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

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ABSTRACT

This paper contributes to the recent debate on whether nonfundamental price dislocations affect real economic activities, using a novel and economically-grounded approach. Hurricanes create liquidity demand from investors living in disaster zones. This translates into additional outflows for mutual funds in the areas affected by hurricanes of about \$2.5 billions. Such outflows cause fire sales, which are followed by temporary price dislocations in stocks unrelated to the natural disaster (-7% reverted within 10 months). The nonfundamental price drop induces firms to reduce investments by 4%. These results indicate that when the source of outflows is identified ex-ante and stems from investors' liquidity needs unrelated to fund performance, the resulting nonfundamental price dislocations actually distort firms' real decisions.

Key words: Real effects of finance, Hurricanes, fire sales.

JEL classification: G14, G31, G23, Q54.

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I 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. Our findings have novel important implications, in light of the recent debate on whether nonfundamental shocks to prices can actually affect firms' real decisions.

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. These transitory price declines alter various firms' decisions, such as investment, capital structure, takeover activity, or governance mechanisms[2]. Yet, these conclusions have been recently challenged (Berger 2019, Wardlaw 2020), because the 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 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

[1]More recently, van Binsbergen 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).

[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.

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 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 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

[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.

price declines following hurricane events. Second, firms respond to these price dislocations by reducing investment. Our results are robust to several tests addressing the concerns raised on the traditional measure of mutual fund pressure. 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. Taken together, these results 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 non-

[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.

fundamental 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 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. While these results might be consistent with many non-mutually exclusive mechanisms discussed by previous literature (e.g. learning, corporate governance, financial constraints), studying the channel through which firm managers respond to nonfundamental price drops following a hurricane is beyond the scope of this paper[8].

This paper primarily contributes to the literature on the real effects of secondary financial markets[9] and fire sales[10] 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.

[8]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.

[9]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)

[10]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).

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[11]. 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 II describes the data used in the analysis, while Section III discusses the identification strategy. Section IV focuses on showing that mutual funds have a significant local clientele. The impact of hurricanes on mutual fund flows is studied in Section V, while the effects on prices and real decisions are described in Sections VI and VII, respectively. Section VIII concludes.

II 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 A1 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 I reports summary statistics for the main variables of interest. Figure I 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 in few

[11]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).

areas only, then the presence of a local clientele would have been utterly unlikely, as the investor base is broadly dispersed.

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 I.

FigureII 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[12]. 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 A2 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 III 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 III that the portion of US mainland hit by hurricane

[12]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).

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[13] in terms of Core-Based Statistical Area (CBSA). CRSP Mutual Fund and 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.

III 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

[13] 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 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.

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)[14]. 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:

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

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 2019).

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,

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

Goldstein, and Jiang (2012) and takes into account the caveats of Wardlaw (2020). The definition is as follows:

$$HIF_{i,t} = \sum_{j=1}^{m} \frac{flow_{j,t} \times Holdings_{i,j,t-1}}{Shares\ Outstanding_{i,t-1}} \times \mathbb{1}\{(Location_{j,t} = l)\,\&\,(flow_{j,t} < 0)\}\,, \tag{2}$$

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,

$$HISF_{i,t} = \sum_{j=1}^{m} \frac{surprise_{j,t} \times Holdings_{i,j,t-1}}{Shares\ Outstanding_{i,t-1}} \times \mathbb{1}\{(Location_{j,t} = l)\}\,. \tag{3}$$

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

Equation (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). 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 A3 regresses the quarterly realizations of the variable in equation (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.

IV 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 II 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[15].

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 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.

[15]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.

Table III 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 III 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 A4. 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.

V 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:

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

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 IV. 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[16], 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.

[16] 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.

Table IV 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[17]. On the other hand, these outflows 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 IV, but that includes individual quarters dummies. Results of this exercise are shown in Table A5. 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 A2 (\$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 A6, 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 IV.

We proxy local clientele in two ways. First, we look at the correlation of outflows

[17] 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.

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 A7. 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 A8 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 A9 and A10 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.

