Disastrous Selling Decisions:
The Disposition Effect and Natural Disasters*

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Abstract: Combining county-level natural disaster data with individual investor transactions, I document an increased disposition effect for investors impacted by a natural disaster. This effect is increasing in disaster severity and decreasing in the length of time following the event, suggesting that extreme natural disasters can significantly influence investor behavior, especially in the short term. These findings are not explained by liquidity constraints, tax incentives, or informed trading. The effect strengthens with local stocks and investors' duration at their residence. Moreover, the increased disposition effect of disaster-affected investors is consistent with investors deriving utility from environmental damages and realized gains/losses.

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1. Introduction

First introduced to the finance literature by Shefrin and Statman (1985), the disposition effect is the tendency for investors to be more eager to liquidate stocks that are at a gain compared to stocks that are at a loss. Since then, the disposition effect has been well-documented in a myriad of investors.[1] Odean (1998) shows the effect is particularly strong for US retail investors and is unexplained by tax considerations, informed trading, and portfolio rebalancing. Instead, most theoretical explanations for the disposition effect rely on investors having Kahneman and Tversky's (1979, 1992) prospect theory preferences.[2]

Recently, An et al. (2019) find robust empirical evidence that the disposition effect increases dramatically when an investor's other holdings are performing poorly. One explanation they propose is that investors derive utility from both realized and unrealized gains/losses. While many economists have incorporated preferences assuming investors derive utility from paper and realized gains/losses[3], I extend this analysis beyond an investor's portfolio. Instead, I use per capita natural disaster damages to ask a simple question: does an investor's choice to sell securities depend on utility from sources outside the investor's portfolio?

In this setting, natural disaster exposure could be a significant paper and/or realized loss in the form of financial impacts and psychological well-being. In the case of individual-specific damages to the investor (such as property damages), a meaningful amount may be realized at the time of the event. Yet, if the disaster was especially impactful to the community as a whole, it is likely that housing values may depreciate in the short-run, indicating a paper loss to the investor (assuming she does not

[1] See Grinblatt and Keloharju (2001), Shapira and Venezia (2001), Genesove and Mayer (2001), and Heath, Huddart, and Lang (1999) among others.

[2] See Barberis and Xiong (2009).

[3] For models incorporating preferences over paper gains/losses, see Barberis and Huang (2001), Barberis, Huang, and Santos (2001), and Barberis and Xiong (2009). For models incorporating preferences over realized gains/losses, see Barberis and Xiong (2009), Barberis and Xiong (2012), Henderson (2012), and Ingersoll and Jin (2013).

immediately sell her affected property). If investors receive a "burst" of utility when they realize gains (Barberis and Xiong, 2012; Henderson, 2012; Ingersoll and Jin, 2013; Frydman et. al., 2014), then they might be especially in need of that burst following a natural disaster. Ex-ante, it is entirely possible that disaster-affected investors could trade significantly less due to inattention, and the difference between their propensity to sell gains and losses may be insignificant. Thus, their disposition effect could be reduced if they stop actively investing. However, based on the need for disaster-affected investors to seek a positive burst of utility, I hypothesize an increased disposition effect for disaster-affected individuals.

Additionally, if investors attempt to offset disutility caused by their environment, then the disposition effect should increase with disaster severity. Moreover, the increase should be strongest immediately following a natural disaster and diminish over time. These relationships are precisely what I find in the data. For the most extreme disasters, the disposition effect increases 51-129% in the year following the event, while more moderate disasters only increase the disposition effect by 2-13%.

I document the relationship between natural disasters and the disposition effect in univariate tests as well as regressions with several fixed effects controls. First, I show with simple dummy tests that *any* level of disaster damage is associated with a larger disposition effect. However, this relationship does not remain statistically significant after clustering standard errors to account for correlations within account, within stock, and within date. Next, I test if the disposition effect is positively related to the per capita damage estimates, and I find statistically significant evidence (at the 1% level) of a positive relation within two years following an event, even after controlling for account, stock, and date fixed effects (as well as clustering standard errors across these three dimensions). Then, I divide the sample into four mutually exclusive cohorts based on disaster severity: *None*, *Moderate*, *Severe*, and *Extreme*. Across each of these cohorts, I document a monotonic increase of the disposition effect. Moreover, when controlling for account, stock, and date fixed effects, the *Extreme* group is associated with a staggering 96% (t-stat 3.91) increase in the disposition effect over the group with no disaster impacts.

To account for changes in the disposition effect across time (An et al., 2019; Bernard, Loos, and Weber, 2018) and differences in regional selling behavior, I implement a difference-in-difference approach matching affected accounts to unaffected accounts that reside within the same state. First, I create a panel of only the accounts impacted by an extreme disaster and define them as the treatment group. I then match these accounts to other accounts that reside in the same state that were not affected by a natural disaster. Those other accounts are defined as the control group to reduce any confounding effects from regional differences in selling behaviors. This structure allows me to measure the difference in the disposition effect within accounts while taking out any time-varying impacts on the disposition effect. In comparing the year before and after the event of the treatment and control groups, I find that the disposition effect for the treatment (control) group increases (decreases) by 129% (30%). Moreover, the difference between the treatment and control groups remains economically and statistically significant at the 5% level for all specifications, even when controlling for account, stock, and date fixed effects.

Next, I consider potential mechanisms for the increased disposition effect of disaster-affected investors. The findings indicate the increase is not driven by liquidity constraints, tax incentives, or informed trading. In fact, the subsequent performance of investor-stocks driving the result indicates the disposition effect of extreme disaster-affected individuals costs them -10.6% (t-stat -2.21) in future market-adjusted returns annually.[4] Instead, I find that the increased disposition effect for disaster-affected investors strengthens significantly when those investors trade local stocks and they have lived at their residence for at least 10 years.

[4] The result is robust to using a Daniel, Grinblatt, Titman, and Wermers (1997) matched portfolio, although the annual magnitude lessens slightly to -5.8% (t-stat -1.86).

Based on these findings, I hypothesize that investors receive a negative utility shock via their environment, and subsequently, increase their disposition effect to offset this random event. Essentially, the marginal utility that investors receive from the disposition effect varies based on external, individual-specific events. Moreover, a natural disaster enables a loss-averse investor to receive (lose) more marginal utility from realizing a gain (loss). Thus, a larger disposition effect occurs for disaster-affected investors.[5]

Frydman, Hartzmark, and Solomon (2018) find that reinvesting a sale into a different stock allows the investor to keep her mental account open. In accordance with this logic, I find that investors are most (least) likely to reinvest the proceeds from the sale of a loss (gain) following a natural disaster. Thus, reinvesting proceeds after realizing a loss allows an investor to lessen the disutility received from the sale. Similarly, holding a gain in cash allows for a more lasting burst of positive utility. Together, the evidence is most consistent with investors deriving utility from negative shocks outside their portfolio and subsequently exhibiting the disposition effect on their holdings to garner offsetting positive utility.

The paper is organized as follows. Section 2 discusses related literature. Section 3 describes the main data sources and empirical methodology. In Section 4, I analyze the relationship between the disposition effect and natural disasters. Section 5 considers potential explanations and mechanisms, and Section 6 concludes.

2. Related Literature

How does an individual's environment affect her investment choices? Genetics and experiences are two complementary drivers of human behavior. Among investors, several studies

[5] It is worth noting this intuition follows even after I show that an investor's liquidity does not play a significant role in this behavior, suggesting the psychological impact of the natural disaster may be just as (if not more) salient than the monetary impact.

(Barnea, Cronqvist, and Siegel, 2010; Cesarini at al., 2010; Cronqvist and Siegel, 2014) have shown that genetics play a vital role in explaining variation among portfolios, while others (Levy and Galili, 2006; Cronqvist et al. 2016; Knupfer, Rantapuska, and Sarvimaki, 2017) have shown that experiences and environments can be even more influential to one's investment decisions. I contribute to this area of literature by combining county-level disaster data with account-level retail brokerage data to study the impact of a unique environmental experience (natural disasters) on individual investor selling behavior.

Individual investors display many qualities that deviate from rational investing. Although high IQ investors can display superior stock picking abilities (Grinblatt, Keloharju, and Linnainmaa, 2012), the majority of individuals are likely to display various biases that hurt their performance. Their portfolios tend to be under-diversified (Goetzmann and Kumar, 2008) with a particular tilt toward local stocks (Seasholes and Zhu, 2010). When choosing which stocks to sell, they are eager to sell gains and reluctant to realize losses (the disposition effect – Odean, 1998). In general, these behaviors are not driven by informed trading, tax considerations, portfolio rebalancing, or other rational reasoning. I contribute to the literature on individual investors by identifying a unique experience that they derive utility from and documenting how that experience affects their trading behavior biases.

While I am the first to my knowledge to study retail investor selling decisions in the setting of natural disasters, I am not the first to exploit the randomness of natural disaster exposure in financial settings. Barrot and Sauvagnat (2016) find evidence that suppliers affected by natural disasters transfer those losses to their customers, especially when the likelihood of substitution is low (i.e. when they produce very specific inputs). Additionally, Cortes and Strahan (2017) investigate how banks shield their core markets from disaster-related shocks in credit supply by bidding up the rate for deposits in core markets and reducing credit in unaffected markets. Furthermore, Elnahas, Kim, and Kim (2017) find firms in more disaster-prone counties are charged higher spreads by lenders and have more conservative leverage policies and greater earnings volatility, consistent with trade-off theory of capital structure.

Additionally, some researchers find significant natural disaster impacts at the individual-level. For instance, Dessaint and Matray (2017) show that managers overreact to hurricanes through increased corporate cash holdings. Moreover, Bernile, Bhagwat, and Rau (2017) present evidence of a nonlinear relationship between natural disasters and CEO behavior. Those CEOs slightly (severely) affected tend to be more risk-taking (risk-averse) than those not affected. Alok and Kumar (2016) analyze mutual fund holdings around disasters and show that managers nearby underweight firms with headquarters in disaster areas due to saliency bias.

Perhaps the papers most related to mine are those that measure how major life events influence individuals' investment decisions. Even the earliest experiences can have lasting effects. Cronqvist et al. (2016) finds that the prenatal environment can explain significant differences in investment choices. Malmendier and Nagel (2011) show that individuals who experience particularly low returns (such as those who lived during the Great Depression) consequently invest less in risky assets. Additionally, Knupfer, Rantapuska, and Sarvimaki (2017) identify labor market variation from the Finnish Great Depression to show that poor labor market conditions are associated with less risky asset investment. Wang and Young (2019) find reduced stock market participation and trading activity coupled with increased savings following an increase in U.S. terrorist attacks. Similarly, Levy and Galili (2006) document reduced stock market participation among Israeli households around terrorist attacks. Overall, these studies tend to focus on stock market participation rather than choices within securities. I contribute to this area of study by being the first to use a robust dataset of natural disasters as the source of exogenous variation in individual selling decisions, specifically measuring changes in the disposition effect.

3. Data and Methodology

I collect data from a retail investment brokerage and a natural disaster database. In this section, I will explain the sources for those data, the identification procedure, and the main empirical structure for testing.

3.1. Individual Investor Data

The setting for hypothesis testing is the trading activity of retail investors. I use the same large discount broker dataset utilized by Barber and Odean (2000). The raw data span January 1991 to November 1996 and record trading activity for approximately 78,000 households with 158,000 accounts.

I use daily account transactions to construct a dataset of holdings at the account-day-stock level. The initial sample includes 104 thousand accounts with common stock positions that own a mean of 3.5 stocks across 1,497 trading days. However, only 71% of households are associated with detailed location data necessary for my analysis. Additionally, I restrict attention to only those account-days in which at least one sale occurred similar to Birru (2015) and Chang, Solomon, and Westerfield (2016). Given that a sale only occurs for 0.5% of account-dates in which investors hold common stock, this creates 1.9 million potential observations.[6] Finally, similar to Ben-David and Hirshleifer (2012), I apply several filters to avoid common issues.

