March 2020

Essays on the Disposition Effect

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by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy in Business Administration
with a concentration in Finance
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Date of Approval:
March 5, 2020

Keywords: Investor Behavior, Realization Utility, Natural Disasters, Portfolio Performance

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ABSTRACT

This dissertation contains two essays that shed new light on the disposition effect – that is, the tendency for investors to be especially eager to realize gains over losses. The first essay combines county-level natural disaster data with individual investor transactions to 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 needs, 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. In the second essay, I find no material disposition effect for a stock if the remaining portfolio is at a gain. I find a large disposition effect only when the remaining portfolio is at a loss. This portfolio-driven disposition effect is not explained by extreme returns, portfolio rebalancing, simultaneous transactions, or investor sophistication/skill. The evidence suggests investors' utility comes from both paper gains and losses and realized gains and losses; and when their portfolio has paper losses, they compensate by realizing gains. Together, these essays find the disposition effect increases (decreases) with negative (positive) experiences. These findings suggest investors exhibit the disposition effect to offset negative utility from other salient channels, even though this behavior is costly to their future portfolio performance.

ESSAY 1 – DISASTROUS SELLING DECISIONS: THE DISPOSITION EFFECT AND NATURAL DISASTERS

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 sell assets that are at a gain compared to assets 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 U.S. retail investors, unexplained by rational mechanisms, and costly to investors.[2]

Most theoretical explanations for the disposition effect rely on investor preferences. Barberis and Xiong (2009) show that the disposition effect is generated in a model of prospect theory preferences with realization utility. Chang, Solomon, and Westerfield (2016) extend this model by incorporating disutility from paper losses and documenting evidence of cognitive dissonance. Recently, An et al. (2019) find robust empirical evidence that the disposition effect increases dramatically when an investor's other holdings are performing poorly and propose 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

[1] See Grinblatt and Keloharju (2001), Shapira and Venezia (2001), Genesove and Mayer (2001), and Heath, Huddart, and Lang (1999), among others.
[2] Odean (1998) shows the disposition effect is not explained by trade costs, portfolio rebalancing, tax considerations, and informed trading. Furthermore, he documents that the trades driving the disposition effect cost investors about 3.4% in market-adjusted annual returns.
[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

extend this question beyond an investor's portfolio. Instead, I ask: does an investor's choice to sell securities depend on utility from significant sources outside the investor's portfolio?

To answer this question, I employ natural disaster exposure as an exogenous source of variation. In this setting, natural disasters serve two primary purposes: (1) they are significant individual-specific events and (2) they are random. 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, a meaningful amount may be realized at the time of the event. Yet, if the disaster was especially impactful to the community, it is possible that housing values may depreciate in the short-run, indicating a paper loss to investors (assuming they do not immediately sell their affected property).

Researchers have shown investors receive a "burst" of utility (disutility) when they realize gains (losses).[4] If their utility is negatively shocked by a natural disaster, then they might be especially eager (reluctant) to receive a positive (negative) burst following a disaster event. Thus, the disposition effect may increase after natural disaster exposure because the marginal utility of realizing a gain (loss) increases (decreases). 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.

Based on the expected impact to investor utility, I hypothesize an increased disposition effect for disaster-affected individuals. Additionally, if the disposition effect allows investors to offset disutility caused by their environment, then the disposition effect may increase with disaster severity. Moreover, the increase could be strongest when the natural disaster is most salient and diminish over time. These relationships are precisely what I find in the data. For the most extreme disasters, the

gains/losses, see Barberis and Xiong (2009), Barberis and Xiong (2012), Henderson (2012), and Ingersoll and Jin (2013).

[4] See Barberis and Xiong (2012), Henderson (2012), Ingersoll and Jin (2013), and Frydman et. al. (2014).

disposition effect increases 51-96% in the year following the event, while more moderate disasters only increase the disposition effect by 2-13%. These relationships reduce significantly after two years, and after three years, the impact no longer persists.

I document the relationship between natural disasters and the disposition effect in univariate statistics as well as panel ordinary least squares (OLS) regressions with a host of fixed effects controls. First, I show with simple summary statistics tests that *any* level of disaster damage is associated with a larger disposition effect. Next, I test if the disposition effect is positively related to the county-level 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. Finally, I show the results are robust across disaster types and seasons.

Subsequently, I consider potential mechanisms for the increased disposition effect of disaster-affected investors. The evidence indicates the increase is not driven by liquidity constraints, tax incentives, or informed trading. In fact, the disposition effect of extreme disaster-affected individuals costs them -10.6% (t-stat -2.21) in future market-adjusted annual returns.[5] 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.

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

Based on these findings, I hypothesize that investors receive a negative utility shock via their environment, and afterward, 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.[6]

Frydman, Hartzmark, and Solomon (2018) find that reinvesting a sale into a different stock allows an 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 at the detriment of their future portfolio performance.

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. Section 6 performs several robustness checks, and Section 7 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

[6] It is worth noting this intuition follows even after I show investor liquidity constraints do 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.

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. Most closely related to my setting, Bharath and Cho (2019) document a long-run reduction in risky asset investment for natural disaster-affected individuals. Overall, these studies tend to focus on stock market participation rather than choices within securities. I contribute to this area by studying asset-level individual selling decisions, specifically focusing on 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.[7] 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,

[7] 1.9 million = 104,000 * 1,497 * 3.5 * 71% * 0.5%. The sale condition is a key part of the econometric design as it removes the decision that prompts a sale (such as a liquidity event) and instead focuses on the choice of asset to sell, given a sale occurring. I expand on this condition further in Section 3.3.

I require all stocks to have at least one day of active trading in the preceding 250 trading days. Fourth, I remove positions held at the start of the period because the initial purchase price cannot be determined. 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 retail investor 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.[8] 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.[9] Disaster types include droughts, thunderstorms, hail, hurricanes, winter storms, landslides, floods, volcanoes,

[8] 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.

[9] 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.

wildfires, and tornados. While the entire SHELDUS data includes events from 1965 to 2015, I focus 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 impact I include is four years and the investor data begin January of 1991.[10] Over this time period, SHELDUS includes 23 named events. Table 1.1 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.

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.[11] 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, 1-4 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) and Bharath and Cho (2019) 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 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

[10] 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 three years.

[11] 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.

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 a gain realized (PGR) and the probability of a loss realized (PLR). To measure how a natural disaster affects an investor's disposition effect, I employ a panel OLS 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 linear probability model, 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. 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 a natural disaster. Therefore, the sum of β_1 and β_3 measures the disposition effect for observations exposed to a natural disaster. In addition to this baseline specification, I add fixed effects for account, day, and stock. This ensures that the relationship between disasters and the disposition effect is not driven by sale propensities of a given account, day, or stock, and consequently controls for heterogeneity of trading activity across accounts, attention across dates (such as earnings announcements), and trading volume across stocks.

It is salient to note once more that the sample of analysis is conditioned on the investor selling at least one share of stock on that particular day to be included. This condition is particularly important in this setting as natural disaster damages may induce a liquidity shock to investors, and as a result, their unconditional selling probability may increase. Thus, the difference in the unconditional probability of realizing a gain versus a loss may simply be driven by an overall increase in selling from any liquidity event. While I find this story is inconsistent with the data in Section 5.1, the sale condition also helps to mitigate this concern from the start. Moreover, when measuring investor choices, it only makes sense to include those observations in which investors are paying attention to their accounts. Consequently, removing the sale condition would imply investors are always paying attention to every holding in their portfolio. Admittingly, some days in which the investor is paying attention and does not choose to sell will be omitted in the sale conditioned sample, and therefore, the ideal sample probably lies somewhere between the unconditional and sale conditioned sample. Gargano and Rossi (2018) examine log-in data from 2013-2014 and show that investors log-in about six times more often than they trade.[12] Additionally, in this sample, traders sell about one time per year on average. Thus, it follows that an unconditional sample would add roughly five days of relevant data points and more than 200 days of noise for every sale date. For these reasons, the sale conditioned interpretation is the primary method of measurement.

4. The Disposition Effect and Natural Disasters

4.1. Disaster Exposure

First, I examine 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.

Table 1.2 displays summary statistics of the sample split. 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%.[13] 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

[12] This estimate acts as an upper bound when extrapolating for investors from 1991-1996 (since brokerage accessibility has increased over time due to technology).
[13] This difference is equivalent to the interaction coefficient from equation (2).

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.

Table 1.2 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 common and does not elicit a significant shock. Instead, it is likely that severe impacts are less common and thus more meaningful. From Table 1.1, 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.

Table 1.3 displays the regressions of equation (2) using *Damage* instead of the *Disaster* dummy variable with a host of fixed effect controls. In addition to using robust standard errors clustered by account, day, and stock, column 4 of Table 1.3 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 results are 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.[14]

4.2. Disaster Severity Indicators

Although I calculate the average linear effect of disaster dollar damages in Table 1.3, 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*.[15] 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 0.2% as *Extreme*. Across each of these subsets, the disposition effect persists.

Figure 1.1 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,

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[14] 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.
[15] From Table 1.1, the 99th (90th) percentile is $10,590 ($1,118).

the difference between PGR and 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.

In Table 1.4, I report regressions with severity indicator variables and their interactions with *Gain*. Since *None* is omitted, the interaction coefficients 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.[16] After 2 years, the

[16] Appendix Table A.2 shows all four specifications used in Table 1.3 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.

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. Disaster Seasons and Types

This study is not limited to a single disaster event, but instead uses all 23 disaster events over the sample period. These events include hurricanes, tropical storms, earthquakes, wildfires, droughts, winter storms, blizzards, tornadoes, and other severe weather. Moreover, they are not restricted to a certain time of the year. It is possible that the results are concentrated in certain event types or certain seasons within the year. Therefore, I analyze the relationship between disaster severity and the disposition effect across different seasons and event types. Because events vary in the level of severity and some do not reach severe or extreme cutoff points, I focus on examining the specification that uses the level of per capita damage from column 4 of Panel B in Table 1.3.

Panel A of Table 1.5 divides the sample into the following mutually exclusive seasons: *Winter, Spring, Summer, Fall,* and *December*. *Winter* represents those observations in January and February, *Spring* includes March through May, *Summer* includes June through August, *Fall* includes September through November, and *December* includes only the month of December. The interaction coefficient is significant in all subsets except for December. The lack of results in December is not surprising given Odean (1998) shows that December is the only month in which retail investors actually exhibit a reverse disposition effect due to tax-loss selling. Investors harvest losses at the end of the year to reduce tax liability. Interestingly, this tax-loss selling is more rampant among severely disaster-affected observations as indicated by a negative coefficient, albeit the relationship is not statistically significant. Still, for the remaining four seasons, the strong positive relationship between disaster severity and the disposition effect persists with similar economic and statistical significance across all as the interaction coefficients range from 1.9% to 3.2% (t-stats 2.39 to 3.84). Thus, the main results are robust across seasons with the exception of December.