VI 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 (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 III. The dependent variable is the monthly stock abnormal return, calculated using the Daniel, Grinblatt, Titman, and Wermers (1997) benchmark[18]. The main explanatory variables are treated dummies interacted with time indicators. For consistency with Table IV, 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).

[18]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.

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 V, 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 IV, that plots the cumulative coefficients of a regression similar to that in Table V 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 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.

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 A11. 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 V 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 V. Therefore, to avoid confounding effects, we exclude fund-stock-quarter information for stocks that are affected by the hurricane[19]. The control variables are the fund and stock characteristics used throughout the paper. Table A12

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

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 V using the two measures described in equations (2) and (3), and define treated those stocks that display negative values of those variables[20] Table A13 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 IV and Table V 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 VI. 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 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

[20]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.

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 V hold in a subsample where firms with low or zero institutional ownership are excluded. Results are reported in Table A14, 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 V and Table A14, we stress that only 4% (10%) of the sample have zero (less than 1%) institutional ownership[21]. Most importantly, Table A14 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 VII, 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.

VII The real effects of hurricane induced flows

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.

Addressing the question of whether nonfudamental price dislocations affect real economic activities requires the use of an instrumental variable approach, because using a

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

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). 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 this 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 VI. 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% 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[22]. 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 A15, 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 (2) and (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 VII and show that the hypothesis of a negative effect of *Hurricane Hypothetical Sales* onto investments is generally confirmed even in this more tight sample.

VIII 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

[22]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.

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.

Overall, our results contribute to the debate as they imply that stock price inefficiencies, when correctly isolated, actually affect real decisions. These results might be consistent with multiple and non-mutually exclusive mechanisms through which firms' managers respond to the nonfundamental shock. While the literature proposed a series of channels (e.g., learning, catering, and financial constraints), future extensions should study what induces firms' managers to respond to temporary price dislocations in this setting.

Moreover, further 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.

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Tables

Table I: 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 II. 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 A1.

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 II: 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 III: 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 IV: 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 V: 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 III. 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 VI: 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 V. 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.

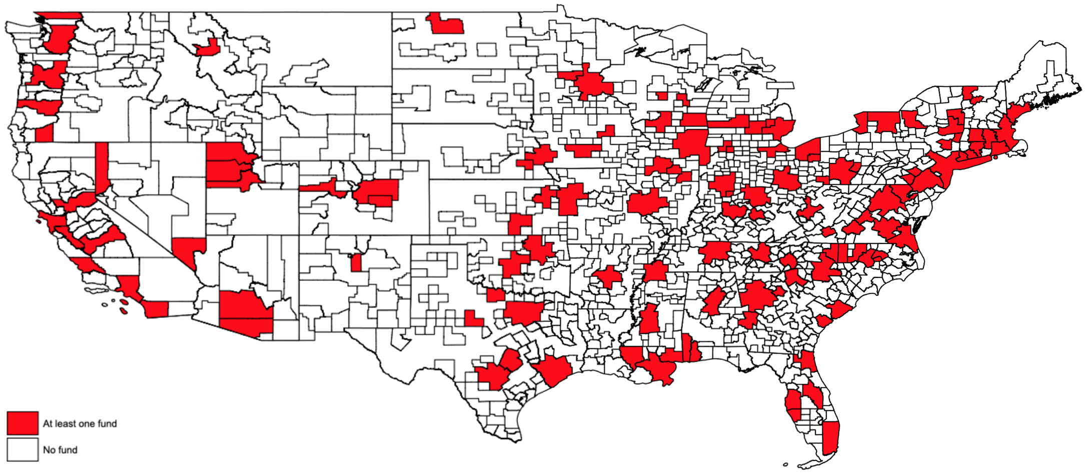
Panel A	Firm, Time, Location FE					
Dependent variable	Capex/PPE					
	IV	1st Stage	RF	IV	1st Stage	RF
	(1)	(2)	(3)	(4)	(5)	(6)
Q	0.621***			0.644***		
	(4.235)			(3.548)		
HHS		-0.036***	-0.022***		-0.032***	-0.021***
		(-4.869)	(-5.784)		(-4.982)	(-5.094)
Cash Flow	0.224***	-0.197***	0.101***	0.228***	-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