First, I include only common stocks that appear in CRSP with price and share data. Additionally, I adjust for splits and dividends using CRSP factor adjustments since prices in the discount brokerage dataset are unadjusted. Second, I eliminate account-stocks with negative commissions as they could indicate a reverse transaction. Third, to reduce the effect of illiquid stocks, I require all stocks to have at least one day of active trading in the preceding 250 trading days. Fourth,

[6] 1.9 million = 104,000 * 1,497 * 3.5 * 71% * 0.5%

I remove positions held at the start of the period as the initial purchase price cannot be determined, so the stock's return to the investor is ambiguous. Fifth, investor-stocks that attain negative positions (through short selling) are assumed to be liquidated at the time of turning negative. Sixth, the initial purchase day for each investor-stock is dropped since the data do not include intraday time stamps. After applying these restrictions, the sample has 827,430 account-day-stock observations.

3.2. Natural Disaster Identification

Natural disasters serve two primary purposes for this analysis: (1) they represent significant wealth (and potentially psychological) shocks to individuals outside the holdings of their portfolio, and (2) they are random, which makes them especially useful for unbiased identification.[7] While the psychological impacts are nearly impossible to measure, I am able to proxy for individual impacts using per capita damage estimates. Although an ideal experiment would utilize individual-level damages, my proxy is at the county-level. Admittedly, this aggregate measure implies that investor damages will be measured with noise and bias *against* finding significant results.

Nonetheless, I gather hazard data at the county-level for natural disaster events from the Spatial Hazard Events and Losses Database for the United States (SHELDUS). The underlying source for SHELDUS is the National Climatic Data Center (NCDC). The natural disaster dataset includes event name, county, state, hazard type, month, year, damages, and damages per capita.[8] Disaster types include droughts, thunderstorms, hail, hurricanes, winter storms, landslides, floods, volcanoes, wildfires, and tornados. While the entire SHELDUS data includes events from 1965 to 2015, I focus

[7] Even though one could argue that natural disasters may be predictable in certain regions of the country, it is unlikely that predictability exists at the county level.

[8] Dollar damage totals are adjusted to 2016 for inflation. When collecting damage estimates, SHELDUS provides a conservative total. SHELDUS takes the lower bound of the ranges included in NCDC reports. Thus, aggregate event damages are often lower than other sources report; however, SHELDUS estimates still maintain a correlation of 0.95 with the National Oceanic and Atmospheric Administration (NOAA 2017). Damages include all property and crop damages.

on events before December 1996 since the individual investor data ends on November 1996. Additionally, I only include events that occur after January 1987 since the maximum assumed impact I include is four years and the investor data begin January 1991.[9] Over this time period, SHELDUS includes 23 named events. Table 1 displays the list of disaster events as well as the associated month of the event, number of counties impacted, total damages, and distribution statistics of damages per capita across counties.

[Insert Table 1 Here]

To measure investor behavior variation by natural disasters, I must first identify county-months with disaster impacts. This identification requires two dimensions of measurement: severity and time. I create a cumulative damage per capita variable for each county-month since many counties are affected multiple times.[10] Because disaster impacts on investor behavior are likely to decrease over time, I test various cut-off points for the length of time assumed to be impactful: 1 year, 2 years, 3 years, and 4 years. For example, when assuming a 1-year impact, if a county is impacted with $1,000 of per capita damages in January of 1991, then every month following reflects the $1,000 until January of 1992 when the amount is then subtracted to reduce the cumulative impact back to zero. While the initial impact is expected to be the strongest, studies such as Bernile, Bhagwat, and Rau (2017) show natural disasters could have a lasting effect.

It is worth noting the investor data only include the zip code of the investor while they operate their brokerage account (from 1991 to 1996), so the more years the disaster occurred before this time

[9] Appendix Table A.1 shows results using 5-year and endless impact assumptions, which includes all 40 disaster events from 1965-1996. These tests are omitted from the main portion of the paper because the impacts I document largely disappear after 3 years.

[10] In fact, each county that appears in the natural disasters database over the sample period is impacted by at least some level of disaster exposure 3.5 times on average.

period, the more likely it is that investor location is measured with noise (due to individuals moving). Assuming a longer impact period allows me to include more events and potentially gain greater statistical power. However, a longer exposure period introduces more noise to the investor location measurement. Thus, these cutoff points also test the appropriate empirical balance between power of the test and measurement error.

3.3. *Basic Methodology*

Odean (1998) defines the disposition effect as the difference in the probability of selling a gain and the probability of selling a loss. To measure how a natural disaster affects an investor's disposition effect, I employ a regression method similar in spirit to An et al. (2019), Birru (2015), and Chang, Solomon, and Westerfield (2016).

These previous studies measure the disposition effect of investors as:

$$Sale_{i,j,t} = \beta_0 + \beta_1 Gain_{i,j,t} + \epsilon_{i,j,t} \tag{1}$$

where observations occur at the account (i), stock (j), and date (t) level. For each observation, *Sale* equals one if the stock of interest is sold (including partial sales) and zero otherwise, and *Gain* equals one if the stock's return to the investor is strictly positive and zero otherwise. Under this specification, the mean of *Sale* is simply the probability of selling a given stock. Therefore, β_0 represents the probability of selling a loss, while β_1 represents the increase in probability of selling a gain (i.e. the disposition effect). Chang, Solomon, and Westerfield (2016) and others document β_1 as positive and statistically significant.

My focus is the relationship between the disposition effect and natural disaster exposure. To estimate this relationship, I use the following regression structure:

$$Sale_{i,j,t} = \beta_0 + \beta_1 Gain_{i,j,t} + \beta_2 Disaster_{i,t} + \beta_3 Gain_{i,j,t} \times Disaster_{i,t} + \epsilon_{i,j,t} \tag{2}$$

where observations are also at the account (i), stock (j), and date (t) level. To start, *Disaster* is an indicator variable equal to one if the given account-date is exposed to a natural disaster, and zero otherwise. As discussed in Section 4, I use various definitions of *Disaster* to measure how the affect changes based on severity.

The primary coefficient of interest in equation (2) is β_3 (the interaction term). This coefficient measures the increase in the disposition effect for disaster-exposed observations. To gauge economic significance, it is worth noting that β_1 in equation (2) measures the disposition effect for observations that are not exposed to natural disasters. Therefore, the sum of β_1 and β_3 measures the disposition effect for observations exposed to a natural disaster.

4. The Disposition Effect and Natural Disasters

4.1. Disaster Exposure

First, I test if any level of disaster exposure affects the disposition effect of individual investors. To measure this, I divide the sample into those observations impacted by *any* level of natural disaster and those that are not affected. Using the four different impact length assumptions, I define *Disaster* to be equal to one if the cumulative disaster damages per capita in the investor's county of residence is positive, and zero otherwise.

[Insert Table 2 Here]

Table 2 displays summary statistics of the sample split based on this identification procedure as well as the interaction coefficient from equation (2). Panels A-D reflect the results when the disaster damages are assumed to last 1-4 years, respectively. While the proportion of *Disaster* observations increases as the assumed length of the disaster grows, the disposition effect is prevalent in all subsets of the data. This highlights just how pervasive the disposition effect is among retail investors. Moreover, the disposition effect is larger for all *Disaster* subsets regardless of the assumed impact length. The largest increase for the disaster-affected observations occurs in Panel D (using 4-year impacts) with a difference of 1.3% (t-stat 6.70).[11] This difference represents an 18% increase (8.7%/7.6% -1) in the disposition effect from those observations unaffected by natural disasters. Other time periods show qualitatively similar results. Moreover, the change in the disposition effect across all panels seems entirely driven by the increased propensity to realize gains since loss sale probabilities are similar in both subsets. This is the first piece of evidence in favor of the main hypothesis: an increased disposition effect for disaster-affected investors.

Although these results are a natural first step, sale decisions are likely to be correlated within accounts, within stocks, and within dates. To account for these correlations, I report t-stats that use standard errors clustered across these three dimensions using the procedure of Cameron, Gelbach, and Miller (2011). Once the standard errors are adjusted, statistical significance no longer exists in any of the panels from Table 2. Now, the largest t-stat (which still occurs in Panel D) only reaches 1.24. Thus, the difference is not statistically meaningful when assuming *any* level of disaster exposure.

Table 2 also shows how the *Disaster* proportion of the sample grows substantially as the disaster period grows. In fact, because the disasters are so sprawling, the 4-year impacts identify about 48% of the account-stock-date observations as disaster-impacted. These results are in stark contrast to the belief that natural disasters are rare. Perhaps any level of disaster exposure is actually quite

[11] This difference is equivalent to the interaction coefficient from equation (2).

common and does not elicit a significant shock. Instead, maybe severe impacts are less common and more likely to be meaningful. From Table 1, very minor disaster-impacted counties make it to the sample on several occasions. These observations highlight the need to analyze disaster severity.

To first measure the impact of severity, I replace the *Disaster* indicator variable from equation (2) with *Damage*, a variable that indicates the per capita cumulative dollar impact to the given county in which the investor resides. This allows me to differentiate between the treatment and the dosage of a disaster event. For easier interpretation of regression coefficients, damages are scaled by $10,000.

[Insert Table 3 Here]

Table 3 displays the regressions of equation (2) using *Damage* instead of the *Disaster* dummy variable with a host of fixed effect controls. In addition to clustering standard errors, column 4 of Table 3 in all panels controls for unobserved account, time, and stock characteristics. Panel B (using 2-year impacts) shows the largest interaction coefficient of 1.9% across all four specification. Furthermore, the result is statistically significant at the 1% level even when controlling for account, stock, and date fixed effects (t-stat 3.64). This means that a $10,000 dollar increase in per capita disaster damages is associated with an average disposition effect increase of 21% (1.9%/9.1%). This table is the first piece of statistically significant evidence using various fixed effect controls that the disposition effect increases with disaster severity. Moreover, when impacts are assumed for longer than 3 years, the increase is no longer statistically significant after clustering standard errors across accounts, dates, and stocks. Finally, the strongest increase in the disposition effect for disaster-affected investors occurs when using a 2-year impact period.[12]

[12] Appendix Table A.1 shows the results when using 5-year and endless impact assumptions. The interaction coefficient continues to decline across all specifications as the length of time grows.

4.2. Disaster Severity Indicators

Although I calculate the average linear effect of disaster dollar damages in Table 3, it is possible that the relationship between disaster severity and the disposition effect increases nonlinearly. Moreover, since the distribution of per capita damages across counties tends to increase exponentially, I hypothesize that the most extreme disasters will increase the disposition effect much larger than the 21% increase per $10,000 documented in Section 4.1.

To better understand the disposition effect across levels of disaster severity, I segment the observations into four cohorts (*None*, *Moderate*, *Severe*, and *Extreme*) similar in spirit to Bernile, Bhagwat, and Rau (2017). Thus, I categorize a county-month as *Extreme* if the county-month's cumulative disaster damage is at least the 99th percentile across all events. Similarly, I define a county-month as *Severe* if the cumulative disaster damage is below the 99th percentile and greater than or equal to the 90th percentile. If the county-month is below the 90th percentile but greater than zero, I define it as *Moderate*.[13] County-months with no damages are defined as *None*.

When using an assumed disaster impact of 2 years, 74% of the total 827,430 observations are classified as *None*, 23% as *Moderate*, 3% as *Severe*, and 0.1% as *Extreme*. Across each of these subsets, the disposition effect persists.