In Panel B of Table 1.5, I analyze the increased disposition effect of disaster-affected investors across different event types. Columns 1-3 include only the disaster observations identified by *Hurricanes/Tropical Storms*, *Blizzards/Winter Storms*, and *Other*, respectively. *Other* includes earthquakes, droughts, wildfire, and other severe weather.[17] While all three disaster types have a positive interaction coefficient, the *Other* category is the only which is not statistically significant. Thus, the statistical significance of the main results is concentrated in hurricanes, tropical storms, blizzards, and winter storms. To ensure the results are robust to a majority of events, columns 4 and 5 report the full specification with additional event fixed effects and event*gain fixed effects controls, respectively. These fixed effects control for the variation in the propensity to sell across events as well as the variation in the disposition effect across events, respectively. In both robustness tests, the interaction coefficient is similar in economic and statistical significance to the main specification with values of 1.8% (t-stat 3.51) and 2.2% (t-stat 5.22) in columns 4 and 5, respectively. Consequently, I conclude that although some disaster types elicit a stronger response than others, the positive relationship between natural disaster severity and the disposition effect is robust across disaster types.

5. Potential Explanations and Mechanisms

In this section, I examine several potential mechanisms that may drive the increased disposition effect of disaster-affected investors. Because not all investors have demographic information available, the combination of any subsamples used in this section is sometimes smaller

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[17] Although it may seem worthwhile to further examine within this *Other* category, power becomes difficult to obtain as there are usually only one or two events in these sub-categories, and many events do not have a significant number of accounts in the affected areas to achieve power individually.

than the entire sample. Additionally, I continue to focus on the specification from column 4 of Panel B in Table 1.3 when diagnosing 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

To begin, 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. While investors may experience short-term inattention due the severity of the event, they may ultimately be hit with a liquidity shock if investors have limited access to funds for damages. In this case, investors could partially liquidate all holdings to meet their liquidity needs if they truly believe their holdings are chosen optimally. On the other hand, individuals may exhibit the disposition effect because gains generally have more equity, and it is less costly to simply liquidate one larger holding. If this mechanism drives the increased disposition effect, then it also may be more prevalent around bill pay cycles and for households with less access to alternate funds. I test this mechanism in three steps. First, I examine abnormal selling propensities around natural disaster events. Second, I add controls variables for asset and portfolio equity values. Third, I analyze subsamples to determine the relationship of the increased disposition effect with bill pay seasonality and investor income levels.

If a natural disaster event generates a liquidity shock, then the probability of a sale occurring in an affected county should significantly increase following that event. To test abnormal selling probabilities after a natural disaster, I construct a panel dataset at the county-quarter level of unconditional sale probabilities. The sale condition is dropped here because a sale conditioned probability expresses *what* is sold, given a sale occurs, while an unconditional sale probability indicates *when* a sale occurs, which is the purpose of this test. Similar in spirit to Cortes and Strahan (2017),[18] I regress these unconditional sale probabilities at the county-quarter level on county and quarter fixed effects plus event-quarter indicator dummies around severe and extreme disaster events (defined in Section 4.2) as follows:

$$SaleProbability_{j,t} = \alpha_j + \gamma_t + \sum \beta^k D_{j,t}^k + \epsilon_{j,t} \tag{3}$$

where *j* indexes counties and *t* indexes quarters. County fixed effects are α_j, and quarter fixed effects are γ_t.[19] Event-quarter indicators are denoted as $D_{j,t}^k$ and span $k = -1$ to $k = 8$, where $k = 0$ represents the quarter of the disaster event. Under this specification, the β^k coefficients estimate abnormal selling probabilities relative to each county's average over the sample period and each quarter's average across all counties.

Figure 1.2 charts the β^k coefficients as the black line and 95% confidence intervals as the shaded grey area. All quarters struggle to show any statistical significance. In fact, the only two quarters that come close are the event quarter $(k = 0)$ and the following quarter $(k = 1)$ where the coefficients are negative but not statistically significant with t-stats of -1.36 and -1.38, respectively. This evidence is inconsistent with a positive abnormal selling probability following a disaster event. Instead, there seems to be mild short-term inattention (likely due to the severe event taking attention

[18] Cortes and Strahan (2017) use the same methodology to study abnormal home mortgage loan demand following a natural disaster.
[19] In Appendix Figure A.1, I also analyze abnormal selling around moderate disasters and the same qualitative patterns persist.

away) followed by average levels of selling activity. Thus, I conclude that these disaster events do not elicit an abnormal number of liquidity events.

Although natural disasters do not increase the frequency of sales in an affected county, liquidity needs could still be a potential mechanism of the increased disposition effect simply because gains generally represent more equity and making multiple trades is costly to investors. To test this liquidity mechanism, I add several additional control variables to the main specification from column 4 of Panel B of Table 1.3.

Table 1.6 adds the following additional variables: *Asset Value*, *Portfolio Size*, and *Asset Proportion*. *Asset Value* is the dollar value of the asset to the given investor on the day prior, scaled by $100,000 for easier interpretation. *Portfolio Size* is the dollar value of the investor's stock portfolio on the day prior, scaled by $1,000,000. *Asset Proportion* is the value-weighted proportion of the asset in the investor's stock portfolio on the day prior. Columns 1-3 add each variable respectively, and column 4 adds all three. Still, the coefficient of interest (*Gain*Damage*) is practically unchanged in all variations. Although the additional control variables show statistical significance at times, they are all economically small. Thus, the asset value under consideration, the portfolio value of the investor, and the asset's relative value to the investor's portfolio do not explain the increased disposition effect of disaster-affected investors.

For a final test of the liquidity constraints mechanism, I employ a subsample analysis dividing observations by within-month 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 the sample as about 97% of the investors with residence information are identified as homeowners. It naturally follows then to ask: does the disposition effect for disaster-affected investors exhibit within-month seasonality? If these individuals 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.

Columns 1-3 of Table 1.7 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 at the 1% level (t-stats range from 2.18 to 3.50).

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 test the impact of investor income levels. Similar to the tests for within-month seasonality, I 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. Columns 4 and 5 of Table 1.6 conducts the tests on these sample splits, and the results are nearly identical for both subsamples.[20]

After examining abnormal sale probabilities, asset/portfolio controls variables, within-month seasonality, and investor income levels, I conclude that the liquidity constraints mechanism does not explain any significant portion 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.

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[20] The results are qualitatively similar for other income cutoff points. In Appendix Table A.3, 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.4 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 not explained by a simple liquidity shock, investors may still act rationally through a tax incentive. Odean (1998) documents that individuals 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.

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-exempt accounts (such as IRAs and Keogh plans). In columns 6 and 7 of Table 1.7, I 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). 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.

I test the impact of local trading on the disposition effect for disaster-affected individuals in Table 1.8. 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%

[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.

[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.

- 1) for local trading. While 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 found evidence inconsistent with 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.

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.

Table 1.9 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

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[24] See Daniel, Grinblatt, Titman, and Wermers (1997).

[25] Appendix Table A.5 shows the results are robust to shorter (84 days) and longer (504 days) investment horizons similar to Odean (1998).

losses should outperform realized gains, and the difference should be positive and statistically significant.

In the top portion of Table 1.9, returns are calculated in excess of the CRSP value-weighted index. The first column confirms Odean's (1998) result (the aggregate disposition effect does not reflect informed trading). 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 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 1.9 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-

[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 Wermers' website, http://terpconnect.umd.edu/~wermers/ftpsite/Dgtw/coverpage.htm.

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 exhibiting the disposition effect since realizing gains (losses) has been shown to cause a burst in utility (disutility). I hypothesize that the magnitude of the external shock will be greater for individuals with stronger ties to their community, and therefore the increased disposition effect should be even more pronounced for these individuals.

To proxy for the connectedness of individuals to their community, I use demographic information on the duration investors have lived at their 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. Columns 1 and 2 of Table 1.10 display the split sample results based on the investors' duration at their residence. The coefficient of *Gain*Damage* 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]

[27] Appendix Table A.6 tests if the effect is stronger for homeowners compared to renters. Unfortunately, the sample of identified renters is extremely small (3% of the households with demographic information), so

While the duration at residence is a valid proxy for individuals' ties to their local community, the decisions investors make after the sale also may be insightful. What do 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 reinvested into a different stock. This logic generates two predictions regarding gain/loss sales after a natural disaster. If individuals are attempting to 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.

Table 1.11 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 1-4 show reinvestment periods of 5 days, 10 days, 15 days, and 20 days, respectively. *Gain_Disaster* is omitted, so all coefficients represent the increase in the reinvestment probability from the *Gain_Disaster* scenario.

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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.

[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.

Panel A of Table 1.11 shows the results using all accounts. Because withdrawals in tax exempt accounts are unlikely to occur due to penalties, Panel B then restricts to only those accounts that are subject to income taxes. Both panels show similar qualitative results. Consistent with prediction (1), all coefficients 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 4 of Panel A and columns 2-4 of Panel B. For prediction (2) to hold, *Loss_Disaster* should be the largest coefficient. This occurs in columns 1-3 of both panels (any 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 all periods, with the stronger evidence using at least a 10-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. Robustness

This section tests the robustness of the increased disposition effect for disaster-affected investors. Table 1.12 reports a series of robustness checks. Column 1 shows the main specification from column 4 of Panel B in Table 1.3 for easy comparison to the various robustness specifications.

Recall that the sample of analysis is at the account-day-stock level and only includes those account-days in which at least one sale occurs (also referred to as the sale condition). Although I analyze the propensity to sell gains versus losses, there is no requirement that the account-day hold both a gain and a loss. Therefore, it is possible that the eagerness (reluctance) of selling gains (losses) may be driven by different accounts that happen to hold either all gains or all losses and that the overall effect is simply aggregating across these accounts. Column 2 adds the restriction that each account-day must hold at least one gain and one loss so that the investor always has a choice between gains and losses. The interaction coefficient actually increases slightly in statistical and economic significance with a measure of 2.5% (t-stat 4.22).