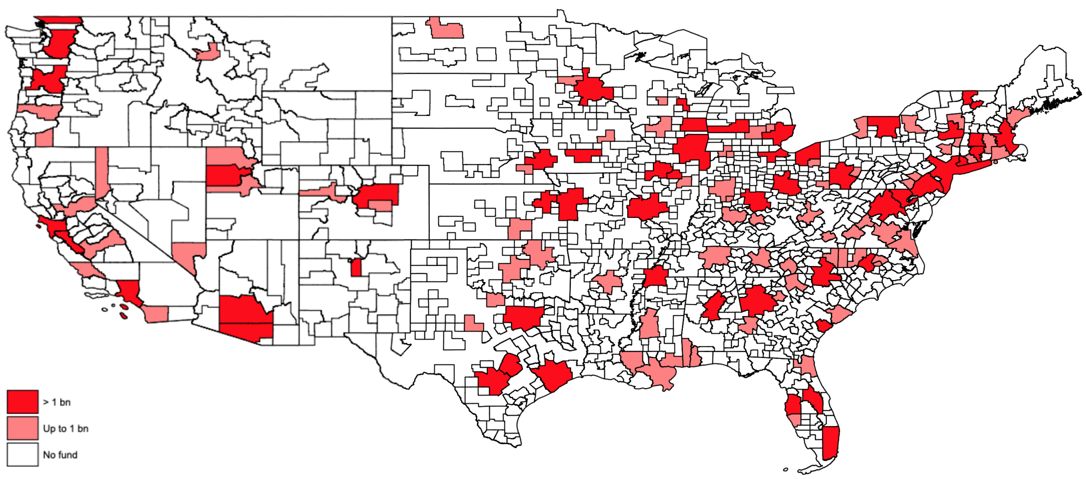
Table VII: Hurricanes and real effects: homogeneous sample. This table reports estimates of the real effects of hurricanes on firms unrelated to the natural events. The model is the IV regression used in Table VI, but the sample comprises stocks that have non-zero value of the instrument *HHS* at least 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