[Insert Figure 1 Here]

Figure 1 reports the sale probabilities across these cohorts. The top graphs report sale probabilities for gains (green bars) and losses (red bars) while the bottom charts the disposition effect, the difference between the probability of gains realized (PGR) and the probability of losses realized

[13] From Table 1, the 99th (90th) percentile is $10,590 ($1,118).

(PLR) for each disaster severity subset. A clear pattern emerges. The disposition effect climbs from 7.8% with no disaster impacts to 8.4% following moderate disasters to 10.0% after severe impacts to 12.3% following extreme impacts. From *None* to *Extreme*, the disposition effect grows a staggering 58% (12.3%/7.8% - 1). Also, in this view, the change is driven more by the reluctancy of selling losses than the eagerness to sell gains. PLR decreases by 41% while PGR only decreases by 13%. Next, I use fixed effects regression analysis to control for variations in the propensity to sell a stock across accounts, stocks, and dates.

[Insert Table 4 Here]

In Table 4, I report regressions with severity indicator variables and their interactions with *Gain*. Since *None* is omitted, the interactions are interpreted as the increase in the disposition effect from no disaster impacts. Therefore, all coefficients are positive but not all statistically significant. The interaction of *Gain*Extreme* in column 1 (1-year impacts) shows the highest value with a coefficient of 8.6% (t-stat 3.91). Additionally, a coefficient value of 8.6% indicates that the disposition effect increases by an astounding 96% (8.6%/9.0%) from no disaster impacts in the year following an extreme disaster. Although the increases for *Severe* and *Moderate* are no longer statistically significant when controlling for unobserved account, stock, and date characteristics, they are still positive across all specifications. Moreover, the pattern across time is clear. The coefficients of *Gain*Extreme* decrease monotonically as the assumed disaster-impact period increases while the interactions of *Severe* and *Moderate* are economically insignificant and flat across time.

All tests show consistent evidence that disaster severity is an important aspect of natural disaster exposure and positively related to the disposition effect. Regarding the length of time following the event, the evidence is a little more mixed. Still, both damage estimates and severity indicator tests indicate the strongest effects occur within 1-2 years after the event.[14] After 2 years, the effects decrease substantially and no longer become economically (and in most cases statistically) significant. From this evidence, I conclude that disaster severity is positively associated with the disposition effect, and the length of time after the event is negatively related to the disposition effect, especially following a 2-year period.

4.3. State-Matched Difference-in-Difference Tests

Thus far, I have analyzed the relationship between natural disaster exposure and investor selling decisions while controlling for variations in the probability to sell a stock across accounts, stocks, and dates. However, it is possible that disaster impacts may be correlated with an unobserved geographic variable that is also correlated with an investor's disposition effect.[15] Additionally, disasters may happen to occur before periods that have a higher disposition effect due to time variations in the disposition effect.[16] This section will use a state-matched difference-in-difference design to measure the change in the disposition effect *within* accounts while controlling for changes in the disposition effect across time and variations in regional selling behavior.

To measure how the disposition effect changes within accounts, I create an event study panel using only the accounts in the *Extreme* cohort (as defined in Section 4.2) as the treatment group and accounts from the same state, but not impacted by a disaster, as the control group. I only use those extreme disasters that take place from January 1992 to November 1995 to ensure a full year in the sample before and after disaster events. I also remove any accounts that were impacted by an extreme

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[14] Appendix Table A.2 shows all four specifications used in Table 3 for the severity indicator tests and a similar pattern emerges. Years 1 and 2 sometimes alternate in order of the strongest interaction coefficients, but following 2 years, every specification shows a decrease as the time assumed grows.
[15] For example, Heimer (2016) documents how social interactions contribute to an investor's disposition effect and finds that connected traders have correlated disposition effects.
[16] An et al (2019) and Bernard, Loos, and Weber (2018) show that the disposition effect has cyclical patterns over time related to the performance of the market.

event in the two years (1990-1991) before the sample to avoid misrepresentation in the disaster identification. Once I have identified the account-dates to use from January 1992 to November 1995, I include all observations one year before and after the event in which the account owned a common stock position. Therefore, I remove the condition that a sale must have occurred on the given account-date in order to generate a more balanced panel and to identify if the effect is driven by over or under selling. I also require that the account owned at least one stock in both the year before and after the disaster event. Once these restrictions are implemented, I analyze the disposition effect of the treatment and control group around the disaster events.[17]

The overall sale propensities before and after the disaster events are nearly identical (before 0.26%, after 0.25%).[18] However, once I condition on the account being in the treatment group, differences emerge. The treatment group increases its selling propensity (0.22% to 0.36%) while the control group decreases slightly (0.27% to 0.25%). Next, to analyze how the disposition effect changes, I condition on the stock's performance to the investor. Although the disposition effect persists across all four sub-samples, it is largest for the treatment group after the disaster. I represent this relationship graphically in Figure 2.

[Insert Figure 2 Here]

Figure 2 graphs the probability of selling gains and losses (top) and the difference between selling gains/losses (bottom) in the year before and after an extreme disaster event. For the treatment group, the probability of selling gains (losses) increases from 0.28% (0.14%) to 0.51% (0.19%).

[17] Summary statistics for the state-matched treatment and control groups around extreme disasters are available in Appendix Table A.3.

[18] It is worth noting the selling probabilities are much lower than previous tests simply due to the removal of the sale condition. Other papers that remove this condition include Ben-David and Hirshleifer (2012) and An et al. (2019).

Because the probability of selling gains increases at a higher rate than losses, the disposition effect increases from 0.14% to 0.32% (a 129% increase) following an extreme natural disaster. While the sale-conditioned tests measure *what* the investor sells, given she sells, these unconditioned tests measure *when* she sells given the performance of the stock.

To control for other time varying effects on the disposition effect, I compare these changes to the control group of accounts that live in the same state as those impacted but reside in counties with no damages. Interestingly, I find that this group's disposition effect actually *decreases* from 0.10% before the event to 0.07% after the event, albeit not as dramatic as the treatment group's increase. This could be driven by the pattern described in Bernard, Loos, and Weber (2018); Or perhaps, a sense of relief from not being impacted lessens the investor's need for a positive burst of utility. To measure the net effect, I subtract the increase in the treatment group and the decrease in the control group to get the difference-in-difference estimate of 0.21%[19], indicating disaster-affected accounts exhibit a disposition effect 81% (0.21%/0.26%) larger than the average disposition effect across all account-stock-dates. Moreover, this effect is driven by an overselling of gains rather than a reluctance to sell loses. Consistent with the previous tests, I continue by testing this relationship using regression analysis with various fixed effects controls to determine robustness.

[Insert Table 5 Here]

Table 5 reports the state-matched difference-in-difference regressions. *Sale* and *Gain* are defined the same as previous tests. *Treatment* is equal to one if the investor lives in a county impacted by an extreme disaster and zero for all investors in the same state that are unaffected. I define *After* to equal one if the account-date occurs after the associated extreme disaster and zero if it occurs before

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[19] 0.21% = (0.32% - 0.14%) – (0.07% - 0.10%)

the event. The effect actually increases slightly (0.21% to 0.24%) when including account fixed effects controls. Moreover, the difference in the disposition effect between the treatment and control groups around extreme disasters remains economically and statistically significant at the 5% level for all specifications (ranging from t-stats of 2.12 to 2.36), even when controlling for account, stock, and date fixed effects. Thus, I conclude that extreme disasters significantly increase investors' relative propensity to realize gains over losses even when controlling for unobserved account, time, and stock characteristics as well as regional selling differences and time variations in the disposition effect.

5. Potential Explanations and Mechanisms

In this section, I examine several potential mechanisms that may drive the increased disposition effect of disaster-affected investors. Note, because not all investors have demographic information available, the combination of any subsamples used in this section is sometimes smaller than the entire sample. Additionally, I focus on the specification from column 4 of Panel B in Table 3 when diagnosing demographic information for two reasons. First, this specification allows me to analyze across all disaster-affected individuals rather than a specific cohort in which the number of observations may not be large enough to achieve statistical power. Second, this specification controls for variations in the propensity to sell across accounts, dates, and stocks using 3-way fixed effects in addition to controlling for correlations within these three variables by clustering standard errors across the same dimensions.

5.1. Liquidity Constraints

First, I test the impact that liquidity constraints may have on the increased disposition effect for disaster-affected investors. Natural disasters represent negative shocks to an individual, both financially and psychologically. If investors have limited access to liquid funds, then their sale decisions following a disaster may be driven simply by the wealth shock that accompanies a severe natural disaster. Still, if investors truly believe their holdings are chosen optimally, then they should partially liquidate all holdings to meet their liquidity needs. On the other hand, psychological impacts should be independent of investor liquidity constraints. I test if liquidity constraints are related to the result by examining the bill pay seasonality and investor income levels.

Although some bills may have various due dates, the largest bill for individuals, housing, is typically due at the first of the month. This is especially true for mortgage payers, which are common in my sample as about 97% of the investors with residence information are identified as homeowners. It naturally follows then to ask: is there within-month seasonality of the disposition effect for disaster-affected investors? If they need money to cover expenses, then perhaps their sales driving this result are concentrated at the end of the month in anticipation of a large upcoming payment, such as a mortgage.

[Insert Table 6 Here]

Panel A of Table 6 first splits the sample into three subsets based on the day of the month for the observation. In all three subsets, the interaction of *Gain*Damage* is economically similar (coefficients range from 2.4% to 1.7%) and statistically significant (t-stats range from 2.18 to 3.50). Finally, column 4 then interacts the day of the month with the variable of interest to produce a triple interaction. The interpretation of this triple interaction is the incremental increase in the main result when the day of the month increases by one. If the result was concentrated in the end of the month, then this triple interaction coefficient would be positive and statistically significant. However, the coefficient is actually negative and achieves no statistical or economic significance with a t-stat of only -0.33, consistent with the sub-sample analysis.

Still, it may be possible that investors are financially constrained but just choose to sell their securities at various points within the month. Perhaps some investors anticipate the upcoming expenses and some pay late as not to create any aggregate seasonal effect. Additionally, other bills that are less likely to have within-month seasonal patterns, such as credit cards, may drive the selling decisions. For this reason, I next test the impact of investor income levels. The intuition for testing income levels is that low income investors should be more affected by liquidity constraints.

Similar to the tests for within-month seasonality, I first split the sample based on investor yearly income level by using $75,000 as the cutoff point since it is roughly the median of investors with income information available. Panel B of Table 6 conducts the tests on these sample splits, and the results are nearly identical for both subsamples. Column 3 of Panel B then adds a dummy variable, *Low Income,* which takes the value of one if the investor's reported income is below $75,000, and zero otherwise. If lower income levels are driving the result, then the interaction of *Gain*Damage*Low Income* should be positive and statistically significant. Although the coefficient is slightly positive (0.1%), no statistical significance exists (t-stat 0.13). The results are qualitatively similar for other income cutoff points.[20]

Based on the analysis of within-month seasonality and investor income levels, I conclude that liquidity constraints are not a main driver of the increased disposition effect for disaster-affected individuals.[21] While the wealth shocks that accompany a natural disaster may impact investors' utility, a significant portion of the influence may also be psychological.

[20] In Appendix Table A.4, I show the result is robust to using $50,000 as the yearly income cutoff point.
[21] Additionally, I test the impact of investor sophistication since sophisticated investors may be more prepared for negative wealth shocks and less likely to be impacted by liquidity constraints in the wake of a natural disaster. Appendix Table A.5 shows that the increased disposition effect of disaster-affected investors exists among professional employment categories as defined in Dhar and Zhu (2006), further suggesting that this increase is not driven by liquidity constraints.