Additionally, because the sample is sale conditioned it may be the case that only especially active traders drive the results. Although account fixed effects should alleviate this concern, column 3 reports the results when those with an "active" classification in the brokerage data are removed. This classification is made to those accounts that average at least 48 trades per year and roughly reflects the top 8% of the most active traders. The coefficient of interest remains similar in magnitude and statistical significance with a value of 1.8% (t-stat 3.44). However, it is worth noting that the economic significance actually increases substantially because the coefficient of *Gain* is now only 1.8% (3.66). Recall that the coefficient of *Gain* is the disposition effect for those observations not impacted by a disaster. That means that in this subset of less active accounts, $10,000 worth of per capita damage increases the disposition effect by 100% (1.8%/1.8%) instead of 21% (1.9%/9.1%) from column 1.

Ben-David and Hirshleifer (2012) show that the disposition effect is not monotonic across asset returns levels but instead reflects a v-shaped pattern with higher sale propensities for extreme winners and losers. To control for this pattern and consequently the return level of the holding, I add return size fixed effects similar to An et al. (2019). More specifically, I group all observations by holding period return into 50 brackets: (-∞, -50%), …, [-4%, -2%), [-2%, 0), [0, 2%), [2%, 4%), …, [50%, -∞). Column 4 then reports the results when including a fixed effect for each one of these return brackets. Still, the main results persist with an interaction coefficient of 2.1% (t-stat 5.17).

Furthermore, the main results are measured using a linear probability model, which calculates the disposition effect as the *difference* in PGR and PLR. For additional robustness, I estimate a Cox proportional hazard model with time-varying covariates, which defines the disposition effect at the *ratio* of PGR and PLR similar to Seru, Shumway, and Stoffman (2010). I count every purchase of a stock as a new position and assume the position ends on the date the investor liquidates the holding to estimate:

$$h_{i,j,t} = \phi_t exp\,\{\beta_1 Gain_{i,j,t} + \beta_2 Damage_{i,t} + \beta_3 Gain_{i,j,t} \times Damage_{i,t} + \gamma X\} \qquad (4)$$

where the hazard rate, $h_{i,j,t}$, is account *i*'s probability of liquidating holding *j* at time *t* conditional on not liquidating at time *t*-1, and ϕ_t is the baseline hazard. A benefit of Cox's (1972) proportional hazard model is that the partial likelihood approach allows measurement of the coefficients without estimating the baseline hazard. For inference, $exp(\beta_1)$ measures the ratio of PGR and PLR for observations not impacted by a natural disaster and $exp(\beta_3)$ measures the increasing effect of per capita damage estimates on the disposition effect ratio.

Column 5 of Table 1.12 reports the hazard model results. I stratify by account, which is similar to fixed effects in a linear probability model.[29] The coefficient on *Gain* of 0.372 (t-stat 33.18) indicates that investors are exp(0.372)≈1.45 times more likely to realize a gain versus a loss when not impacted by a disaster. The coefficient on *Gain*Damage* of 0.064 (t-stat 2.19) indicates that investors are exp(0.372+0.064)≈1.55 times more likely to realize a gain versus a loss when impacted by $10,000 of damage. This pattern is consistent with previous results.

As a final robustness check, I drop the sale condition and analyze the sale propensities across account-day-stock positions including those in which no activity occurred. As discussed in Section 3.3, this procedure adds a significant amount of noise to the sample as investors do not pay attention to every stock they own on every day they hold it. Nonetheless, column 6 reports these unconditional results. Notice the number of observations grows immensely to over 70 million. The interaction

[29] Unfortunately, I cannot include three-way stratification tests due to computational limitations.

coefficient of 0.016% (t-stat 1.85) indicates that investors impacted by $10,000 of per capita damage display a 7% (.016%/.233%) larger disposition effect. Although statistical and economic significance decrease in this column, it is likely due to the additional noise from new, inattentive observations. Recall also that the per capita damage variable is also measured with noise at the county level, so this test likely acts as a lower bound on the true effect. Still, the main results hold across all alternate specifications.

7. Conclusion

I find evidence that the well-documented disposition effect increases after investors are impacted by a natural disaster. The increased disposition effect for damaging events holds even when controlling for variations in the probability to sell across accounts, stocks, and dates, and the effect persists across different disaster types and seasons. Furthermore, the disposition effect increases with disaster severity and decreases with the length of time following the event. Overall, these results suggest that external individual-specific events may strongly affect investor behavior, especially in the short-term.

I consider mechanisms related to liquidity constraints, tax incentives, local stocks, informed trading, and residence utility. While the increased disposition effect of disaster-affected investors 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.

This paper documents how investors may 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 since the marginal utility from realizing a gain (loss) increases (decreases). 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.

Table 1.1: 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 1.2: Individual Investor Summary Statistics by Disaster Exposure

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. *Disaster* observations are those with any level of exposure to a natural disaster, and *No Disaster* observations are those with no exposure. 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
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		
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
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		
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
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		
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
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		
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 1.3: Natural Disaster Damage Per Capita Regressions

This table reports the regressions of equation (2) with additional fixed effects controls using natural disaster damage per capita at the county-level to proxy for individual disaster exposure. *Damage* is equal to the per capita dollar damage to the county in which the account resides and 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 1.4: 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)
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

Table 1.5: Natural Disaster Seasons and Types

This table reports the specification from column 4 of Panel B in Table 1.3 by disaster seasons (Panel A) and disaster types (Panel B). 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 (scaled by $10,000) to the county in which the account resides. In Panel A, *Winter* include all observations in the months of January and February, *Spring* includes March through May, *Summer* includes June through August, *Fall* includes September through November, and *December* includes only the month of December. In Panel B, columns 1-3 include only the disaster observations identified by the type listed to determine the impact across different disaster types. *Other* includes earthquakes, droughts, wildfire, and other severe weather. Columns 4 and 5 report the full specification with additional event fixed effects and event*gain fixed effects, respectively. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

PANEL A: Seasons

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Sale	Winter	Spring	Summer	Fall	December
Gain	0.106***	0.111***	0.100***	0.087***	0.004
	(16.01)	(18.98)	(16.40)	(15.48)	(0.38)
Damage	-0.000	-0.020**	0.001	-0.009	-0.031
	(-0.01)	(-2.08)	(0.11)	(-1.04)	(-0.73)
Gain * Damage	0.019**	0.022***	0.025***	0.032***	-0.073
	(2.39)	(3.03)	(3.84)	(3.59)	(-1.42)
Observations	132,621	199,279	194,073	220,659	62,398
R-squared	0.258	0.255	0.256	0.247	0.249
Date FE	Yes	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes

PANEL B: Event Types

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Sale	Hurricanes / Tropical Storms	Blizzards / Winter Storms	Other	Event FE	Event*Gain FE
Gain	0.100***	0.099***	0.099***	0.091***	0.091***
	(18.79)	(19.05)	(18.70)	(19.65)	(17.65)
Damage	-0.004	-0.008	-0.005	-0.010	-0.012*
	(-0.55)	(-1.23)	(-0.27)	(-1.63)	(-1.93)
Gain * Damage	0.016***	0.031***	0.010	0.018***	0.022***
	(3.05)	(4.48)	(0.33)	(3.51)	(5.22)
Observations	603,088	644,814	621,747	820,820	820,820
R-squared	0.250	0.247	0.248	0.242	0.242
Date FE	Yes	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes

Table 1.6: Liquidity Constraint Controls

This table provides additional control variables for various liquidity constraints explanations. 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. *Asset Value* represents the dollar value of the asset to the given investor on the day prior, scaled by $100,000. *Portfolio Size* represents the dollar value of the investor's stock portfolio on the day prior, scaled by $1,000,000. *Asset Proportion* represents the value-weighted proportion of the asset in the investor's stock portfolio on the day prior. All 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	0.090***	0.091***	0.066***	0.067***
	(19.46)	(19.57)	(15.55)	(15.84)
Damage	-0.007	-0.007	-0.004	-0.004
	(-1.32)	(-1.33)	(-0.88)	(-0.87)
Gain * Damage	0.019***	0.019***	0.017***	0.017***
	(3.66)	(3.56)	(3.57)	(3.53)
Asset Value	0.018**			-0.030***
	(2.43)			(-6.59)
Portfolio Size		-0.029**		-0.003
		(-2.18)		(-0.95)
Asset Proportion			0.602***	0.621***
			(7.26)	(8.05)
Observations	820,820	820,820	820,820	820,820
R-squared	0.242	0.242	0.286	0.287
Date FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes

Table 1.7: Within-Month Seasonality, Income, and Tax Incentives

This table reports subsample analysis based on within-month seasonality, investor income, and account tax laws. 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, I report subsample results of column 4 of Panel B in Table 1.3 based on the day of the month for the observation. Similarly, columns 4 and 5 report subsets based on the investor's reported income splitting the investors near the median at $75,000 USD. Finally, columns 6 and 7 report subsets based on the tax laws regarding the type of account used. Taxable refers to accounts that are subject to yearly income taxes, while Tax-exempt restricts to those that are tax-exempt, such as various retirement accounts. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Day of Month			Income		Taxes	
Dependent Variable: Sale	Day 1-10	Day 11-20	Day 21-31	High	Low	Tax-exempt	Taxable
Gain	0.100***	0.091***	0.086***	0.097***	0.090***	0.110***	0.088***
	(17.99)	(16.99)	(15.95)	(16.92)	(14.16)	(13.92)	(17.51)
Damage	-0.004	-0.018***	-0.010	-0.015	-0.003	-0.027***	-0.004
	(-0.58)	(-2.72)	(-1.06)	(-1.53)	(-0.75)	(-3.10)	(-0.69)
Gain * Damage	0.023***	0.024***	0.017**	0.019**	0.022***	0.031**	0.017***
	(3.50)	(2.65)	(2.18)	(2.08)	(3.67)	(2.40)	(2.60)
Observations	253,931	279,776	279,198	360,912	332,526	148,727	671,839
R-squared	0.247	0.244	0.239	0.246	0.253	0.289	0.231
Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.8: 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 1.3 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

Table 1.9: 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)	(2)	(3)	(4)	(5)
	All Obs	Extreme	Severe	Moderate	None
Average Excess Returns					
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)
Average DGTW Returns					
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 1.10: 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).

	(1)	(2)	(3)
Dependent Variable: Sale	Short	Long	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

Table 1.11: Short-Term Reinvestment Probabilities

This table tests the short-term reinvestment probabilities based on natural disaster exposure. Panel A uses all accounts in the sample, and Panel B restricts to those that are subject to yearly income taxes. 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).