Figures

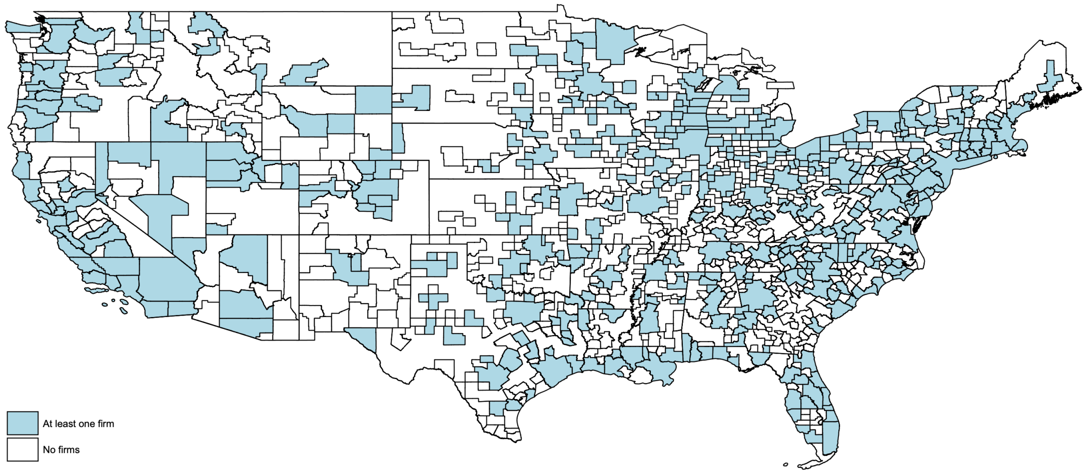


(a) *CBSAs with at least one fund*

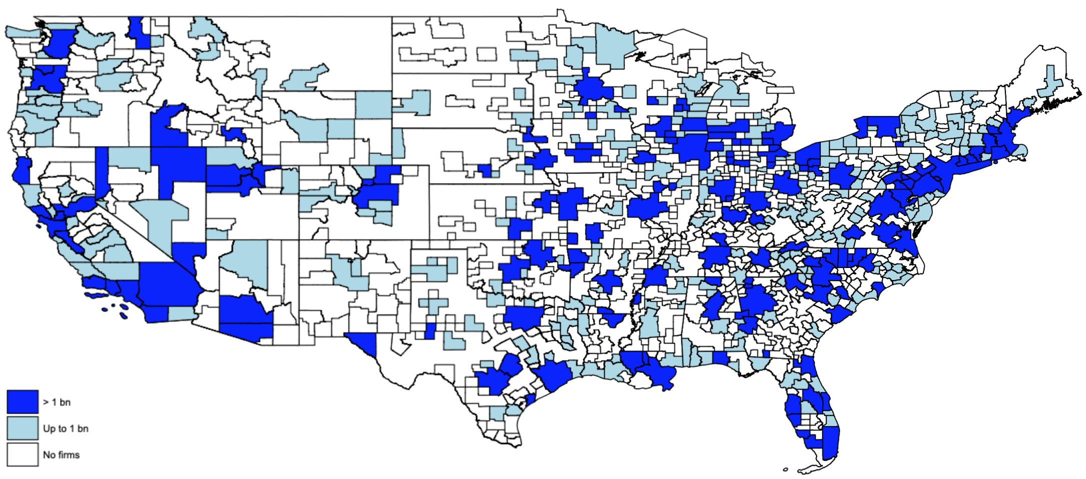


(b) *Total TNAs by CBSA*

Figure I: 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.