5.2. Tax Incentives

Given that the increased disposition effect for disaster-affected individuals is independent of liquidity constraints, it may be the case that these investors are simply exhibiting the disposition effect to take advantage of the disaster event through a tax incentive. Odean (1998) documents that individuals may adjust their relative propensity to realize gains/losses based on tax incentives as he shows a reverse disposition effect in December in accordance with tax loss selling. In fact, to exhibit the disposition effect in a taxable brokerage account actually increases an investor's tax burden. However, for disaster-affected individuals, the IRS allows casualty loss deductions that could lower an individual's taxable income. It naturally follows then that investors may oversell their gains simply because their taxable income is relatively low, and thus they can realize a gain at a lower tax rate. Similarly, they may be reluctant to sell losses because the tax benefit of realizing a loss is reduced and would be better utilized in a year that they cannot reduce their taxable income through a casualty loss deduction.

[Insert Table 7 Here]

To test this mechanism, I exploit the different tax rules for the accounts in the individual investor trading data. The data include both taxable brokerage accounts and tax-deferred accounts (such as IRAs and Keogh plans). In Panel B of Table 7, I first split the sample based on the tax rules of the account. The coefficient of interest is still positive and significant in both subsamples and is actually higher for tax-exempt accounts (3.1%, t-stat 2.40) than taxable accounts (1.7%, t-stat 2.60). The third column adds a variable, *taxable*, that takes the value of one if the account is subject to yearly income taxes, and zero otherwise. If tax incentives are driving the increased disposition effect for disaster-affected individuals, then the triple interaction of *Gain*Damage*Taxable* should be positive and significant. However, the coefficient is actually negative (-1.4%) and not statistically significant (t-stat -0.94). These results are counter to any tax incentive mechanisms driving the increased disposition effect of disaster-affected individuals.

5.3. Local Stocks

Another plausible explanation for investor selling decisions after a natural disaster is that individuals may have an informational advantage for local stocks since they are closest to any environmental impacts. Perhaps local investors choose to sell (hold) local gains (losses) because they can better predict the future performance of local firms. While it is reasonable to assume natural disasters adversely impact local businesses, some businesses may actually profit from the disaster-relief funding that is likely to follow.[22] Thus, winners and losers may emerge even though the net effect is negative. If local investors can identify those winners and losers, the increased disposition effect of disaster-affected investors may be a rational response to a natural disaster. Although some papers (Ivkovic and Weisbenner, 2005; Massa and Simonov, 2006) argue that local trading may be a result of an informational advantage, most recently, Seasholes and Zhu (2010) provide strong evidence that the aggregate local bias of individual investor portfolios in this sample is not a reflection of informed trading. Thus, they may also trade local stocks for reasons related to local affinity, familiarity, or overconfidence.

[Insert Table 8 Here]

[22] Although disasters are most often negative shocks to areas afflicted, it is also worth noting that Loayza et al. (2012) find evidence of heterogeneity among disaster types with some disasters having positive effects to certain economic sectors.

Nonetheless, I identify the impact of local trading on the disposition effect for disaster-affected individuals in Table 8. Columns 1 and 2 provide split sample results based on the distance of the stock's headquarters to the investor. I define *Local* to be one if the firm's headquarters are within 250 miles of the investor's location, and zero otherwise.[23] The coefficient of interest is positive for both subsamples. Although the statistical significance is stronger for non-local stocks (t-stat 2.35) than local stocks (t-stat 1.17), the economic magnitude is actually weaker for non-local stocks (1.7%) than local stocks (9.7%). The statistical significance for local stocks may diminish due to the power of the test as the sample size decreases by about 73%. Column 3 then adds the *Local* variable and interacts with the coefficient of interest to determine if the increased effect for local stocks is statistically significant. The triple interaction of *Gain*Damage*Local* is positive (9.3%) and statistically significant at the 5% level (t-stat 1.97) indicating that the increased disposition effect is roughly 5.6 times larger (9.3%/1.4% - 1) for local trading. Even though the effect still exists for non-local stocks, I conclude that local trading plays a significant role in the increased disposition effect of disaster-affected individuals.

5.4. *Informed Trading*

Thus far, I have ruled out mechanisms related to liquidity constraints and tax incentives, yet I found evidence in favor of local trading. This section aims at answering whether or not the increased disposition effect of disaster-affected investors is driven by informed trading. Although the aggregate disposition effect does not reflect informed trading (Odean, 1998), it may be the case that after natural disasters, individuals help incorporate disaster-related information into local stocks. In contrast, individuals may propagate their afflictions if their trades are especially uninformed.

[23] In addition to Compustat firm headquarter information, I use Compact Disclosure to adjust the location of any firms that have changed headquarters since the 1996, the end of the individual investor sample.

Recall that the disposition effect is a result of an individual's eagerness to sell gains and reluctancy to sell losses. Thus, if the disposition effect is a result of informed trading, future returns of unrealized (or paper) loses should outperform future returns of realized gains. I employ a similar methodology as Odean (1998) to compare the ex-post returns of realized gains and paper losses across the disaster cohorts defined in Section 4.2.

[Insert Table 9 Here]

Table 9 shows these tests using two methodologies, excess market returns and excess DGTW returns.[24] I report excess returns for paper losses and realized gains over the subsequent 252-trading days (one year) consistent with Benartzi and Thaler's (1995) estimated average investment horizons.[25] Additionally, the difference is calculated and standard errors are clustered across accounts, dates, and stocks to account for correlations within these three dimensions. If the increased disposition effect of disaster-affected individuals reflects informed trading, the subsequent performance of paper losses should outperform realized gains, and the difference should be positive and statistically significant.

In the top portion of Table 9, returns are calculated in excess of the CRSP value-weighted index. The first column establishes that Odean's (1998) result (the aggregate disposition effect does not reflect informed trading) holds for my sample as well. In fact, paper losses tend to under-perform the market by 4.5% percent annually while realized gains achieve slightly above annual market returns (0.4%). Moreover, the difference is statistically significant at the 1% level (t-stat -4.00). Columns 2-5 report the same tests for *Extreme, Severe, Moderate, and None* disaster cohorts. Not only does each cohort show a negative difference (annual percentages range from -3.6% to -10.6%) but all achieve statistical

[24] See Daniel, Grinblatt, Titman, and Wermers (1997).
[25] Appendix Table A.6 shows the results are robust to shorter (84 days) and longer (504 days) investment horizons similar to Odean (1998).

significance (t-stats range from -1.75 to -4.71). For extreme disasters, the disposition effect costs investors -10.6% (t-stat -2.21) in excess market returns annually, which is in stark contrast to the belief that these investors have an informational advantage.

To ensure these results are not driven by size, value, and momentum characteristics of investor holdings, the bottom portion of Table 9 reports excess DGTW returns. To compute DGTW excess returns, each stock-date is matched to one of the 125 (5 x 5 x 5) DGTW member groups in each year.[26] Then, the member group's holding period return is subtracted from the stock's holding period return. Even when using these characteristic-adjusted matched portfolios, the difference in ex-post returns for paper losses and realized gains remains negative in all subsamples. While the differences are smaller when using excess DGTW returns instead of excess market returns, the disposition effect of extreme disaster-affect investors is still costly at -5.8% (t-stat -1.86) annual characteristic-adjusted returns. In summary, the ex-post returns indicate that the increased disposition effect of disaster-affected individuals does not reflect informed trading. In fact, this behavior reflects significantly uninformed trading.

5.5. *Residence Utility*

The question remains: why do investors display a significantly stronger disposition effect after a natural disaster? If this behavior is costly, unchanged by liquidity constraints and tax incentives, yet stronger when trading local stocks, perhaps the answer is related to affinity for their local area. A natural disaster causes monetary and psychological distress to an individual. If investors receive this external utility shock from their environment, then they may attempt to offset that negative experience by realizing gains, which have been shown to cause a burst in utility to investors (Frydman et al., 2014).

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[26] All observations in July-December are matched to the same year, and all observations in January-June are matched to the previous year because the DGTW groups are created on June 30. The DGTW benchmarks are available via Russ Wermer's website, http://terpconnect.umd.edu/~wermers/ftpsite/Dgtw/coverpage.htm.

Similarly, they may be especially reluctant to realize a loss, resulting in an increased disposition effect. I hypothesize that the magnitude of the external shock will be greater for individuals with stronger ties to their community.

[Insert Table 10 Here]

To proxy for the connectedness of individuals to their community, I use demographic information on duration investors have lived at their listed address. I define a variable, *Long Residence,* as one if the investor has lived at her residence for at least 10 years, and zero otherwise. Table 10 displays the split sample results based on the investors' duration at their residence. The coefficient of the interaction term is much larger for long residencies (8.9%, t-stat 2.83) than short residencies (1.7%, t-stat 3.42). In column 3, the triple interaction (that tests the difference of the interactions in columns 1 and 2) of *Gain*Damage*Long Residence* is positive (7.7%) and statistically significant (t-stat 2.40). This means that the increased disposition effect for disaster-affected investors is 3.8 (7.7%/1.6% - 1) times larger for individuals that have maintained a residence for at least 10 years. [27]

While the duration at residence is a valid proxy for individuals' ties to their local community, the decisions investors make after the sale is also telling. What do the investors do with the cash from these sales? Do they reinvest in a different stock or do they hold on it? Frydman, Hartzmark, and Solomon (2018) document that individuals do not close their mental account when they reinvest the earnings from a sale into a new stock. Instead, they continue with the initial reference point. Thus, the burst of positive (negative) utility from the sale of a gain (loss) is attenuated if the proceeds are

[27] Appendix Table A.7 tests if the effect is stronger for homeowners compared to renters. Unfortunately, the sample of identified renters is extremely small (3%), so statistical power is hard to achieve. However, the coefficient of interest is approximately twice as larger for homeowners than renters, consistent with the residence utility hypothesis.

reinvested into a different stock. This logic generates two predictions regarding gain/loss sales after a natural disaster. If individuals are attempting of offset their environmental utility shock, I hypothesize that (1) investors will be least likely to reinvest after the sale of a gain in the wake of a natural disaster and (2) investors will be most likely to reinvest after the sale of a loss in the wake of a natural disaster. In these cases, it is important to the investor that the gain is realized and held, while the loss is diverted to a different investment.

[Insert Table 11 Here]

Table 11 tests the reinvestment probability levels of four situations: *Loss_Disaster, Gain_Disaster, Loss_None, and Gain_None.*[28] Let *Loss_Disaster* be defined as one if a disaster-affected investor sells a loss, and zero otherwise. Similarly, *Gain_Disaster* is defined as one if the disaster-affected investor sells a gain, and zero otherwise. Finally, *Gain_None (Loss_None)* takes the value of one if an investor unimpacted by a disaster sells a gain (loss). The dependent variable, *Reinvest*, equals one if the given account purchases a different stock within the period following the sale. I restrict to only account-dates in which one sale occurred to avoid ambiguity. Columns 1-6 show reinvestment periods of 1 day, 5 days, 10 days, 15 days, 20 days, and 25 days, respectively. *Gain_Disaster* is omitted, so all coefficients represent the increase in the reinvestment probability from the *Gain_Disaster* scenario. Consistent with prediction (1), all coefficients except one have positive values, indicating the subsequent reinvestment probability is lowest when disaster-affected investors sell a gain. Additionally, all coefficients are statistically significant in column 6 (using a 25-day reinvestment period). For prediction (2) to hold, *Loss_Disaster* should be the largest coefficient. This occurs in columns 1-4 (any

[28] This test is similar in spirit to An et al. (2019) when they are determining if investors receive utility over both paper and realized gains/losses.

period within 15 days). Although the exact reinvestment period likely differs for each individual, this evidence shows that prediction (2) holds for up to 15-day reinvestment periods while prediction (1) holds for any period over 5 days, with the strongest evidence using a 25-day reinvestment period. Still, across most specifications, investors are most (least) likely to reinvest proceeds from a loss (gain) following a disaster, consistent with the idea that they are attempting to offset an external negative utility shock.

