.009	-0.143*	0.003	0.004	0.032*	0.049	0.025	0.067*
	(0.875)	(2.353)	(0.521)	(0.269)	(0.184)	(-1.742)	(0.582)	(0.460)	(1.695)	(1.301)	(1.356)	(1.775)
Surprise	-0.046***	0.017	-0.000	-0.018	0.006	-0.036	-0.004	-0.003	-0.005	-0.005	-0.002	0.002
	(-4.104)	(1.262)	(-0.000)	(-0.447)	(0.138)	(-0.790)	(-1.256)	(-0.672)	(-0.388)	(-0.216)	(-0.187)	(0.084)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,564	1,799	4,564	1,799	4,564	1,799	13,333	5,905	13,333	5,982	13,333	5,982
R-squared	0.433	0.566	0.473	0.570	0.529	0.647	0.246	0.372	0.629	0.642	0.547	0.590

Panel D	Other variables															
	Before announcement of Reg-SHO Program								After reintroduction of uptick rule							
Dependent variable	High n. Of brokers		Broker centrality		Broker familiarity		Log-dollar volume		High n. Of brokers		Broker centrality		Broker familiarity		Log-dollar volume	
News	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Pilot × Program Period	0.016	0.034	0.012	-0.015	-0.009	0.001	-0.005	-0.046	-0.047	-0.035	-0.015	-0.009	-0.008	-0.056**	0.069	0.409
	(0.510)	(0.629)	(1.300)	(-0.817)	(-0.560)	(0.056)	(-0.053)	(-0.206)	(-1.448)	(-0.666)	(-0.786)	(-0.260)	(-0.576)	(-2.237)	(0.312)	(1.053)
Return Volatility	0.032	0.036	0.002	-0.014	0.004	-0.016	0.178***	0.047	0.002	0.053	0.011	0.029	0.005	-0.006	0.192	-0.129
	(1.626)	(1.017)	(0.249)	(-1.299)	(0.435)	(-0.866)	(2.993)	(0.359)	(0.123)	(1.496)	(0.852)	(1.236)	(0.414)	(-0.286)	(1.203)	(-0.564)
Market Cap	1.150***	1.027***	-0.023**	-0.034**	0.085***	0.066***	0.472***	0.534***	1.071***	1.170***	-0.157***	-0.269***	0.103***	0.136***	-2.527***	-3.573***
	(38.142)	(20.296)	(-2.572)	(-2.193)	(6.043)	(2.610)	(5.755)	(3.069)	(18.136)	(15.252)	(-4.679)	(-6.445)	(3.691)	(3.252)	(-6.178)	(-7.226)
Amihud Illiquidity	0.032***	0.038	0.006	-0.008	0.007	0.026**	-0.034	-0.095	0.001	0.014	-0.004	0.010*	-0.010*	-0.038***	-0.064	0.043
	(2.670)	(1.478)	(1.613)	(-0.822)	(1.471)	(2.180)	(-1.298)	(-0.614)	(0.243)	(1.229)	(-0.807)	(1.742)	(-1.765)	(-6.596)	(-0.905)	(0.590)
Bid-Ask spread	-0.032	-0.009	-0.008	0.006	-0.024**	0.001	0.293***	0.218*	0.044*	0.025	-0.022*	0.002	-0.023*	-0.020	-0.353**	-0.241
	(-1.520)	(-0.203)	(-1.610)	(0.684)	(-2.341)	(0.053)	(5.912)	(1.942)	(1.947)	(0.728)	(-1.703)	(0.086)	(-1.873)	(-1.207)	(-2.495)	(-1.257)
Number of Analysts	0.034***	0.029	-0.004	-0.003	0.008	0.005	-0.122***	0.033	0.011	0.012	-0.010	-0.042***	-0.003	-0.007	-0.169*	-0.293*
	(3.623)	(1.463)	(-1.090)	(-0.413)	(1.472)	(0.508)	(-3.583)	(0.415)	(0.908)	(0.575)	(-1.200)	(-3.195)	(-0.532)	(-0.617)	(-1.894)	(-1.879)
Surprise	0.017**	-0.023**	0.002	-0.006	-0.001	0.002	0.002	-0.077	0.004	0.004	0.009***	-0.002	0.004	0.002	0.038	-0.002
	(2.262)	(-2.016)	(0.643)	(-1.117)	(-0.196)	(0.407)	(0.064)	(-1.267)	(0.756)	(0.327)	(2.714)	(-0.298)	(1.265)	(0.275)	(0.838)	(-0.038)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,749	6,252	19,749	6,252	19,749	6,252	19,749	6,252	13,333	5,905	13,333	5,905	13,333	5,905	13,333	5,905
R-squared	0.764	0.805	0.222	0.404	0.359	0.497	0.342	0.479	0.785	0.811	0.312	0.444	0.340	0.452	0.406	0.506

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