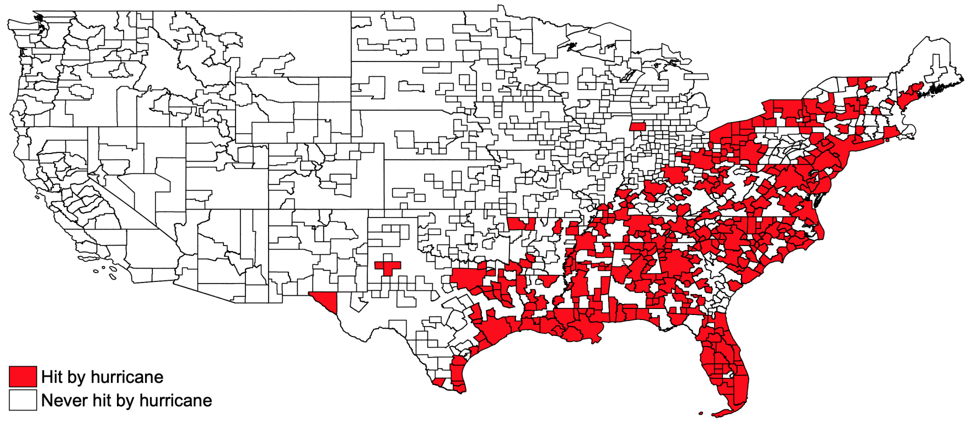


(a) *CBSAs with at least one firm*

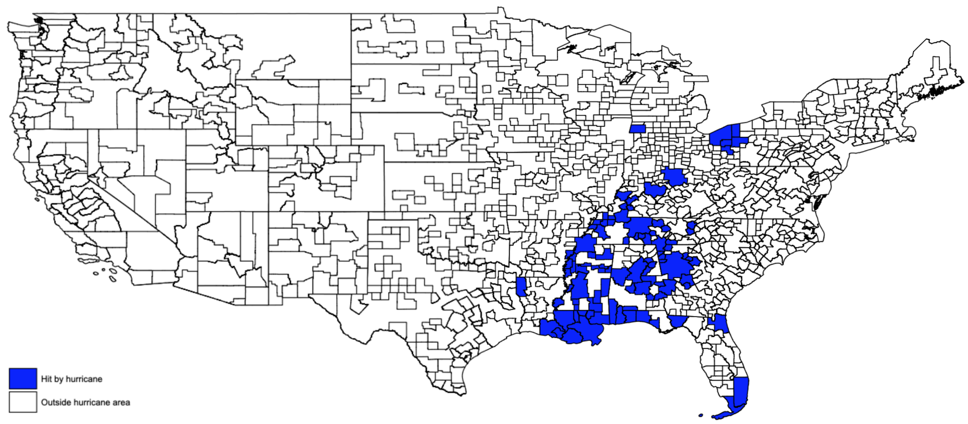


(b) *Total market cap by CBSA*

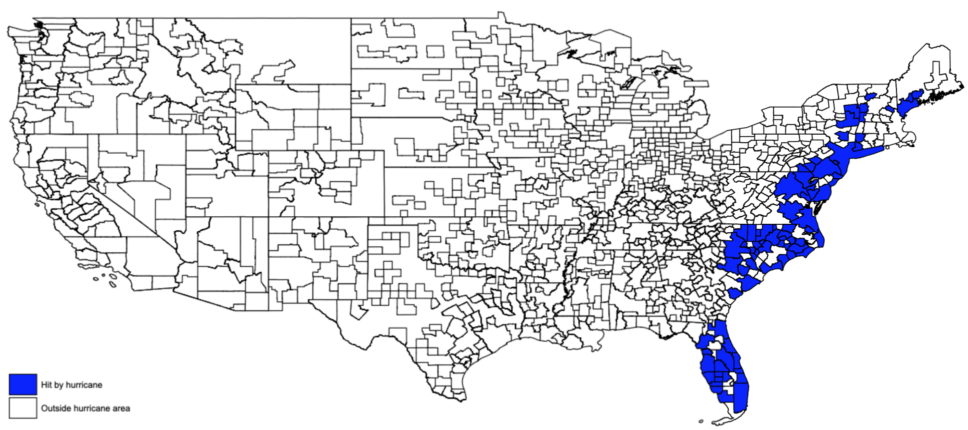
Figure II: Geographic distribution of firms. This figure reports the geographic distribution of the sample of firms. In Panel A, CBSAs with at least one firms are shown in light blue, while in Panel B darker color indicates higher total total market cap in 2020 billion of dollars in a CBSA.



(a) *Counties hit at least once*



(b) *Hurricane Katrina (2005)*



(c) *Hurricane Floyd (1999)*

Figure III: Localization of hurricanes. This figure displays the localization of the hurricanes in our sample. Panel A reports in red the counties 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).