6. Conclusion

I find evidence that the well-documented disposition effect increases after investors are impacted by a natural disaster. The increase for damaging events holds even when controlling for variations in the probability to sell across accounts, stocks, and dates as well as controlling for time-series variations of the disposition effect and variations in regional selling behavior. I show this behavior in univariate tests, multivariate regressions with a host of fixed effects, and a difference-in-difference design with a state-matched control sample. Furthermore, the disposition effect increases with disaster severity and decreases with the length of time following the event. Overall, these results suggest that investor behavior may be strongly affected by external individual-specific events, especially in the short-term.

I consider mechanisms related to liquidity constraints, tax incentives, local stocks, informed trading, and residence utility. While the effect seems unrelated to liquidity constraints, taxes, and informed trading, the effect increases significantly when investors trade local stocks and have lived at their residence for at least 10 years. Moreover, reinvestment probabilities after disaster sale decisions are consistent with investors deriving utility from their environment and exhibiting the disposition effect to offset this negative shock.

Furthermore, these results document that investors use their portfolio to offset utility derived from sources outside of their portfolio. In this case, utility from disproportionately realizing gains versus losses help offset natural disaster losses. While natural disasters may have financial *and* psychological impacts, it is nearly impossible to separate those effects. Because financial constraints do not seem to impact these results, the psychological impact may be especially strong. One potential avenue for future research is the effect of non-financial sources of utility (such as marriage, health, or social status) on trading behavior. Additionally, the shocks I identify only have significant impacts in the short-term, but others may have more lasting effects. I leave these questions for future research.

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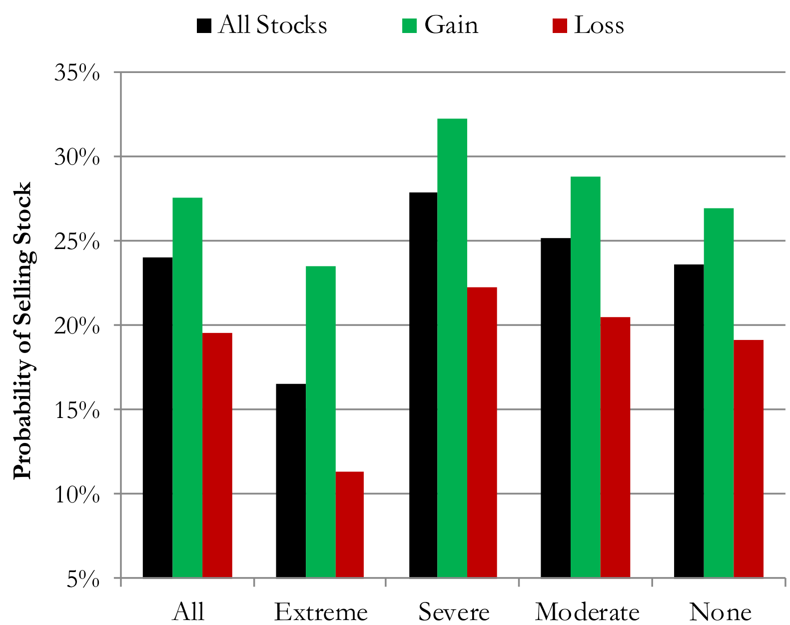
Figure 1: The Disposition Effect by Natural Disaster Severity Cohorts

This figure displays the probability of selling a stock based on the stock's cumulative return to the investor for various levels of natural disaster severity. I assume natural disaster impacts last for two years. I define *Gain* (*Loss*) to be those stocks that have a cumulative return to the investor greater than zero (less than or equal to zero). *Extreme* observations are those in which the given account-day is exposed to the top 99th percentile of damage ($10,590) across all disaster-counties. *Severe* observations are those in which the given account-day is exposed to damage below the 99th percentile of damage but greater than or equal to the 90th percentile across all disasters ($1,118). *Moderate* observations occur if the given account-day is exposed to damage below the 90th percentile of damage but greater than zero across all disasters. *None* represents the account-days with no natural disaster exposure. In the top graphs, the probability of realizing a gain (loss) is represented by the green (red) bars, and the black bars represents the weighted average. The bottom graph charts the difference between the probability of a gain realized (PGR) and the probability of a loss realized (PLR) – i.e., the disposition effect.



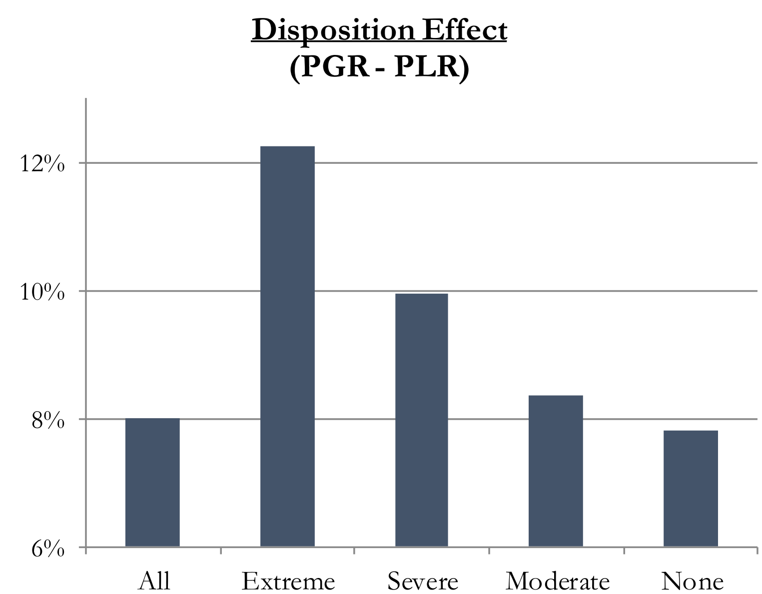


Figure 2: State-Matched Difference-in-Difference around Extreme Disasters

This figure displays the probability of selling a stock based on the stock's cumulative return to the investor around extreme disaster events. Extreme disasters are those in the top 99th percentile of damage across all disaster-counties ($10,590 per capita). I chart the selling probabilities of two groups of investors, the treatment and control groups. The treatment group of investors are those that live in a county impacted by an extreme disaster, and the control group consists of unaffected investors that live in the same state as those impacted. I plot the probability of selling for these groups one year before and one year after the disaster event. I define Gain (Loss) to be those stocks that have a cumulative return to the investor greater than zero (less than or equal to zero). In the top graphs, the probability of realizing a gain (loss) is represented by the green (red) bars, and the black bars represents the weighted average. The bottom graph charts the difference between the probability of a gain realized (PGR) and the probability of a loss realized (PLR) – i.e., the disposition effect.

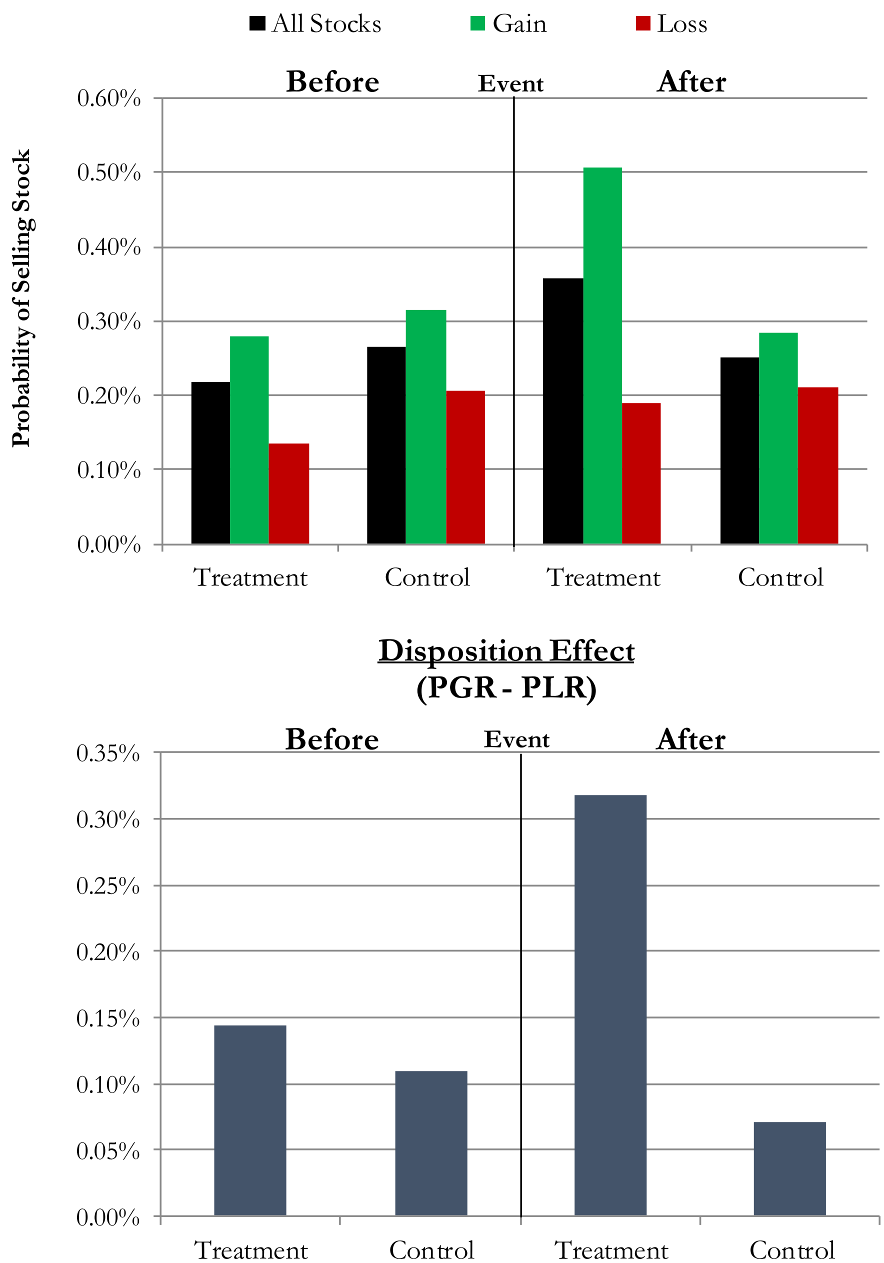


Table 1: Disaster Summary Statistics by Event

For each disaster event, this table displays the month it began, number of counties impacted, total damages (in millions), and damages per capita distribution statistics across counties. All dollar estimates are inflation adjusted to 2016 $USD.