PANEL A: All Accounts

	(1)	(2)	(3)	(4)
Dependent Variable: Reinvest	5 Days	10 Days	15 Days	20 Days
Loss_Disaster	0.025***	0.024***	0.020***	0.017***
	(4.68)	(4.41)	(3.76)	(3.15)
Loss_None	0.021***	0.017***	0.017***	0.017***
	(3.78)	(3.17)	(3.22)	(3.16)
Gain_None	0.002	0.006	0.007	0.009*
	(0.36)	(1.19)	(1.35)	(1.91)
Observations	145,844	145,844	145,844	145,844
R-squared	0.362	0.393	0.414	0.429
Date FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes

PANEL B: Taxable Accounts

	(1)	(2)	(3)	(4)
Dependent Variable: Reinvest	5 Days	10 Days	15 Days	20 Days
Loss_Disaster	0.017***	0.020***	0.017***	0.014**
	(2.83)	(3.34)	(2.89)	(2.26)
Loss_None	0.013**	0.013**	0.016**	0.015**
	(2.07)	(2.17)	(2.58)	(2.53)
Gain_None	0.002	0.010*	0.012**	0.015***
	(0.32)	(1.81)	(2.17)	(2.84)
Observations	111,836	111,836	111,836	111,836
R-squared	0.362	0.396	0.419	0.436
Date FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes

Table 1.12: Robustness

This table reports the specification from column 4 of Panel B in Table 1.3 (column 1) with various robustness procedures. 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 (scaled by $10,000) to the county in which the account resides. Column 2 uses only account-dates in which the investor had at least one gain and one loss. Column 3 drops those accounts that trades at least 48 times per year on average. Column 4 includes returns size fixed effects to control for the v-shaped selling behavior documented in Ben-David and Hirshleifer (2012). More specifically, I group all observations by holding period return into 50 brackets: (-∞, -50%), …, [-4%, -2%), [-2%, 0), [0, 2%), [2%, 4%), …, [50%, -∞). Column 5 uses a Cox proportional hazard model from equation (4) in place of the linear probability model. Column 6 removes the sale condition and thus includes all account-day-stock holding positions. For columns 1-4 and column 6, all standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Sale	(1) Base	(2) Holding Gain and Loss	(3) Drop Most Active	(4) Return Size FEs	(5) Hazard	(6) Unconditional
Gain	0.091***	0.090***	0.018***	0.129***	0.372***	0.00233***
	(19.61)	(17.85)	(3.66)	(24.01)	(33.18)	(22.49)
Damage	-0.007	-0.009**	-0.007	-0.009	-0.065*	-0.00003
	(-1.31)	(-1.98)	(-1.28)	(-1.46)	(-1.73)	(-0.48)
Gain * Damage	0.019***	0.025***	0.018***	0.021***	0.064**	0.00016*
	(3.64)	(4.22)	(3.44)	(5.17)	(2.19)	(1.85)
Observations	820,820	699,320	258,536	820,820	644,489	70,407,249
R-squared	0.242	0.135	0.269	0.243	0.006	0.010
Date FE	Yes	Yes	Yes	Yes	No	Yes
Account FE	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	No	Yes

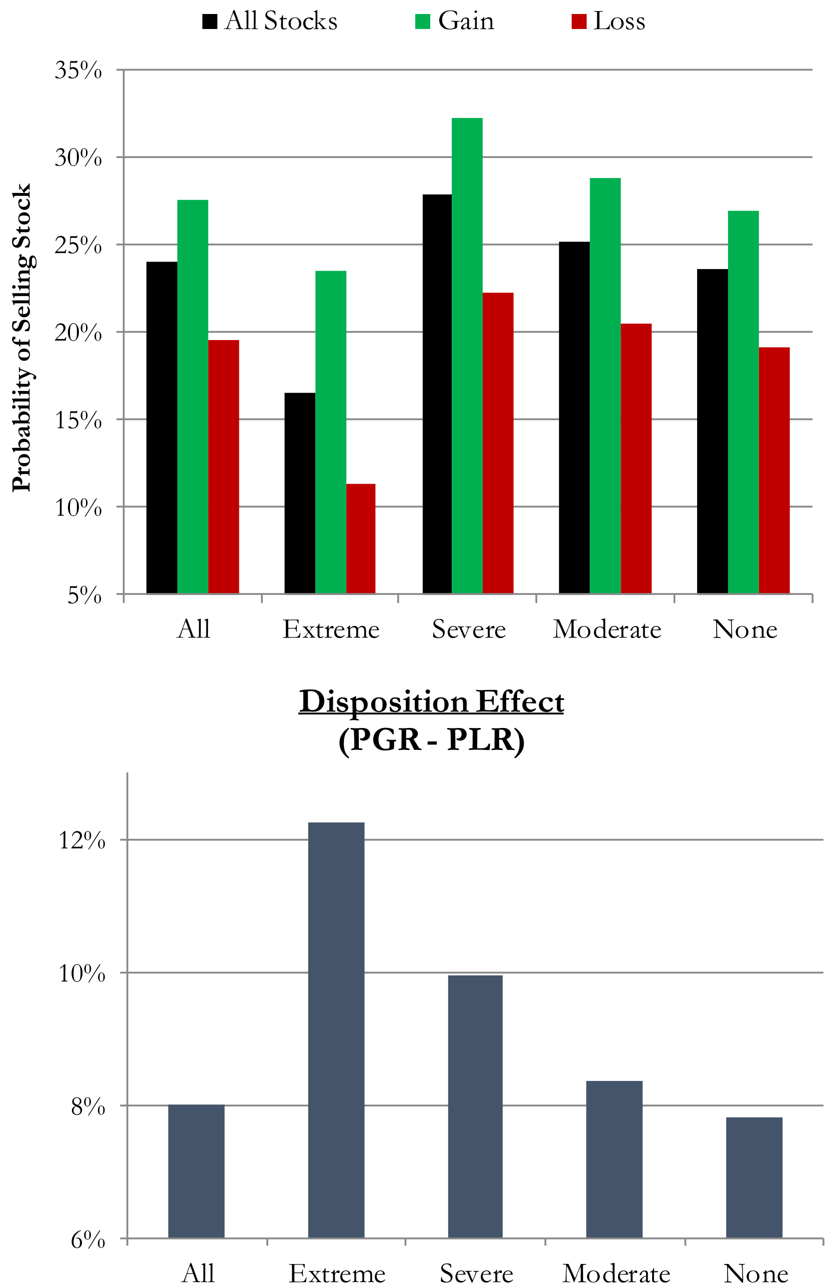


Figure 1.1: The Disposition Effect by Natural Disaster Severity Cohorts

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 90^{th} 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.

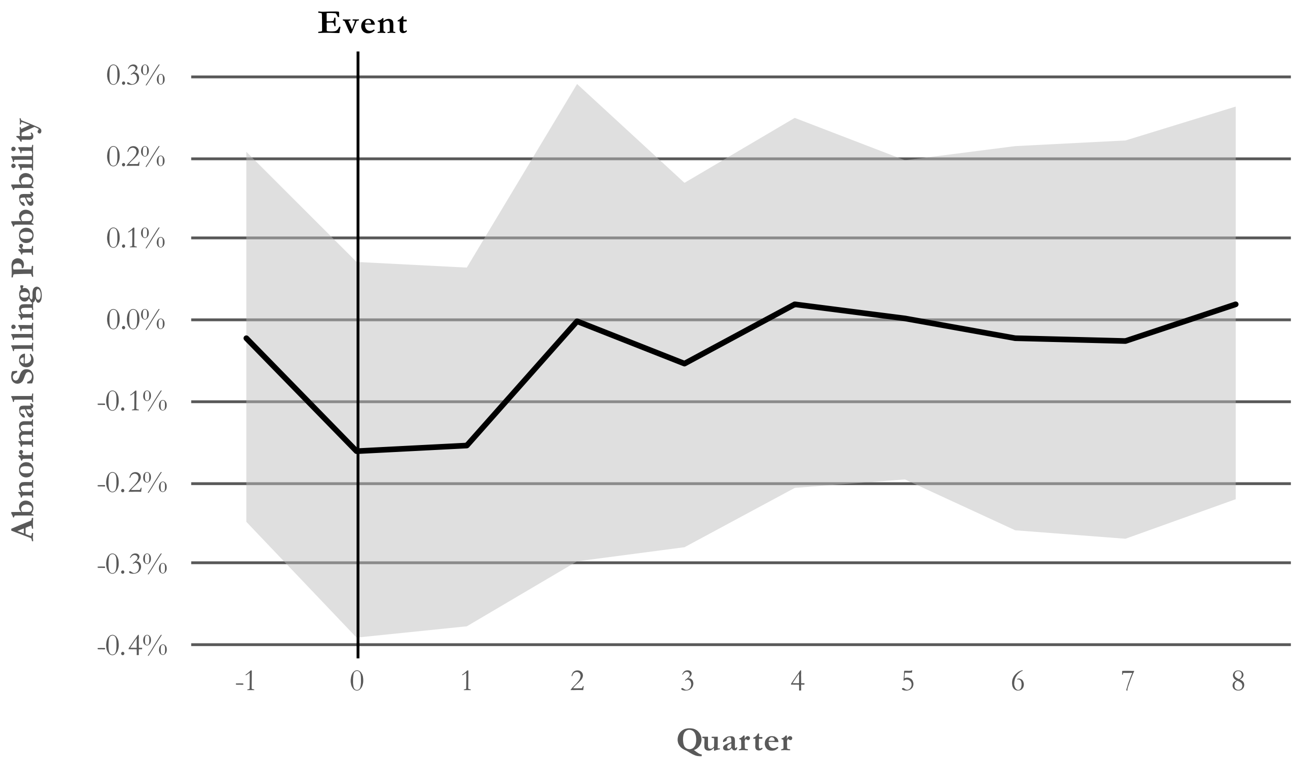


Figure 1.2: Unconditional Abnormal Selling around Natural Disasters

This figure displays the unconditional abnormal selling probabilities at the county-quarter level. The black line represents the β coefficients from the estimate of equation (3) with 95% confidence intervals shaded in grey. Natural disaster events are those that inflict at least \$1,118 per capita in a given county (also described as the severe and extreme cohorts in Section 4.2).