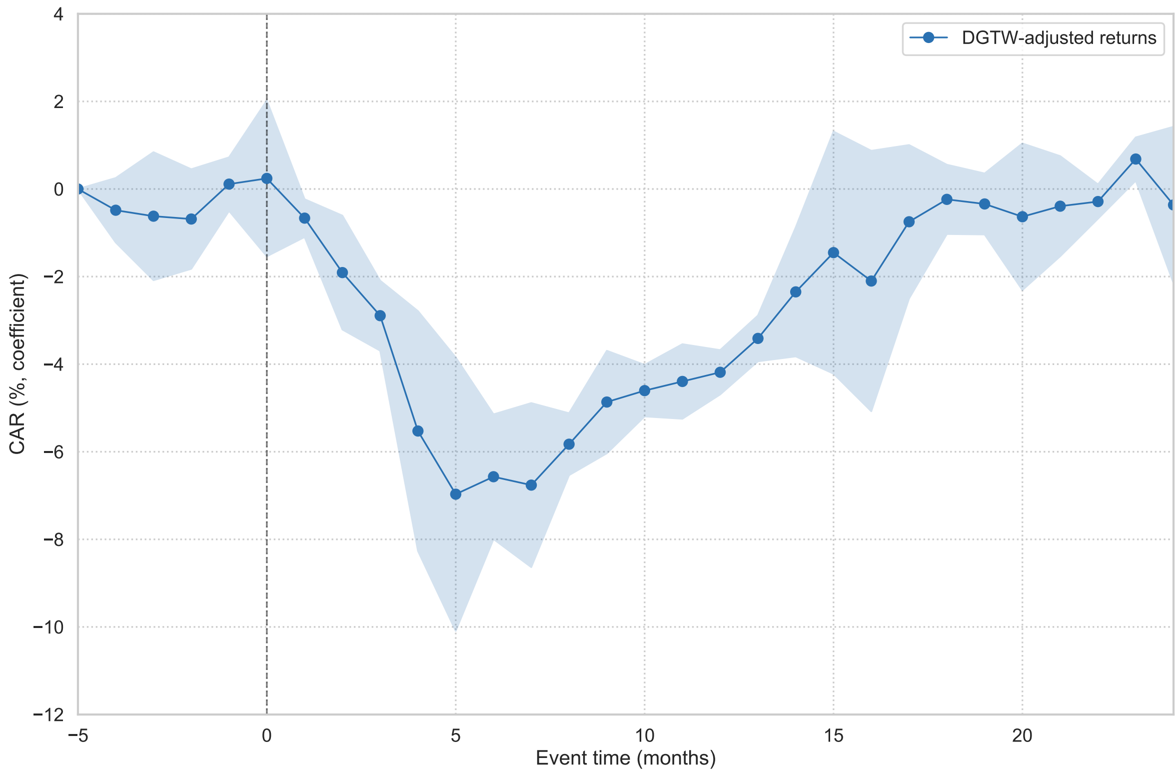
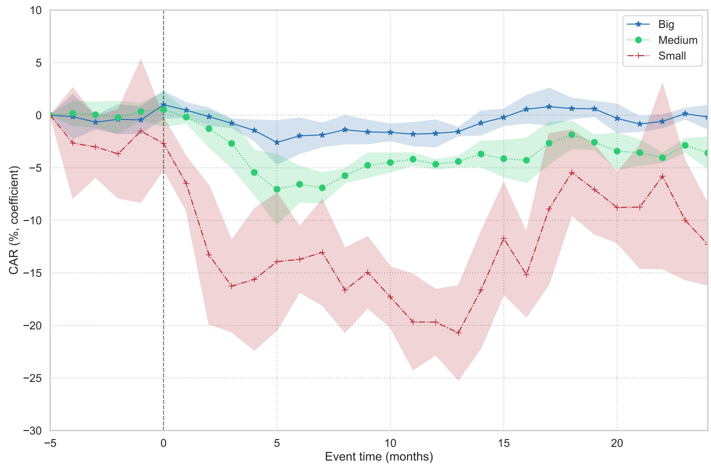


Figure IV: Hurricanes and stock returns This figure displays cumulative coefficients of a difference-in-differences model where the dependent variable is the monthly DGTW-adjusted stock return and the treated group is made of stocks unrelated to the hurricane but held by mutual funds headquartered in 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.