		# of	Total Damage	County-Level $ Damages Per Capita						
Disaster Event Name	Month	Counties	(in $Mil)	Mean	10%	Median	90%	95%	99%	Max
1996 Hurricane Fran	Sep-96	202	6,293	882	0	30	2,556	4,218	9,647	21,627
1996 Drought Southern Plains	Apr-96	166	1,102	1,458	4	288	1,818	3,990	17,908	97,158
1996 Flooding Pacific Northwest	Feb-96	7	1	24	3	16	91	91	91	91
1996 Blizzard Flooding	Jan-96	580	1,238	47	0	4	93	180	945	2,425
1995 Hurricane Opal	Oct-95	337	5,544	451	0	7	423	2,181	9,309	15,068
1995 Severe Weather	May-95	118	6,092	1,090	0	3	3,836	7,691	12,602	16,284
1994 Flooding Texas	Oct-94	35	53	22	0	11	57	68	142	142
1994 Tropical Storm Alberto	Jul-94	88	147	85	0	1	397	584	1,217	1,217
1994 Tornadoes	Apr-94	53	100	8	0	0	20	42	148	148
1994 Ice Storm Southeast	Feb-94	372	1,278	124	0	15	116	191	2,905	10,002
1994 Earthquake Northridge	Jan-94	1	32,979	3,645	3,645	3,645	3,645	3,645	3,645	3,645
1993 Drought Heat Wave Southeast	Jun-93	277	1,155	132	7	34	356	723	1,608	2,325
1993 Floods Midwest	Apr-93	537	24,059	2,874	1	634	7,101	12,613	41,063	58,052
1993 Blizzard Storm of the Century	Mar-93	938	3,645	142	0	3	149	696	3,435	7,250
1992 Hurricane Iniki	Sep-92	1	3,144	58,248	58,248	58,248	58,248	58,248	58,248	58,248
1992 Hurricane Andrew	Aug-92	78	47,054	3,478	0	2	2,929	5,865	142,116	142,116
1991 Wildfires Oakland Hills	Oct-91	1	3,050	2,319	2,319	2,319	2,319	2,319	2,319	2,319
1991 Hurricane Bob	Aug-91	85	2,236	1,051	0	16	156	846	53,331	53,331
1990 Freeze California	Dec-90	34	9,358	16,056	568	4,469	27,319	82,902	248,856	248,856
1989 Winter Storm	Dec-89	438	330	88	0	3	86	233	1,059	12,921
1989 Earthquake Loma Prieta	Oct-89	8	11,627	7,920	970	3,202	40,220	40,220	40,220	40,220
1989 Hurricane Hugo	Sep-89	189	9,952	740	0	0	936	5,423	16,984	25,692
1988 Drought Heat Wave	Feb-88	355	4,934	2,491	1	160	1,389	11,359	51,322	125,363
All Events		**4,900**	**175,370**	**823**	**0**	**9**	**1,118**	**2,772**	**10,590**	**248,856**
Average Across Events		**213**	**7,625**	**4,495**	**2,859**	**3,179**	**6,707**	**10,623**	**31,266**	**41,065**

Table 2: Individual Investor Summary Statistics

This table reports summary statistics for retail brokerage account data based on natural disaster exposure. I construct account-day-stock level holdings using transaction data and restrict to account-days in which a sale occurs. Then, the sample is divided based on exposure to a natural disaster. For 3D clustering, standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011). The length assumed for a disaster impact to last is shown in four variations: 1 year (Panel A), 2 years (Panel B), 3 years (Panel C), and 4 years (Panel D).

PANEL A: 1 Year Disaster Impacts

		Disaster			No Disaster			
	Full Sample	All Obs	Gains	Losses	All Obs	Gains	Losses	DE Diff
N	827,430	146,366	81,761	64,605	681,064	388,375	292,689	
Sell Obs	199,182	35,691	23,142	12,549	163,491	106,305	57,186	
Sell Percent	0.241	0.244	0.283	0.194	0.240	0.274	0.195	
Disposition Effect (DE)	0.080	0.089			0.078			0.010***
t-stat								*(4.24)*
t-stat (3D clustering)								*(1.06)*
Return Mean	0.12	0.11	0.34	-0.19	0.13	0.37	-0.19	
10%	-0.29	-0.30	0.03	-0.46	-0.29	0.03	-0.45	
Median	0.03	0.03	0.17	-0.13	0.03	0.17	-0.13	
90%	0.54	0.51	0.79	-0.02	0.55	0.84	-0.02	

PANEL B: 2 Year Disaster Impacts

		Disaster			No Disaster			
	Full Sample	All Obs	Gains	Losses	All Obs	Gains	Losses	DE Diff
N	827,430	215,374	120,759	94,615	612,056	349,377	262,679	
Sell Obs	199,182	54,916	35,356	19,560	144,266	94,091	50,175	
Sell Percent	0.241	0.255	0.293	0.207	0.236	0.269	0.191	
Disposition Effect (DE)	0.080	0.086			0.078			0.008***
t-stat								*(3.61)*
t-stat (3D clustering)								*(0.91)*
Return Mean	0.12	0.10	0.33	-0.19	0.13	0.38	-0.19	
10%	-0.29	-0.29	0.03	-0.45	-0.29	0.03	-0.45	
Median	0.03	0.03	0.16	-0.13	0.03	0.18	-0.13	
90%	0.54	0.49	0.76	-0.02	0.56	0.85	-0.02	

PANEL C: 3 Year Disaster Impacts

		Disaster			No Disaster			
	Full Sample	All Obs	Gains	Losses	All Obs	Gains	Losses	DE Diff
N	827,430	316,652	179,187	137,465	510,778	290,949	219,829	
Sell Obs	199,182	80,176	52,147	28,029	119,006	77,300	41,706	
Sell Percent	0.241	0.253	0.291	0.204	0.233	0.266	0.190	
Disposition Effect (DE)	0.080	0.087			0.076			0.011***
t-stat								*(5.74)*
t-stat (3D clustering)								*(1.21)*
Return Mean	0.12	0.11	0.34	-0.19	0.13	0.38	-0.19	
10%	-0.29	-0.28	0.03	-0.45	-0.29	0.03	-0.46	
Median	0.03	0.03	0.17	-0.13	0.03	0.18	-0.13	
90%	0.54	0.51	0.78	-0.02	0.56	0.86	-0.02	

PANEL D: 4 Year Disaster Impacts

		Disaster			No Disaster			
	Full Sample	All Obs	Gains	Losses	All Obs	Gains	Losses	DE Diff
N	827,430	394,089	224,105	169,984	433,341	246,031	187,310	
Sell Obs	199,182	98,289	64,283	34,006	100,893	65,164	35,729	
Sell Percent	0.241	0.249	0.287	0.200	0.233	0.265	0.191	
Disposition Effect (DE)	0.080	0.087			0.074			0.013***
t-stat								*(6.70)*
t-stat (3D clustering)								*(1.24)*
Return Mean	0.12	0.12	0.35	-0.19	0.13	0.38	-0.19	
10%	-0.29	-0.28	0.03	-0.45	-0.29	0.03	-0.46	
Median	0.03	0.03	0.17	-0.13	0.03	0.18	-0.13	
90%	0.54	0.53	0.80	-0.02	0.56	0.85	-0.02	

Table 3: Natural Disaster Damage Per Capita Regressions

This table reports the results for regression equation (2) with various fixed effects controls using natural disaster damage per capita at the county-level to proxy for natural disaster exposure. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. I report four variations of *Damage* based on the length assumed for the impact to last: 1 year (Panel A), 2 years (Panel B), 3 years (Panel C), and 4 years (Panel D). All damage estimates are inflation adjusted to 2016 $USD. For columns 2-4, standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

PANEL A: 1 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.080***	0.080***	0.080***	0.091***
	(84.67)	(11.70)	(16.45)	(19.61)
Damage	0.001	0.001	-0.011**	-0.012*
	(0.29)	(0.18)	(-2.11)	(-1.92)
Gain * Damage	0.008	0.008	0.014**	0.015**
	(1.31)	(1.10)	(2.36)	(2.54)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
Clustering (Date, Account, Stock)	No	Yes	Yes	Yes

PANEL B: 2 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.080***	0.080***	0.080***	0.091***
	(84.24)	(11.70)	(16.45)	(19.61)
Damage	0.003	0.003	-0.005	-0.007
	(0.92)	(0.44)	(-0.92)	(-1.31)
Gain * Damage	0.019***	0.019**	0.019***	0.019***
	(3.83)	(2.04)	(3.43)	(3.64)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
Clustering (Date, Account, Stock)	No	Yes	Yes	Yes

PANEL C: 3 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.080***	0.080***	0.080***	0.091***
	(83.80)	(11.63)	(16.37)	(19.54)
Damage	0.008**	0.008	-0.002	-0.005
	(2.45)	(0.91)	(-0.33)	(-0.86)
Gain * Damage	0.014***	0.014	0.012**	0.012*
	(3.15)	(1.51)	(1.99)	(1.96)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
Clustering (Date, Account, Stock)	No	Yes	Yes	Yes

PANEL D: 4 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.080***	0.080***	0.080***	0.091***
	(83.53)	(11.63)	(16.37)	(19.54)
Damage	0.009***	0.009	0.002	0.000
	(3.12)	(1.11)	(0.28)	(0.03)
Gain * Damage	0.008**	0.008	0.007	0.007
	(2.04)	(1.02)	(1.28)	(1.23)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
Clustering (Date, Account, Stock)	No	Yes	Yes	Yes

Table 4: Natural Disaster Severity Indicator Regressions

This table reports regressions using dummy variables for the level of natural disaster severity. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. Each column represents different assumptions for the length assumed for a disaster to last. *Extreme* is equal to one if the given account-day is exposed to the top 99th percentile of damage ($10,590) across all disaster-counties, and zero otherwise. *Severe* is equal to one if the given account-day is exposed to damage below the 99th percentile of damage but greater than or equal to the 90th percentile across all disasters ($1,118), and zero otherwise. *Moderate* is equal to one if the given account-day is exposed to damage below the 90th percentile of damage but greater than zero across all disasters, and zero otherwise. Because the indicator for no disaster impacts is omitted, all interactions are interpreted as the increase in the disposition effect for each severity cohort from the no impact scenario. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

	(1)	(2)	(3)	(4)
Dependent Variable: Sale	1 Year	2 Years	3 Years	4 Years
Gain	0.090***	0.078***	0.090***	0.088***
	(18.50)	(10.81)	(16.13)	(14.52)
Extreme	-0.074***	-0.023	-0.038	-0.044
	(-4.40)	(-1.61)	(-1.45)	(-0.84)
Severe	-0.004	-0.000	-0.000	-0.004
	(-0.32)	(-0.02)	(-0.05)	(-0.54)
Moderate	-0.002	-0.002	-0.001	-0.001
	(-0.46)	(-0.53)	(-0.15)	(-0.15)
Gain * Extreme	0.086***	0.056*	0.031**	0.024*
	(3.91)	(1.81)	(1.99)	(1.72)
Gain * Severe	0.002	0.009	0.006	0.009
	(0.12)	(0.53)	(0.49)	(0.81)
Gain * Moderate	0.006	0.001	0.003	0.006
	(1.00)	(0.17)	(0.47)	(0.96)
Constant				
Observations	820,820	820,820	820,820	820,820
R-squared	0.242	0.242	0.242	0.242
Date FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes	Yes

Table 5: State-Matched Difference-in-Difference Regressions

This table reports difference-in-difference regressions using state-matched control samples for treated investors impacted by an extreme disaster. Extreme disasters are those in the top 99th percentile of damage across all disaster-counties ($10,590 per capita). I include all extreme events between January 1992 and November 1995 to ensure a full year before and after each event. I include all observations one year before and after each event in which the account owned a common stock position, and I require that the account owned at least one stock in both the year before and after the disaster event. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Treatment* is equal to one if the investors lives in a county impacted by an extreme disaster and zero for all investors in the same state that are unaffected. *After* equals one if the account-date is after the associated extreme disaster and zero if it is before the event. For columns 2-4, standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.00109***	0.00109***	0.00194***	0.00220***
	(15.60)	(6.93)	(9.82)	(10.90)
Gain*After	-0.00038***	-0.00038***	-0.00037***	-0.00025*
	(-4.04)	(-2.63)	(-2.58)	(-1.76)
Gain*Treatment	0.00034	0.00034	0.00026	-0.00010
	(0.46)	(0.30)	(0.23)	(-0.07)
After	0.00006	0.00006	0.00028**	0.00008
	(0.88)	(0.56)	(2.41)	(0.41)
Treatment	-0.00070	-0.00070	0.00019	-0.00048
	(-1.24)	(-1.60)	(0.20)	(-0.44)
After*Treatment	0.00048	0.00048	-0.00021	-0.00007
	(0.65)	(0.78)	(-0.36)	(-0.10)
Gain*After*Treatment	0.00212**	0.00212**	0.00245**	0.00241**
	(2.14)	(2.12)	(2.36)	(2.18)
Observations	4,871,527	4,871,527	4,871,517	4,871,512
R-squared	0.000	0.000	0.009	0.012
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
Clustering (Date, Account, Stock)	No	Yes	Yes	Yes