ESSAY 2 – THE PORTFOLIO-DRIVEN DISPOSITION EFFECT

1. Introduction

There is perhaps no more robust trading phenomenon than the disposition effect, the observation that investors are more likely to sell an asset when it is at a gain than when it is at a loss (Shefrin and Statman, 1985). The disposition effect has been documented among US retail stock investors (Odean, 1998), foreign retail investors (Grinblatt and Keloharju, 2001), institutional investors (Shapira and Venezia, 2001), homeowners (Genesove and Mayer, 2001), corporate executives (Heath, Huddart, and Lang, 1999), and in experimental settings (Frydman, Hartzmark, and Solomon, 2018).

Standard explanations for the disposition effect – such as tax considerations, portfolio rebalancing, and informed trading – have been proposed and dismissed (Odean, 1998), leaving explanations that rely on investor preferences such as prospect theory (Kahneman and Tversky, 1979). For example, Barberis and Xiong (2009) show that the disposition effect is most reliably generated in a model of prospect theory preferences over realized gains and losses.

While much of the empirical and theoretical work related to the disposition effect focuses on individual assets, most households hold a portfolio of assets. This paper then asks a simple question: does the disposition effect operate at the individual asset level or at the portfolio level? In doing so I ask the related question of whether investors have preferences over their individual stocks or over the portfolio as a whole.

To illustrate the idea, consider an investor with three stocks: X_1, X_2, and X_3. The disposition effect says Pr(X_i is sold | X_i is at a gain) > Pr(X_i is sold | X_i is at a loss) for all *i*. If the investor has preferences over each individual stock, then those three probabilistic statements should be independent of each other. However, if she has preferences over the portfolio, then the disposition effect for Stock X_1 may depend on the state of the remaining portfolio (X_2 and X_3).

The latter is precisely what I find in the data. In fact, when I examine the trading among the 78,000 households in the Barber and Odean (2000) dataset, I find no material disposition effect for Stock X if the remaining portfolio is up. In this case, Stock X is nearly as likely to be liquidated at a paper gain as a paper loss. However, if the remaining portfolio is down, Stock X is more than twice as likely to be liquidated at a paper gain as a paper loss. Given how pervasive the disposition effect is, it is surprising to find that the disposition effect is economically meaningless among the 64% of observations in which portfolios are up in the Barber and Odean (2000) dataset.

I document this relationship between the performance of an investor's portfolio and her tendency to exhibit a disposition effect in both univariates and regressions with a host of fixed effects. I then consider some possible explanations.

One possibility is that I am simply capturing a manifestation of Hartzmark's (2015) attention-based finding that investors tend to sell their extreme positions, i.e., their best and worst performing stocks. I address this possibility by restricting the sample to only non-extreme stocks in an investor's portfolio and show that the results are strong in this sample too.

I also consider the possibility that portfolio rebalancing drives the result; e.g., perhaps when one of an investor's stocks is up and the rest of her stocks are down, she partially liquidates her winning position because it now comprises a large share of her portfolio, and she uses the proceeds to invest in other stocks to rebalance her portfolio. Additional analysis casts doubt on this explanation: the portfolio-driven disposition effect is actually stronger when restricting attention to complete (rather than partial) liquidations, and investors are *less* likely to reinvest their proceeds when they liquidate a winner and the rest of their portfolio is at a loss.

Another possibility is that portfolio gains proxy for skilled or sophisticated investors. However, when considering investor proxies for sophistication – such as professional jobs or high income – I find the same results. I also find similar results when considering portfolio gains generated entirely from stock-picking alpha, as identified from a DGTW characteristics-based model, or not. Because it does not seem to matter whether the portfolio gain was achieved via stock-picking alpha or not, this suggests the portfolio-driven disposition effect is not being driven by portfolio gain as a statistical stand-in for investor skill.

Yet another possibility is that I am simply capturing framing tricks that investors use to maximize their realization utility. For example, perhaps investors are more willing to recognize a loss when their portfolio is at a gain because they are able to match the losing stock with a winning stock whose gain exceeds the losing stock's loss. By realizing both transactions simultaneously, the investor can mentally account for this dual transaction as a single realized gain. To ensure that this is not driving the results, I restrict the sample to days on which an investor sells just one stock. I find that the result is strong in this subsample too.

The explanation that seems most consistent with the evidence is that investors derive utility from both paper gains and realized gains and that they take utility by realizing gains when they have disutility from unrealized losses.

According to standard expected utility theory, investors only derive utility from consumption, and a stock's return only affects an investor's utility through its effect on the investor's consumption. Because expected utility theory has difficulty explaining people's behavior in many settings, prospect theory was developed to argue that people derive utility over gains and losses rather than over absolute wealth levels (Kahneman and Tversky, 1979; Kahneman and Tversky, 1992). Economists who have incorporated prospect theory preferences into their models have assumed that investors derive utility from *paper* gains (Barberis and Huang, 2001; Barberis, Huang, and Santos, 2001; Barberis and Xiong, 2009) as well as *realized* gains (Barberis and Xiong, 2009; Barberis and Xiong, 2012; Henderson, 2012; Ingersoll and Jin, 2013).

The evidence is consistent with investors deriving utility from *both* paper gains and realized gains. Regarding paper gains, when an investor's portfolio is performing well, the investor might derive utility from anticipating the increased future consumption she will have. Alternatively, the performance of an investor's portfolio might affect her utility in ways that are unrelated to her future consumption. For example, if an investor's portfolio is doing poorly, she might experience regret over her decision to participate in the market, or she might have lower self-esteem because of her poor choice of stocks (Barberis, Huang, and Santos, 2001). Regarding realized gains, investors receive a "burst" of utility at the moment that gains are realized (Barberis and Xiong, 2012; Henderson, 2012; Ingersoll and Jin, 2013; Frydman et. al., 2014).

If investors derive utility from both sources, an investor's desire for a "burst" of realization utility should be inversely related to the level of utility the investor is deriving from her paper gains/losses. More specifically, when an investor's overall portfolio is down, the investor will receive a lot of negative utility from the paper losses, so she should be especially likely to seek a burst of positive utility from realizing a paper gain to offset some of the negative utility she has received due to the poor performance of her portfolio. This could explain why I find such a strong disposition effect when an investors' portfolio is down.

Following this intuition, I find that this condition – when the stock is at a gain and the portfolio is at a loss – is the one in which investors are most likely to keep their stock sale in cash. That is, in the case when their portfolio is down and they realize a gain, it is important to investors that the gain *stay* realized rather than creating a new mental account as in Frydman, Hartzmark and Solomon (2018).

Conversely, when her portfolio is performing well, she receives positive utility from the paper gains, so she should feel less need for a burst of utility from realizing a gain. This could explain why the disposition effect largely attenuates when an investor's portfolio sits at a gain.

The paper is organized as follows. I discuss the data and methodology in Section 2. Section 3 analyzes the disposition effect and portfolio performance impacts, and Section 4 discusses alternate explanations. Section 5 concludes.

2. Data and Methodology

I begin with the large discount broker dataset utilized by Barber and Odean (2000). The raw data include trading activity for 78,000 households with 158,000 accounts between January 1991 and November 1996.

The unit of observation is an account-stock-day triple. Given that approximately 104 thousand accounts that hold common stock, with an average of 3.5 stocks per account over the 1,497 trading days in the sample, I begin with approximately 545 million observations. Following Ben-David and Hirshleifer (2012), I filter the raw dataset and make several simplifying assumptions. First, I include only securities that are identified as common shares and appear in CRSP. Because prices in the discount brokerage dataset are not adjusted for splits and dividends, I rely on CRSP factor adjustments to account for these issues. Second, I remove any account-stocks with negative commissions since they may indicate a reverse transaction. Third, if a stock has at least one day with no active trading in the preceding 250 trading days, I remove it. Fourth, investor-stocks with any negative positions (either from short sales or from belonging to a position opened before the start of the sample period) are assumed to be liquidated at the time of turning negative to avoid any misrepresentation in the value-weighted average price (VWAP) of portfolio holdings. Finally, since the primary area of interest is portfolio behavior, I keep only account-days with at least two common stock holdings. After applying these filters and rules, the dataset has 102,275,146 (account, stock, day) observations.

I also analyze a special subset of the dataset described above: only those daily observations where an account has a sale. I refer to this subsample as the "sale conditioned dataset." This filter is used in much of the disposition effect literature (Odean, 1998; Chang, Solomon, and Westerfield, 2016). Given how seldom an account makes a sale, this filter reduces the dataset to 1,371,064 observations. Table 2.1 displays summary statistics of these samples.

The traditional regression specification for measuring the disposition effect (Birru, 2015; Chang, Solomon, and Westerfield, 2016) uses the following equation:

$$Sale_{i,j,t} = \beta_0 + \beta_1 Gain_{i,j,t} + \epsilon_{i,j,t} \tag{5}$$

where observations occur at the account (i), stock (j), and date (t) level. For every account-stock-day, *Sale* is a dummy variable equal to one if a sale occurs (including partial sales) and zero otherwise. Additionally, *Gain* is a dummy variable equal to one if the stock's return (price / VWAP − 1) is strictly positive and zero otherwise. With this structure, the mean of the dependent variable, *Sale*, is the probability of selling a given position. Thus, β_0 (the constant) measures the probability of selling a stock whose return is less than or equal to zero, and β_1 measures the increase in probability of selling a given stock if that stock's return is strictly greater than zero. Recently, Chang, Solomon, and Westerfield (2016) as well as many others show that β_1 is positive and statistically significant.

I analyze the relationship between the disposition effect and the performance of the investor's portfolio by estimating the following regression equation:

$$Sale_{i,j,t} = \beta_0 + \beta_1 Gain_{i,j,t} + \beta_2 Portfolio\_Gain_{i,j,t} + \beta_3 Gain_{i,j,t} \times Portfolio\_Gain_{i,j,t} + \epsilon_{i,j,t} \tag{6}$$

where observations also occur at the account (i), stock (j), and date (t) level. The additional variable, *Portfolio_Gain*, is a dummy indicating whether or not the investor's remaining portfolio is at a gain or a loss. I compute this variable by first summing up the gains/losses (in dollars) of the investor's positions in her other stocks (excluding the stock under consideration) as of the given day. If the investor has a net gain in these other stocks, *Portfolio_Gain* takes the value of 1; otherwise, it is 0.

The main coefficient of interest in (6) is β_3 (the coefficient of the interaction term), which represents the difference in disposition effects for paper gain portfolios and paper loss portfolios. In equation (6), β_1 represents the disposition effect for paper loss portfolios, and the sum of β_1 and β_3 represents the disposition effect for paper gain portfolios.