Cross-section: Size



Cross-section: Liquidity

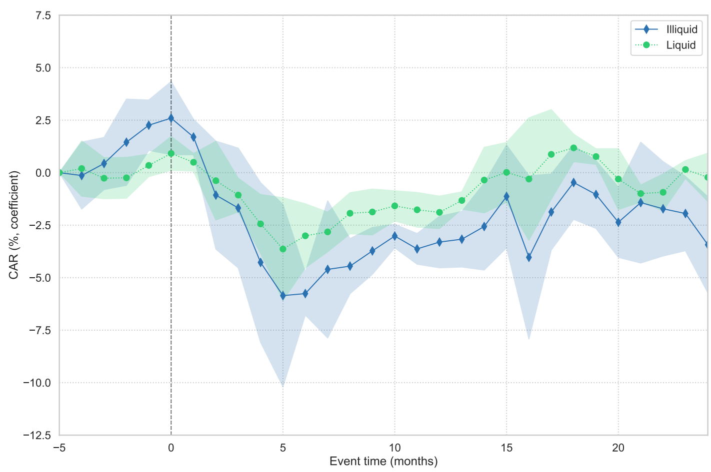
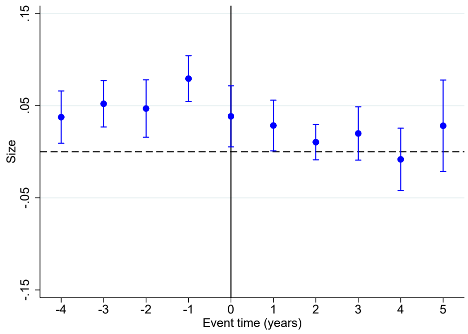
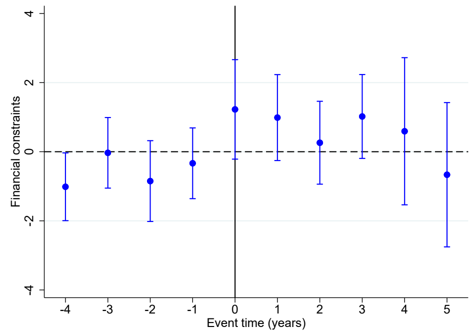


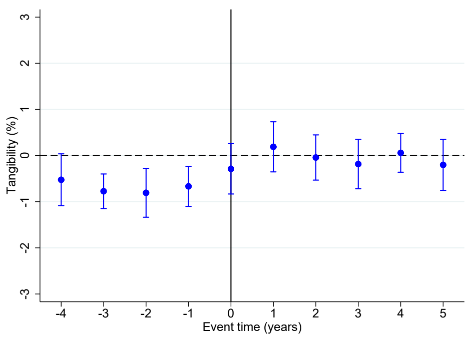
Figure V: Hurricanes and stock returns: cross-sectional analysis. This figure displays cumulative coefficients of a difference-in-differences model where the dependent variable is the monthly DGTW-adjusted stock return and the treated group is made of stocks unrelated to the hurricane but held by mutual 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.