Table 6: Liquidity Constraints

This table tests the impact of liquidity constraints on the increased disposition effect of disaster-affected investors. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. In columns 1-3 of Panel A, I report subsample results of column 4 of Panel B in Table 3 based on the day of the month for the observation. Column 4 then adds the interaction the day of month with the previously reported interaction term to determine the impact of monthly seasonality potentially related to billing cycles. Similarly, Panel B reports subsets based on the investor's reported income. *Low Income* is defined to be one if the investor's yearly income is less than $75,000 USD, and zero otherwise. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

PANEL A: Within-Month Seasonality

	(1)	(2)	(3)	(4)
Dependent Variable: Sale	Day 1-10	Day 11-20	Day 21-31	Int
Gain	0.100***	0.091***	0.086***	0.104***
	(17.99)	(16.99)	(15.95)	(17.51)
Damage	-0.004	-0.018***	-0.010	-0.009
	(-0.58)	(-2.72)	(-1.06)	(-1.62)
Day of Month * Gain				-0.001***
				(-3.61)
Day of Month * Damage				0.000
				(0.41)
Gain * Damage	0.023***	0.024***	0.017**	0.021***
	(3.50)	(2.65)	(2.18)	(2.84)
Gain * Damage * Day of Month				-0.000
				(-0.33)
Observations	253,931	279,776	279,198	820,820
R-squared	0.247	0.244	0.239	0.242
Date FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes	Yes

PANEL B: Income

	(1)	(2)	(3)
Dependent Variable: Sale	High	Low	Difference
Gain	0.097***	0.090***	0.095***
	(16.92)	(14.16)	(16.95)
Damage	-0.015	-0.003	-0.015*
	(-1.53)	(-0.75)	(-1.71)
Low Income * Gain			-0.006
			(-0.77)
Low Income * Damage			0.012
			(1.24)
Gain * Damage	0.019**	0.022***	0.020**
	(2.08)	(3.67)	(2.32)
Gain * Damage * Low Income			0.001
			(0.13)
Observations	360,912	332,526	693,682
R-squared	0.246	0.253	0.244
Date FE	Yes	Yes	Yes
Account FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes

Table 7: Tax Incentives

This table tests the impact of tax incentives on the increased disposition effect of disaster-affected investors. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. The table reports subsets based on the tax laws regarding the type of account used. *Taxable* equals one if the given account is subject to yearly income taxes, and zero otherwise. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Sale	(1) Tax-exempt	(2) Taxable	(3) Difference
Gain	0.110***	0.088***	0.112***
	(13.92)	(17.51)	(14.69)
Damage	-0.027***	-0.004	-0.027***
	(-3.10)	(-0.69)	(-3.55)
Taxable * Gain			-0.026***
			(-3.13)
Taxable * Damage			0.023**
			(2.28)
Gain * Damage	0.031**	0.017***	0.030**
	(2.40)	(2.60)	(2.33)
Gain * Damage * Taxable			-0.014
			(-0.94)
Observations	148,727	671,839	820,820
R-squared	0.289	0.231	0.242
Date FE	Yes	Yes	Yes
Account FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes

Table 8: Local Stocks

This table tests the impact of local trading on the increased disposition effect of disaster-affected investors. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. I report subsample results of column 4 of Panel B in Table 3 based on the company's headquarter distance to the investor. *Local* is defined to be one if the firm is within 250 miles to the investor similar to Seasholes and Zhu (2010). All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Sale	(1) Non-Local	(2) Local	(3) Difference
Gain	0.085***	0.126***	0.090***
	(17.46)	(19.19)	(18.79)
Damage	-0.012*	-0.071	-0.010
	(-1.91)	(-1.14)	(-1.58)
Local * Gain			0.014***
			(4.25)
Local * Damage			-0.086*
			(-1.92)
Gain * Damage	0.017**	0.097	0.014*
	(2.35)	(1.17)	(1.96)
Gain * Damage * Local			0.093**
			(1.97)
Observations	542,162	147,662	695,068
R-squared	0.241	0.314	0.242
Date FE	Yes	Yes	Yes
Account FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes

Table 9: Ex-Post Returns

This table tests whether the increased disposition effect of disaster-affected investors is driven by informed trading. Similar to Odean (1998), this table compares average returns in excess of the CRSP value-weighted index and a stock-matched DGTW portfolio. I compare the subsequent performance of stocks that are sold (including partial sales) for a profit (referred to as realized gains) to stocks that the investor also holds on sale days but does not sell for a potential loss (referred to as paper losses). Returns are measured over the 252 trading days following a realized gain or a paper loss. *Extreme* observations are those in which the given account-day is exposed to the top 99th percentile of damage ($10,590) across all disaster-counties. *Severe* observations are those in which the given account-day is exposed to damage below the 99th percentile of damage but greater than or equal to the 90th percentile across all disasters ($1,118). *Moderate* observations occur if the given account-day is exposed to damage below the 90th percentile of damage but greater than zero across all disasters. *None* represents the account-days with no natural disaster exposure. All t-stats are calculated using standard errors that are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

	(1) All Obs	(2) Extreme	(3) Severe	(4) Moderate	(5) None
<u>Average Excess Returns</u>					
Paper Losses	-0.045	-0.133	-0.01	-0.045	-0.046
Realized Gains	0.004	-0.027	0.026	0.017	-0.001
Difference	-0.049***	-0.106**	-0.036*	-0.062***	-0.045***
t-stat	(-4.00)	(-2.21)	(-1.75)	(-4.71)	(-3.48)
<u>Average DGTW Returns</u>					
Paper Losses	-0.035	-0.127	-0.016	-0.034	-0.035
Realized Gains	-0.005	-0.069	-0.000	0.011	-0.010
Difference	-0.030**	-0.058*	-0.016	-0.045***	-0.025*
t-stat	(-2.26)	(-1.86)	(-0.79)	(-3.29)	(-1.79)

Table 10: Duration at Residence

This table tests the impact of the investors' duration at their listed residence on the increased disposition effect of disaster-affected investors. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. *Long Residence* is defined to be one if the given account has lived at its listed address for at least 10 years, and zero otherwise. All standard errors that are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Sale	(1) Short	(2) Long	(3) Difference
Gain	0.098***	0.085***	0.098***
	(17.65)	(12.55)	(17.74)
Damage	-0.008	-0.031	-0.009
	(-1.39)	(-1.23)	(-1.48)
Long Residence * Gain			-0.016**
			(-2.02)
Long Residence * Damage			-0.023
			(-0.91)
Gain * Damage	0.017***	0.089***	0.016***
	(3.42)	(2.83)	(3.18)
Gain * Damage * Long Residence			0.077**
			(2.40)
Observations	442,964	240,344	683,516
R-squared	0.248	0.250	0.244
Date FE	Yes	Yes	Yes
Account FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes

Table 11: Short-Term Reinvestment Probabilities

This table tests the short-term reinvestment probabilities based on natural disaster exposure. I define the dependent variable, *Reinvest*, to equal one if the investor purchases a stock different from the stock of sale within various periods following the sale. Each column refers to a different assumption for the reinvestment period. *Loss_Disaster* equals one if a disaster-affected account-day sells a loss, and zero otherwise. Similarly, *Loss_None* (*Gain_None*) is defined as one if an unaffected account-day sells a loss (gain). Note, *Gain_Disaster* is omitted from the regression, so all coefficients are interpreted as the increase in the reinvestment probability from the scenario in which a disaster-affect account-day sells a gain. I restrict to only those sale days in which one stock is sold (including partial sales) to avoid ambiguity. Disaster impacts are assumed to last for two years. All t-stats are calculated using standard errors that are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Reinvest	(1) 1 Day	(2) 5 Days	(3) 10 Days	(4) 15 Days	(5) 20 Days	(6) 25 Days
Loss_Disaster	0.052***	0.025***	0.024***	0.020***	0.017***	0.017***
	(9.32)	(4.68)	(4.41)	(3.76)	(3.15)	(3.33)
Loss_None	0.044***	0.021***	0.017***	0.017***	0.017***	0.018***
	(8.21)	(3.78)	(3.17)	(3.22)	(3.16)	(3.66)
Gain_None	-0.001	0.002	0.006	0.007	0.009*	0.010**
	(-0.18)	(0.36)	(1.19)	(1.35)	(1.91)	(2.19)
Observations	145,844	145,844	145,844	145,844	145,844	145,844
R-squared	0.325	0.362	0.393	0.414	0.429	0.441
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes	Yes	Yes	Yes

Appendix

"Disastrous Selling Decisions: The Disposition Effect and Natural Disasters"

Table A.1: Damage Per Capita Regressions for Extended Time Periods

This table reports the results for regression equation (2) with various fixed effects controls using natural disaster damage per capita at the county-level to proxy for natural disaster exposure. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. I report two variations of *Damage* based on the length assumed for the impact to last: 5 years (Panel A) and in perpetuity (Panel B). All damage estimates are inflation adjusted to 2016 $USD. For columns 2-4, standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

PANEL A: 5 Year Disaster Impacts

Dependent Variable: Sale	(0)	(1)	(3)	(5)
Gain	0.0800***	0.0801***	0.0799***	0.0911***
	(83.21)	(11.67)	(16.43)	(19.57)
Damage	0.0060**	0.0060	-0.0001	0.0002
	(2.11)	(0.74)	(-0.02)	(0.04)
Gain * Damage	0.0036	0.0036	0.0057	0.0051
	(0.95)	(0.46)	(0.97)	(0.87)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
Clustering (Date, Account, Stock)	No	Yes	Yes	Yes

PANEL B: Endless Disaster Impacts

Dependent Variable: Sale	(0)	(1)	(3)	(5)
Gain	0.0801***	0.0801***	0.0799***	0.0911***
	(83.65)	(11.67)	(16.43)	(19.57)
Damage	-0.0004	-0.0004	-0.0296***	0.0003
	(-0.33)	(-0.15)	(-3.15)	(0.04)
Gain * Damage	0.0002	0.0002	0.0027	0.0026
	(0.11)	(0.05)	(1.12)	(1.14)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
Clustering (Date, Account, Stock)	No	Yes	Yes	Yes

Table A.2: Additional Specifications using Severity Indicators

This table reports regressions using dummy variables for the level of natural disaster severity. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Extreme* is equal to one if the given account-day is exposed to the top 99th percentile of damage across all disaster-counties, and zero otherwise. *Severe* is equal to one if the given account-day is exposed to damage below the 99th percentile of damage but greater than or equal to the 90th percentile across all disasters, and zero otherwise. *Moderate* is equal to one if the given account-day is exposed to damage below the 90th percentile of damage but greater than zero across all disasters, and zero otherwise. For columns 2-4, standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

PANEL A: 1 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.078***	0.078***	0.079***	0.090***
	(75.19)	(10.81)	(15.41)	(18.50)
Extreme	-0.106***	-0.106***	-0.053***	-0.074***
	(-4.19)	(-2.97)	(-2.67)	(-4.40)
Severe	0.033***	0.033**	0.003	-0.004
	(5.41)	(2.15)	(0.31)	(-0.32)
Moderate	-0.004*	-0.004	-0.009**	-0.002
	(-1.84)	(-0.37)	(-2.15)	(-0.46)
Gain * Extreme	0.040	0.040*	0.053*	0.086***
	(1.16)	(1.80)	(1.84)	(3.91)
Gain * Severe	0.010	0.010	0.006	0.002
	(1.18)	(0.42)	(0.31)	(0.12)
Gain * Moderate	0.010***	0.010	0.006	0.006
	(4.08)	(1.00)	(0.89)	(1.00)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
3-way clustering	No	Yes	Yes	Yes