3. The Portfolio-Driven Disposition Effect

The phenomenon that I document in this essay, referred to as "the portfolio-driven disposition effect," can be illustrated with a simple figure. Consider the probability that an investor sells one of her holdings. This is plotted in the portion of Figure 2.1 labeled "All Portfolios" for both the unconditional dataset (which does not condition on the investor making a sale on the given date) and the sale conditioned dataset.

The disposition effect can be seen visually as the difference between the green (the probability of selling a gain) and the red (the probability of selling a loss) bars. The black bars (which represent all stocks) are included to show the weighted average. The probability of selling a given stock is approximately 0.26% for the unconditional sample and 20% for the sale conditioned sample. Adding the condition that a given stock's return is positive (the green bar) increases that probability of an investor selling to 0.29% for the unconditional sample and 22% for the sale conditioned sample. The difference in the probability of selling a gain versus a loss is approximately 7 bps for the unconditional sample and 6% for the sale conditioned sample. In other words, an investor is approximately 32%

(0.29%/0.22% − 1) more likely to sell a gain than a loss using the unconditional sample and approximately 37% (22%/16% − 1) more likely using the sale conditioned sample. This is the disposition effect.

To illustrate the portfolio-driven disposition effect, I reproduce these probabilities for two different scenarios: (1) the rest of the investor's portfolio is at a gain (the portion labeled ">0"), and (2) the rest of her portfolio is at a loss (the portion labeled "≤ 0"). The portfolio-driven disposition effect refers to the fact that the disposition effect is concentrated in the scenario where the rest of her portfolio is at a loss; when the rest of her portfolio is at a gain, the disposition effect almost entirely disappears. In fact, the disposition effect decreases to approximately 1 bps using the unconditional sample and 2% using the sale conditioned sample. Thus, when the portfolio is at a paper gain, an investor is only 6% more likely to sell a gain than a loss using the unconditional sample and only 13% more likely using the sale conditioned sample. Conversely, the disposition effect more than doubles when restricting to observations in which the rest of the portfolio is at a paper loss to produce a disposition effect of approximately 20 bps using the unconditional sample and 16% using the sale conditioned sample. This means that when an investor's portfolio is at a paper loss, she is 93% more likely to sell a gain than a loss using the unconditional sample and 96% more likely using the sale conditioned sample.

Moreover, the probability of selling *gains* seems to drive the change in the disposition effect based on portfolio performance. While the probability of selling losses changes slightly when conditioning on the rest of the portfolio's performance, the probability of selling gains increases considerably (40% in the unconditional sample and 48% in the sale conditioned sample).

In the rest of the paper, I document that the portfolio-driven disposition effect is a robust phenomenon, examine whether it can be explained by prior studies of the disposition effect, and consider several possible explanations for the phenomenon.

Regarding robustness, I first consider the unconditional dataset, which does not restrict the sample based on whether or not the investor sold any shares of any stock on the given date. I estimate equation (6) on this sample and report the results in Table 2.2. Column 1 of Table 2.2 shows the baseline results with no fixed effects. Columns 2-4 add fixed effects controls for date, account, and stock, respectively. Finally, column 5 displays the most controlled specification with account, date, and stock fixed effects. Because investor sale decisions are likely correlated within account, within stock, and within date, I cluster standard errors across all three of these dimensions following the procedure of Cameron, Gelbach, and Miller (2011).

Across all regressions in Table 2.2, the coefficient on the interaction term, *Gain*Portfolio_Gain,* ranges from −0.18% to −0.25% and is statistically significant well below the 1% level (t-stats between −20 and −21). These results suggest that the portfolio-driven disposition illustrated in Figure 2.1 is unlikely to be explained by unobservable investor, time, or stock characteristics that affect investors' propensity to sell shares of stock. Furthermore, the disposition effect is economically insignificant and at times statistically insignificant when the rest of the portfolio is at a paper gain. Recall the disposition effect when the rest of the portfolio is at a paper gain is measured by the sum of the coefficient from *Gain* and the coefficient on the interaction term (*Gain*Portfolio_Gain*). This sum for the base test (column 1) is 0.015%, and a linear restriction test which tests whether the sum of coefficients is zero fails to reject (p-value 0.08). The sum in column 2, which includes date fixed effects is 0.009% and the corresponding linear restriction test also fails to reject (p-value 0.29). Although the sum is statistically significant at or below the 5% level for columns 3-5, the economic significances are minimal with the largest effect in column 5, which has a sum of 0.089%. Even in this specification, the disposition effect is more than three times larger when the rest of an investor's portfolio is at a loss (0.336%) than when it is at a gain.

Many researchers who study the disposition effect restrict attention to days in which the investor sells shares of any stock in her portfolio. In Table 2.3, I restrict attention to such observations, run the same regressions as in Table 2.2 on this subsample. While the magnitudes of the coefficients are larger due to the sale condition, the interaction coefficients remain negative (between −12% and −14%) and significant (t-stats between −24 and −28). In addition, the disposition effect for gain portfolios (measured as the sum of gain and the interaction coefficient) ranges from 2% - 4%. Although statistically significant, the economic significance of the disposition effect for gain portfolios is immaterial compared to loss portfolios. In fact, even in the most controlled regression (column 5), the disposition effect is still more than four times larger when the rest of an investor's portfolio is at a loss (15.5%) than when it is at a gain (3.7%).

4. Possible Explanations

4.1. Attention Effects

I first test whether extreme stocks drive the portfolio-driven disposition effect. Hartzmark (2015) finds that individual and mutual fund investors are more likely to sell their best and worst performing stock on a given sale day. Intuitively, these extreme stocks grab the investor's attention and, as a result, are sold more often. In this setting, the attention-grabbing hypothesis could predict some results, but not others. For example, if an investor has one stock that is a winner and the rest losers, then this stock is very likely to be sold under both the attention-grabbing hypothesis (it is an extreme stock) and the portfolio-driven disposition effect (investors are very likely to sell their winners when the rest of the portfolio is at a loss). However, if an investor has one stock that is a loser and the rest winners, this stock is very likely to be sold under the attention-grabbing hypothesis because it is an extreme stock, but not the portfolio-driven disposition effect because losers are just as likely to be sold as winners are when the remaining portfolio is at a gain. Nevertheless, in Table 2.4 I test the impact of extreme stocks on the portfolio-driven disposition effect by removing the best and worst stocks for every account-day and running the same regressions as in Table 2.2.

Column 1 of Table 2.4 reports the base test (column 5 from Table 2.2). Column 2 restricts to only extreme observations, and column 3 removes extreme stock observations. It is worth noting that when an investor owns only two stocks, both are considered extreme. Because of this fact, column 3 inherently includes account-days in which at least three stocks are held. Still, the interaction coefficient on extreme observations is −0.27% (t-stat −21.93) and non-extremes is −0.22% (t-stat −13.05). Although the interaction coefficient is slightly smaller for non-extreme observations, it is still statistically significant well below the 1% level and actually offsets a larger proportion of the Gain coefficient. In fact, the disposition effect of an extreme stock declines by 69% (-0.27%/0.39%) when the remaining holdings are at a gain versus at a loss, while the disposition effect of a non-extreme stock declines by 76% (-0.22%/0.29%) when the remaining holdings are at a gain versus at a loss. Similarly, the interaction coefficient remains statistically significant for these sub-samples in the sale conditioned sample. Moreover, the disposition effect of an extreme (non-extreme) stock declines by 49% (71%) when the remaining holdings are at a gain versus at a loss. These results suggest that the rank effect (Hartzmark, 2015) does not explain the portfolio-driven disposition effect.

4.2. Portfolio Rebalancing

Although Odean (1998) provides evidence that portfolio rebalancing does not explain the disposition effect, it is possible that portfolio rebalancing causes the portfolio-driven disposition effect. For example, suppose all but one of an investor's stocks are at a loss. It is likely that the lone stock that is trading at a gain comprises a disproportionately large percentage of the investor's portfolio due to its gains and the rest of the stocks' losses. The investor might therefore want to liquidate some of her holdings in the stock that is at a gain in order to rebalance her portfolio.

According to this explanation, investors should *partially* (not completely) liquidate their positions in the stock that is at a gain when the rest of the portfolio is at a loss. That is, the portfolio-driven disposition effect should disappear when restricting attention to *complete* liquidations of stocks.

To test this, I define the dummy variable *Full_Sale* to equal one if the investor completely liquidates her position in a stock and zero otherwise. The probabilities of complete liquidations are graphed in Figure 2.2 for both samples.

Far from disappearing, the portfolio-driven disposition effect is even *stronger* when restricting attention to complete liquidations. Using only complete liquidations as sales, the disposition effect decreases to 0 bps when the investor's remaining holdings are at a gain using the unconditional sample and 1% using the sale conditioned sample. Therefore, when the remaining holdings are at a paper gain, the investor is no more likely to completely liquidate a gain than a loss using the unconditional sample and only 8% more likely using the sale conditioned sample. On the other hand, the disposition effect is entirely concentrated in those observations in which the rest of the portfolio is at a paper loss to produce a disposition effect of approximately 17 bps using the unconditional sample and 14% using the sale conditioned sample. This means that when an investor's portfolio is at a paper loss, she is 102% more likely to completely liquidate a gain than a loss using the unconditional sample and 104% more likely using the sale conditioned sample.

Next, I turn to multivariate analysis to see whether portfolio rebalancing explains the portfolio-driven disposition effect. Similar to Tables 2.2 and 2.3, I consider five specifications in Table 2.5 with variations of fixed effects for the unconditional sample (Panel A) and the sale conditioned sample (Panel B); the only difference is that I now use *Full_Sale* as the dependent variable. Across all specifications, the interaction coefficient is negative and statistically significant. Even in column 5 of both panels, the interaction coefficient is 77% (unconditional) and 80% (sale conditioned) of the gain coefficient in absolute value. This means that most of the disposition effect is eliminated when the remaining portfolio is at a gain when controlling for unobservable investor, time, or stock characteristics that affect investors' propensity to sell shares of stock.

The univariate and multivariate analysis suggests that the portfolio-driven disposition effect is actually stronger when restricting attention to complete liquidations. Thus, I conclude that portfolio rebalancing is an unlikely explanation for the portfolio-driven disposition effect.