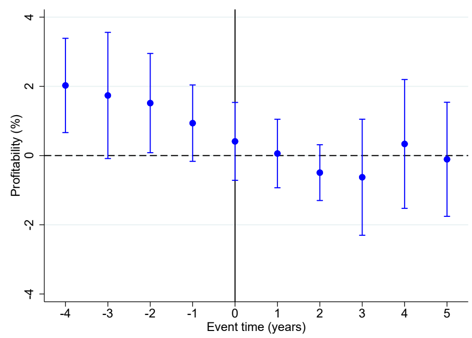
(a) *Size*



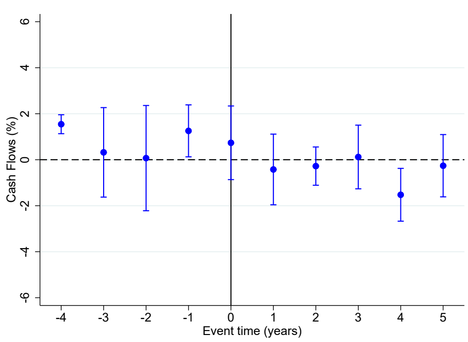
(b) *Financial Constraints*



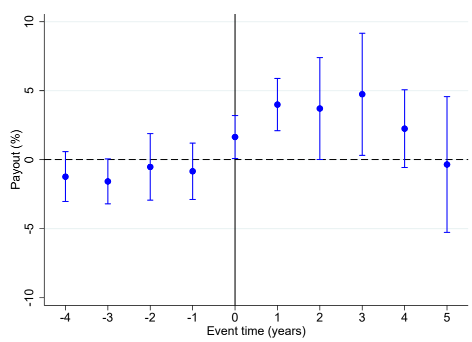
(c) *Tangibility*



(d) *Profitability*

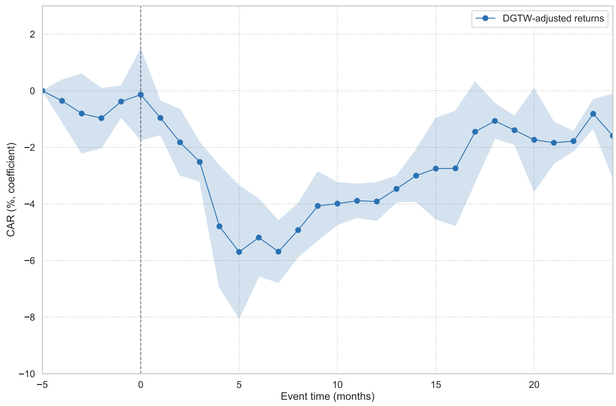


(e) *Cash Flow*

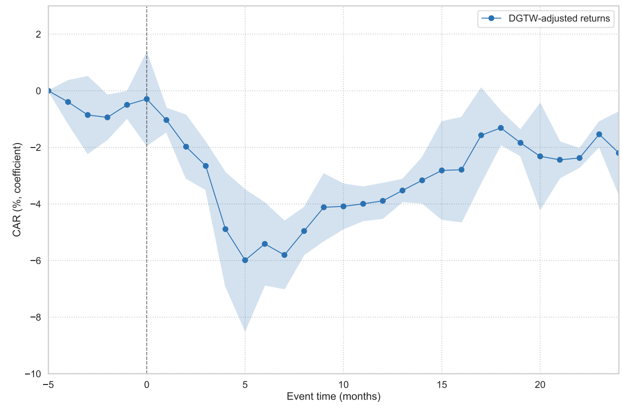


(f) *Payout ratio*

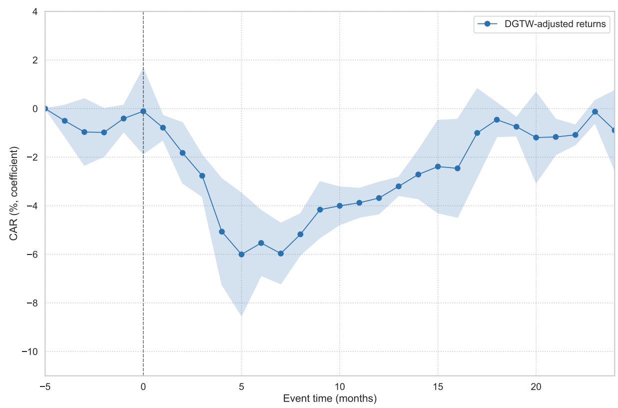
Figure VI: Firm characteristics. This figure displays difference in differences coefficients for several firm characteristics around a hurricane event. Panel (a) shows results where the dependent variable is the log-size, while estimates for financial constraints (Kaplan and Zingales (1997) index) computed 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) and (f) report results where the dependent variable is cash-flow or total payout ratio. Payout ratio is the sum of total dividends (dvp+dvc) and purchase of common and preferred stock (*prstkc*) divided by operating income before depreciation. 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 VII: 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.

Appendix

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 III, 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[23]. 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.

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

Tables

Table A1: 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)
Hurricane-Induced Flow (HIF)	See eq. (2)
Hurricane-Induced Surprise Flow (HISF)	See eq. (3)
CBSA-level variables	
Unemployement rate	From the Bureau of Labor Statistics
House Price Index (HPI)	From FRED database (variable ATNHPIUS)

Table A2: 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 III.

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 A3: 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 A4: 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 A5: 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 IV 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 A6: 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 A7: 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 A8: 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 A9: 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 A10: 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 A11: 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 A12: 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 III. 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 A13: Hurricanes and stock returns: robustness This table reports results for a difference-in-differences regression equal to that used in Table A5, but using firms with negative values of the measures in equations 2 and 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 A14: Hurricane and stock returns: stocks with institutional ownership. This table reports results for a difference-in-differences regression equal to that in table V 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 A15: Real effects: robustness This table reports results for an IV-regressions equal to that used in Table VI, 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 2 (Panel B), or *HISF* from equation 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