PANEL B: 2 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.078***	0.079***	0.091***	0.078***
	(71.25)	(14.56)	(17.68)	(10.81)
Extreme	-0.078***	-0.078***	-0.005	-0.023
	(-4.44)	(-2.88)	(-0.27)	(-1.61)
Severe	0.032***	0.032**	0.012	-0.000
	(7.97)	(2.39)	(1.09)	(-0.02)
Moderate	0.014***	0.014*	-0.000	-0.002
	(8.27)	(1.68)	(-0.08)	(-0.53)
Gain * Extreme	0.044*	0.044	0.031	0.056*
	(1.65)	(1.13)	(0.81)	(1.81)
Gain * Severe	0.021***	0.021	0.013	0.009
	(3.97)	(1.14)	(0.79)	(0.53)
Gain * Moderate	0.005**	0.005	0.002	0.001
	(2.38)	(0.61)	(0.30)	(0.17)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
3-way clustering	No	Yes	Yes	Yes

PANEL C: 3 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.076***	0.076***	0.078***	0.090***
	(63.17)	(8.63)	(13.08)	(16.13)
Extreme	-0.075***	-0.075**	-0.026**	-0.038
	(-5.07)	(-1.99)	(-2.00)	(-1.45)
Severe	0.025***	0.025**	0.011	-0.000
	(8.14)	(1.96)	(1.15)	(-0.05)
Moderate	0.013***	0.013	0.000	-0.001
	(8.33)	(1.39)	(0.05)	(-0.15)
Gain * Extreme	0.022	0.022	0.007	0.031**
	(1.00)	(0.90)	(0.38)	(1.99)
Gain * Severe	0.014***	0.014	0.009	0.006
	(3.35)	(0.82)	(0.70)	(0.49)
Gain * Moderate	0.010***	0.010	0.004	0.003
	(5.05)	(1.09)	(0.55)	(0.47)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
3-way clustering	No	Yes	Yes	Yes

PANEL D: 4 Year Disaster Impacts

Dependent Variable: Sale	(1)	(2)	(3)	(4)
Gain	0.074***	0.074***	0.076***	0.088***
	(56.79)	(7.52)	(11.63)	(14.52)
Extreme	-0.067***	-0.067	-0.056	-0.044
	(-4.88)	(-1.52)	(-1.52)	(-0.84)
Severe	0.014***	0.014	0.004	-0.004
	(5.28)	(1.17)	(0.49)	(-0.54)
Moderate	0.009***	0.009	-0.006	-0.001
	(5.88)	(0.87)	(-1.18)	(-0.15)
Gain * Extreme	0.018	0.018	-0.001	0.024*
	(0.89)	(0.82)	(-0.07)	(1.72)
Gain * Severe	0.011***	0.011	0.013	0.009
	(3.00)	(0.71)	(1.09)	(0.81)
Gain * Moderate	0.013***	0.013	0.007	0.006
	(6.42)	(1.21)	(1.03)	(0.96)
Observations	827,430	827,430	821,012	820,820
R-squared	0.009	0.009	0.221	0.242
Date FE	No	No	No	Yes
Account FE	No	No	Yes	Yes
Stock FE	No	No	No	Yes
3-way clustering	No	Yes	Yes	Yes

Table A.3: State-Matched Difference-in-Difference Summary Statistics

This table reports summary statistics for difference-in-difference tests around an extreme disaster. Extreme disasters are those in the top 99th percentile of damage across all disaster-counties ($10,590 per capita). I include all extreme events between January 1992 and November 1995 to ensure a full year before and after each event. The treatment group of investors are those that live in a county impacted by an extreme disaster, and the control group consists of unaffected investors that live in the same state as those impacted. I include all observations one year before and after each event in which the account owned a common stock position. I also require that the account owned at least one stock in both the year before and after the disaster event.

				Stock Returns			
	N	Sell Obs	% Sell	Mean	10%	Median	90%
All Account-Stock-Dates	**4,871,527**	**12,569**	**0.26%**	**0.12**	**-0.33**	**0.03**	**0.58**
Before Event	**2,136,475**	**5,655**	**0.26%**	**0.08**	**-0.30**	**0.02**	**0.46**
Treatment	18,874	41	0.22%	0.05	-0.20	0.03	0.36
Stock at a Gain	10,758	30	0.28%	0.22	0.02	0.12	0.53
Stock at a Loss	8,116	11	0.14%	-0.17	-0.32	-0.10	-0.01
Control	2,117,601	5,614	0.27%	0.08	-0.30	0.02	0.46
Stock at a Gain	1,154,780	3,635	0.31%	0.31	0.03	0.17	0.69
Stock at a Loss	962,821	1,979	0.21%	-0.20	-0.47	-0.13	-0.02
After Event	**2,735,052**	**6,914**	**0.25%**	**0.14**	**-0.35**	**0.04**	**0.69**
Treatment	24,843	89	0.36%	0.08	-0.33	0.02	0.49
Stock at a Gain	13,219	67	0.51%	0.33	0.03	0.18	0.97
Stock at a Loss	11,624	22	0.19%	-0.20	-0.47	-0.15	-0.02
Control	2,710,209	6,825	0.25%	0.15	-0.35	0.04	0.69
Stock at a Gain	1,522,121	4,310	0.28%	0.43	0.04	0.23	1.00
Stock at a Loss	1,188,088	2,515	0.21%	-0.22	-0.52	-0.16	-0.02

Table A.4: Income Level Robustness

This table tests the robustness of liquidity constraints related to income on the increased disposition effect of disaster-affected investors. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. Columns 1 and 2 reports subset based on the investor's reported income. *Low Income* is defined to be one if an investor's yearly income is less than $50,000 USD, and zero otherwise. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

	(1)	(2)	(3)
Dependent Variable: Sale	High	Low	Difference
Gain	0.092***	0.096***	0.092***
	(17.48)	(12.99)	(17.55)
Damage	-0.013	-0.005	-0.013
	(-1.33)	(-0.91)	(-1.47)
Low Income * Gain			0.004
			(0.48)
Low Income * Damage			0.009
			(0.90)
Gain * Damage	0.021**	0.022***	0.021**
	(2.38)	(3.54)	(2.50)
Gain * Damage * Low Income			-0.001
			(-0.07)
Observations	526,661	166,730	693,682
R-squared	0.244	0.261	0.244
Date FE	Yes	Yes	Yes
Account FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes

Table A.5: Job Sophistication Robustness

This table tests the impact of job sophistication on the increased disposition effect of disaster-affected investors. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. In Panel A, I report subsample results of column 4 of Panel B in Table 3 based on the profession of the investor following the definitions for professional employment from Dhar and Zhu (2006). *Non-Professional* equals one if the investor does not work in a professional role, and zero otherwise. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Sale	(1) Professional	(2) Non-Prof	(3) Difference
Gain	0.096***	0.136***	0.096***
	(13.88)	(8.72)	(13.99)
Damage	-0.019**	-0.005	-0.020***
	(-2.51)	(-0.37)	(-2.90)
Non-Professional * Gain			0.020
			(1.29)
Non-Professional * Damage			0.029**
			(2.51)
Gain * Damage	0.026**	0.005	0.027**
	(2.29)	(0.29)	(2.43)
Gain * Damage * Non-Professional			-0.023
			(-1.47)
Observations	269,984	33,515	303,827
R-squared	0.255	0.309	0.256
Date FE	Yes	Yes	Yes
Account FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes

Table A.6: Ex-Post Return Investment Horizon Robustness

This table tests whether the increased disposition effect of disaster-affected investors is driven by informed trading for additional investment horizons. Similar to Odean (1998), this table compares average returns in excess of the CRSP value-weighted index and a stock-matched DGTW portfolio. I compare the subsequent performance of stocks that are sold (including partial sales) for a profit (referred to as realized gains) to stocks that the investor also holds on sale days but does not sell for a potential loss (referred to as paper losses). Returns are measured over the subsequent 84 trading days (Panel A) and 504 trading days (Panel B) following a realized gain or a paper loss. *Extreme* observations are those in which the given account-day is exposed to the top 99th percentile of damage ($10,590) across all disaster-counties. *Severe* observations are those in which the given account-day is exposed to damage below the 99th percentile of damage but greater than or equal to the 90th percentile across all disasters ($1,118). *Moderate* observations occur if the given account-day is exposed to damage below the 90th percentile of damage but greater than zero across all disasters. *None* represents the account-days with no natural disaster exposure. All t-stats are calculated using standard errors that are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

PANEL A: 84 Days

	(1) All Obs	(2) Extreme	(3) Severe	(4) Moderate	(5) None
Average Excess Returns					
Paper Losses	-0.015	-0.039	-0.007	-0.011	-0.017
Realized Gains	-0.004	-0.030	-0.001	0.003	-0.007
Difference	-0.011**	-0.009	-0.006	-0.014***	-0.010**
t-stat	(-2.42)	(-0.33)	(-0.64)	(-2.83)	(-2.08)
Average DGTW Returns					
Paper Losses	-0.015	-0.019	-0.012	-0.01	-0.016
Realized Gains	-0.010	-0.061	-0.008	-0.004	-0.011
Difference	-0.005	0.042	-0.004	-0.006	-0.005
t-stat	(-1.14)	(1.38)	(-0.38)	(-1.31)	(-0.99)

PANEL B: 504 Days

	(1) All Obs	(2) Extreme	(3) Severe	(4) Moderate	(5) None
Average Excess Returns					
Paper Losses	-0.12	-0.294	-0.027	-0.101	-0.13
Realized Gains	-0.002	-0.148	0.097	0.022	-0.015
Difference	-0.118***	-0.146**	-0.124***	-0.123***	-0.115***
t-stat	(-4.48)	(-2.12)	(-2.92)	(-4.34)	(-4.27)
Average DGTW Returns					
Paper Losses	-0.088	-0.25	-0.051	-0.063	-0.096
Realized Gains	-0.008	-0.229	0.050	0.011	-0.016
Difference	-0.080***	-0.021	-0.101**	-0.074**	-0.080***
t-stat	(-3.04)	(-0.37)	(-2.44)	(-2.53)	(-3.02)

Table A.7: Homeownership

This table tests the impact of homeownership on the increased disposition effect of disaster-affected investors. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the given observation, and zero otherwise. I define *Gain* to be one if the return to the investor is positive, and zero otherwise. *Damage* is equal to the per capita dollar damage to the county in which the account resides. For easier interpretation, *Damage* is scaled by $10,000. *Homeownership* is equal to one if the investor owns his/her home, and zero otherwise. All standard errors that are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Sale	(1) Rent	(2) Own	(3) Difference
Gain	0.085***	0.095***	0.076***
	(5.09)	(18.99)	(6.10)
Damage	-0.027*	-0.010	0.003
	(-1.77)	(-1.64)	(0.60)
Homeownership * Gain			0.018
			(1.40)
Homeownership * Damage			-0.014
			(-1.59)
Gain * Damage	0.012	0.020***	0.010*
	(0.67)	(3.56)	(1.71)
Gain * Damage * Homeownership			0.010
			(1.10)
Observations	16,018	623,350	639,737
R-squared	0.355	0.243	0.244
Date FE	Yes	Yes	Yes
Account FE	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes
Clustering (Date, Account, Stock)	Yes	Yes	Yes