4.3. Mental Accounting Tricks with Simultaneous Liquidations

Another possible explanation for the portfolio-driven disposition effect is that investors might simultaneously realize losses and gains in order to cushion the blow of realizing the loss. For example, suppose an investor liquidates a losing position at the same time that she liquidates a winning position. If the gains of the winning position exceed the loss from the losing position, she can mentally account for these two transactions as a single realized gain. When an investor's portfolio is performing well, she can more easily find a winning position that dominates any of her losing positions, and hence, she might be more likely to realize one of her losses when her portfolio is performing well.

To address this possibility, I first examine whether there is any evidence that investors actually seek to simultaneously realize gains and losses. To do this, I need a model to predict how often investors simultaneously liquidating two losses, two gains, and one gain and one loss (conditional on them liquidating two positions). Consider an investor who has N stocks in her portfolio, and N_G are at a gain while $N_L = N - N_G$ are at a loss. Suppose that she liquidates exactly two stocks on a given day. If she randomly picks two stocks to liquidate, it is straightforward to verify that

$$\Pr(\textit{sell two gains}) = \frac{\binom{N_G}{2}}{\binom{N}{2}} = \frac{N_G(N_G-1)}{N(N-1)} \tag{7}$$

$$\Pr(\textit{sell one gain and one loss}) = \frac{N_G N_L}{\binom{N}{2}} = \frac{2N_G N_L}{N(N-1)} \tag{8}$$

$$\Pr(\textit{sell two losses}) \;=\; \frac{\binom{N_L}{2}}{\binom{N}{2}} \;=\; \frac{N_L(N_L-1)}{N(N-1)} \tag{9}$$

where $\binom{x}{y}$ ("x choose y") is the binomial coefficient representing the number of subsets of size y that exist given a set of size x. Of course, a priori this model is not entirely valid, because the disposition effect implies that investors should be more likely to liquidate two gains than two losses. Hence, the disposition effect implies that this model should overestimate the likelihood that investors simultaneously liquidate two losses.

Having established a baseline prediction for the probability of an investor liquidating two gains, two losses, or one gain and one loss, I can test whether investors are disproportionately likely to simultaneously recognize a gain and loss by comparing the empirical frequencies with the model's predictions. For each instance when an investor liquidates exactly two stocks on a given day, I calculate the model's residual for each of the three possibilities according to (7)-(9). For example, suppose the model predicts (conditional on an investor liquidating two positions) that it is 50% likely that the investor will liquidate two gains, 40% likely she will liquidate one gain and one loss, and 10% likely that she will liquidate two losses. If in reality the investor liquidates two gains, then the model's residual for the "liquidate two gains" scenario is 0.5, while the model's residuals for the "liquidate one gain and one loss" and "liquidate two losses" are -0.4 and -0.1, respectively. I report average residuals and their t-stats clustered by account using Rogers (1993) standard errors in Table 2.6. The average residual of the "sell one gain and one loss" is -0.129, which is highly significant (t-stat -32.89). This suggests that investors are not disproportionately likely to simultaneously liquidate gains and losses in order to soften the blow of realizing losses.

Even though the evidence in Table 2.6 suggests that investors are not disproportionately likely to simultaneously realize gains and losses, it is possible that other forms of mental accounting tricks using simultaneous liquidations can explain the portfolio-driven disposition effect. To address this, I run the baseline regression on the sample of investor-dates where the investor only sells shares of one stock. I find that the portfolio-driven disposition effect (i.e., negative and significant coefficient of the interaction *Gain*Portfolio_Gain*) is similar in magnitude in this subsample as it is in the entire sample, suggesting that the portfolio-driven disposition effect is not driven by mental accounting tricks relying on simultaneous liquidations. These regressions are reported in Table 2.7.

4.4. Unobserved Skill

Grinblatt, Keloharju, and Linnainmaa (2012) analyze data on Finnish investors and document that high IQ investors are superior stock pickers and they exhibit less of a disposition effect. Hence, it is possible that high IQ investors (who do not exhibit a disposition effect and are superior traders) likely have portfolios at a gain, and low IQ investors (who are prone to the disposition effect and are inferior traders) likely have portfolios at a loss. In other words, it is possible that I am simply documenting a consequence of Grinblatt, Keloharju, and Linnainmaa's (2012) finding. I address this possibility in two ways. First, I use proxies for investor sophistication that have been used by prior researchers to see if the results differ across investor sophistication. Second, I identify situations in which an investor's portfolio gain is more likely to be driven by skill than luck, and I compare the disposition effect in these two scenarios. If investor IQ drives the results, then there should be little disposition effect in scenarios where the portfolio is performing well due to stock-picking skill but a much stronger disposition effect when the portfolio is performing well due to luck.

The trading data have several demographic characteristics available for a sub-sample of investors. I follow Dhar and Zhu (2006) in using employment and income as proxies for investor sophistication. Like them, I classify employment as either professional ("professional/technical" or "administrative/managerial") or non-professional ("white collar/clerical," "blue collar/craftsman," or "service/sale"). Additionally, I follow them in categorizing annual income as high if their income is at least \$100,000 and low if it is no more than \$40,000. Dhar and Zhu (2006) document that investor sophistication is negatively correlated with the disposition effect, so I test whether the portfolio-driven disposition effect holds for both samples, or if it disappears when separating investors based on their level of sophistication.

Table 2.8, Panel A shows tests of the four sub-samples of sophistication-related proxies: non-professional, professional, low income, and high income. Columns 1 and 4 confirm the Dhar and Zhu (2006) result without fixed effect controls. Indeed, the disposition effect is higher for non-professional (0.094%, t-stat 7.05) versus professional (0.082%, t-stat 7.93) investors, as well as low-income (0.091%, t-stat 8.49) versus high-income (0.073%, t-stat 7.02) investors. Columns 2 and 4 add account, stock, and date fixed effects controls and show the same pattern. In columns 3 and 6, I estimate equation (6) to determine the impact of these proxies on the portfolio-driven disposition effect. Here, I actually find that the portfolio's impact on the disposition effect, as denoted by the interaction coefficient, is actually economically and statistically stronger among professional (−0.25%, t-stat −13.64) versus non-professional (−0.21%, t-stat −7.67) investors. Therefore, this measure of sophistication does not seem to have any impact on the portfolio-driven disposition effect. Additionally, the portfolio-driven disposition effect is nearly identical among low-income (−0.25%, t-stat −14.53) and high-income (−0.25%, t-stat −11.63) investors. From these results, I conclude that the findings are not explained by the patterns documented by Dhar and Zhu (2006).

Investor sophistication is different than investor skill, so as a final approach, I decompose an investor's portfolio return based on their Daniel, Grinblatt, Titman, and Wermers (DGTW) performance.[30] More specifically, I decompose each investor's portfolio return into two components, one that is determined based on each stock's characteristic (size, book-to-market, and momentum),

[30] See Daniel, Grinblatt, Titman, and Wermers (1997).

and the other based on the stock's performance relative to its matched portfolio (where the matching is done on size, book-to-market, and momentum). The idea is that while highly skilled investors might be able to pick stocks that perform well relative to the stock's matched portfolio, it is unlikely that individual investors can predict the future performance of the market, HML, SMB, and MOM factors. By comparing the disposition effect among investors whose positive portfolio performance is driven by luck versus skill, I can examine the likelihood that portfolio gain is simply proxying for investor skill.

I match each stock-date to one of the 125 (5 x 5 x 5) DGTW member groups for each year using the benchmarks available on Russ Wermers' website.[31] Since the DGTW member groups are created on June 30 of each year, I match all account-stock observations in July-December to the same year and all account-stock observations in January-June to the previous year's member group. With some abuse of notation, I separate each account-stock-date's return into "alpha" and "beta," where beta represents the return (rather than a factor loading) of the corresponding DGTW portfolio and alpha equals the stock's return minus the matched portfolio. It trivially follows that any stock's cumulative return since the investor purchased it is simply the sum of its alpha and beta.

I define a variable, *Alpha,* to identify observations in which portfolio gain is driven by skill versus luck. Therefore, *Alpha* is defined as 1 if portfolio gain is generated only due to positive DGTW alpha (i.e., alpha > 0, beta ≤ 0) and 0 if portfolio gain is generated only due to positive DGTW beta (i.e., alpha ≤ 0, beta > 0). For all other observations, *Alpha* is defined as missing. Due to ambiguity, I omit the instances when both alpha and beta drive portfolio gain (alpha > 0, beta > 0).

Table 2.8, Panel B tests whether *Portfolio_Gain* is simply a proxy for skill using DGTW performance benchmarks. Column 1 shows the disposition effect for all observations with a DGTW

[31] The DGTW benchmarks are available via
http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm

identifier in which *Portfolio_Gain* equals 1 using no fixed effects controls (0.008%, t-stat 0.92). This column shows the main result: in simple univariate tests, the disposition effect is insignificant when the return of an investor's remaining holdings is strictly greater than zero. Column 2 adds account, stock, and date fixed effects controls (0.091%, t-stat 12.81). Column 3 then shows the same test as column 2 with the restriction that the investor's portfolio gain is entirely driven by skill (*Alpha* = 1), whereas Column 4 reports the results with the restriction that the investor's portfolio gain is entirely driven by luck (*Alpha* = 0). If *Portfolio_Gain* were simply proxying for unobserved investor skill, then there should be a smaller disposition effect once conditioning on portfolio gains that appeared to be the result of stock-picking alpha. In fact, the *Gain* coefficient increases to 0.130% (t-stat 7.09). Column 5 tests the difference between the coefficients reported in columns 3 and 4. I add the *Alpha* variable as well as an interaction term (*Gain*Alpha*) to the regression equation. Thus, the coefficient of the interaction term is the difference in disposition effects between skilled investors and lucky investors. This coefficient is actually positive although only statistically significant at the 10% level (0.025%, t-stat 1.89). This suggests that *Portfolio_Gain* is not simply proxying for investor skill because there is little difference in disposition effect when the source of the gain as coming from stock-picking skill or not, and the small difference that is observed is actually slightly positive.

In Table 2.9, I conduct the same tests as in Table 2.8, except on the sale conditioned sample. These tests produce the same qualitative results.

4.5. Utility over both Paper Gains/Losses and Realized Gains/Losses

According to standard expected utility theory, investors only derive utility from consumption. According to this view, a stock's return only affects an investor's utility through its effect on the investor's consumption. This standard theory has had difficulty explaining people's behavior in many settings, and an alternative theory (prospect theory) was developed that posits that people derive utility over gains and losses rather than over absolute wealth levels (Kahneman and Tversky 1979; Kahneman and Tversky 1992). Prospect theory is silent on whether people derive utility from paper gains or realized gains, and some models have been built on the assumption that investors derive utility from paper gains/losses, while others have been built on the assumption that they derive utility from realized gains/losses.[32] Frydman et al. (2014) conduct experiments of trade in an asset market, and they measure subjects' brain activity using functional magnetic resonance imaging. They find evidence that subjects' brains exhibit activity consistent with them receiving pleasure upon learning that their positions have increased in value, and they find that the effect is much stronger when subjects actually realize their gains, which is consistent with the predictions of realization utility.

This leads to another possible explanation for the portfolio-driven disposition effect: that investors derive utility from *both* paper (i.e., unrealized) gains/losses and realized gains/losses. The idea is the following. When an investor's portfolio is at a gain, she has received a lot of positive utility from the paper gains. The positive utility causes her to feel psychologically strong and hence more willing to realize a loss and take the resulting realization (dis)utility. Hence, there is less of a disposition effect in this scenario as she is willing to realize her losses. Conversely, if her portfolio is loss, she has received a lot of negative utility from the paper losses, which leaves her feeling psychologically fragile. In this scenario, she is loath to experience additional disutility by realizing a loss; rather, she is likely to realize a gain in order to reduce her disutility from her paper losses. It follows that there is a strong disposition effect when her portfolio is down.

[32] Because transaction costs are generally small, especially at discount brokerages, the distinction between paper gains/losses and realized gains/losses should be irrelevant, because investors can easily convert their paper gains/losses to realized gains/losses without incurring any significant costs. However, economists have argued that realized losses are more painful than paper losses (Thaler, 1999), and they have shown that investors' risk tolerance is differentially affected by paper losses and realized losses (Imas, 2016). In other words, investors do seem to distinguish between paper gains/losses and realized gains/losses, even though it is unclear why.

To develop a testable prediction of this explanation, I consider what investors do once they sell their stock: do they keep it in cash or do they reinvest it in a different stock? Frydman, Hartzmark, and Solomon (2018) provide strong evidence that people do not "close" their mental accounts when they liquidate a stock and reinvest the proceeds into a new stock; rather, they continue to use the amount they invested in the initial stock as a reference point when deciding whether or not to liquidate their position in the new stock. According to this view, investors should be less likely to receive a burst of realization utility whenever they sell shares at a gain and reinvest the proceeds into a new position; rather, the bursts of realization utility should occur when investors realize a gain and "close" the mental account by not reinvesting the proceeds into a new stock. Hence, if the results are driven by investors receiving utility over both paper gains/losses and realized gains/losses, then investors should be unlikely to invest in a different stock whenever they sell a stock at a gain and their portfolio is at a loss; keeping their mental account open in this way would prevent them from receiving the burst of positive realization utility from realizing the gain.[33]

To test this, I take the sample of account-days in which the investor sells exactly one stock. The dependent variable is a dummy for whether or not she purchases shares of a different stock (*reinvest dummy*). The independent variables of interest are the four dummies representing the possible scenarios for whether the stock that she sold was at a gain or a loss and whether her portfolio was at a gain or a loss at the time she sold the stock. I predict that investors should be unlikely to reinvest whenever the stock that they sold was at a gain and their portfolio was at a loss.

I report the results of this test in Table 2.10. The variable *Loss_Gain* takes the value of one if the stock sold is at a loss and the remaining portfolio is at a gain. The same naming convention follows for the other independent variables. The variable *Gain_Loss* is omitted. Thus, each coefficient is

[33] In contrast, if portfolio rebalancing (discussed in Section 4.2) explains the portfolio-driven disposition effect, investors should be more likely to reinvest when they sell a stock at a gain and the rest of their portfolio is at a loss.

interpreted as the difference in reinvestment probability from the case in which the stock is at a gain and the remaining portfolio is at a loss.

In Table 2.10, because all coefficients are positive and statistically significant well below the 1% level, investors selling a gain when the rest of their portfolio is at a loss are most likely to keep those gains in cash over the next two trading days. These results are consistent with the idea that investors are eager to realize gains whenever their portfolio is at a loss, and when they do so, they refrain from reinvesting the proceeds because they want to close the mental account (and lock in the realized gain).

5. Conclusion

I document a new stylized fact termed the portfolio-driven disposition effect: the disposition effect is concentrated in scenarios which the investor's remaining portfolio is performing poorly. When an investor's portfolio is performing well, the disposition effect is almost non-existent. The effect is robust to a wide variety of controls. I explore several possible explanations for the effect, and the one that is most consistent with the data is that investors derive utility from both paper gains/losses and realized gains/losses. When an investor has disutility from unrealized losses, she takes utility by realizing gains.

One way to think about this finding is that investors treat unrealized gains like an in-the-money "utility option" that they can exercise at a time which is most valuable to them, i.e. when they are experiencing disutility somewhere else. While this study focuses on individual stocks in a portfolio, of interest is the generality of this phenomenon. For example, does an investor's choice to sell stocks or bonds depend on her unrealized gains or losses in another asset class such as housing? Or could it also depend on disutility from non-financial sources such as health or well-being? I find these questions of interest for future research.

Table 2.1: Individual Investor Summary Statistics

This table presents summary statistics for the two datasets created from the individual trading data from January 1991 to November 1996 (Barber and Odean, 2000). Panel A represents all account-stock-days in which a position is held while Panel B adds the condition that a sale occurred on a given account-day. I define gains as strictly greater than zero while losses include zeros.

PANEL A: Unconditional Data				Stock Returns				Portfolio Returns			
	N	Sell Obs	% Sell	Mean	10%	Median	90%	Mean	10%	Median	90%
All Account-Stock-Dates	**132,262,250**	**340,674**	**0.26%**	**0.15**	**-0.34**	**0.04**	**0.68**				
with 1 stock	29,987,104	71,133	0.24%	0.12	-0.38	0.02	0.62				
Stock at a Gain	15,686,914	47,821	0.30%	0.43	0.03	0.21	1.00				
Stock at a Loss	14,300,190	23,312	0.16%	-0.23	-0.53	-0.17	-0.02				
with 2+ stocks	102,275,146	269,541	0.26%	0.16	-0.32	0.04	0.69	0.15	-0.20	0.07	0.56
Stock at a Gain	58,777,857	172,819	0.29%	0.44	0.03	0.23	1.02	0.19	-0.16	0.11	0.62
Stock at a Loss	43,497,289	96,722	0.22%	-0.21	-0.50	-0.15	-0.02	0.09	-0.25	0.03	0.46
Portoflio at a Gain	65,786,903	156,887	0.24%	0.22	-0.28	0.08	0.80	0.32	0.03	0.19	0.72
Portfolio at a Loss	36,488,243	112,654	0.31%	0.06	-0.38	-0.01	0.48	-0.16	-0.37	-0.11	-0.02
Accounts	75,054										
Account-Days	57,358,574										

PANEL B: Sale Conditioned Data				Stock Returns				Portfolio Returns			
	N	Sell Obs	% Sell	Mean	10%	Median	90%	Mean	10%	Median	90%
All Account-Stock-Dates	**1,442,197**	**340,674**	**24%**	**0.12**	**-0.28**	**0.03**	**0.54**				
with 1 stock	71,133	71,133	100%	0.13	-0.24	0.07	0.49				
Stock at a Gain	47,821	47,821	100%	0.29	0.03	0.15	0.64				
Stock at a Loss	23,312	23,312	100%	-0.21	-0.48	-0.13	-0.02				
with 2+ stocks	1,371,064	269,541	20%	0.12	-0.28	0.03	0.54	0.11	-0.14	0.06	0.39
Stock at a Gain	777,037	172,819	22%	0.36	0.03	0.17	0.82	0.13	-0.11	0.09	0.43
Stock at a Loss	594,027	96,722	16%	-0.19	-0.45	-0.13	-0.02	0.07	-0.17	0.03	0.33
Portoflio at a Gain	910,019	156,887	17%	0.17	-0.25	0.05	0.63	0.22	0.02	0.14	0.49
Portfolio at a Loss	461,045	112,654	24%	0.03	-0.34	-0.01	0.35	-0.12	-0.27	-0.08	-0.01
Accounts	52,044										
Account-Days	295,763										

Table 2.2: The Unconditional Portfolio-Driven Disposition Effect Regressions

I report the results of various regressions on the sample of 102,275,146 account-stock-day triples such that the account owns shares of at least two different common stocks on the given day. After controlling for account, stock, day fixed effects, the sample has 102,275,125 observations. The dependent variable, *Sale*, is a dummy variable representing whether the investor sold any shares of the given stock on the given date. The variable *Gain* represents whether the investor's position in the stock is at a gain on the given date. Similarly, *Portfolio_Gain* represents whether the rest of the investor's portfolio (excluding the stock under consideration) is at a gain on the given date. I report cluster-robust t-stats in parentheses. All 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)	(5)
Gain	0.00200***	0.00193***	0.00321***	0.00239***	0.00336***
	(16.04)	(15.95)	(21.84)	(19.85)	(22.73)
Portfolio_Gain	0.00016***	0.00009*	0.00183***	0.00024***	0.00170***
	(3.22)	(1.75)	(19.50)	(5.02)	(18.86)
Gain * Portfolio_Gain	-0.00185***	-0.00184***	-0.00253***	-0.00183***	-0.00247***
	(-20.75)	(-20.67)	(-20.56)	(-21.43)	(-20.50)
Observations	102,275,125	102,275,125	102,275,125	102,275,125	102,275,125
R-squared	0.000	0.001	0.009	0.001	0.010
Date FE	No	Yes	No	No	Yes
Account FE	No	No	Yes	No	Yes
Stock FE	No	No	No	Yes	Yes

Table 2.3: The Sale-Conditioned Portfolio-Driven Disposition Effect Regressions

I report the same analysis as Table 2.2 with the condition that I only include account-days in which a sale occurred. This restriction reduces the sample to 1,371,064 account-stock-day triples. After controlling for account, stock, day fixed effects, the sample has 1,370,869 observations. I report cluster-robust t-stats in parentheses. All 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)	(5)
Gain	0.161***	0.160***	0.147***	0.171***	0.155***
	(25.81)	(26.38)	(28.56)	(29.26)	(32.23)
Portfolio_Gain	-0.008	-0.005	0.009***	-0.005	0.011***
	(-1.43)	(-0.89)	(3.22)	(-0.98)	(3.70)
Gain * Portfolio_Gain	-0.140***	-0.137***	-0.119***	-0.135***	-0.118***
	(-23.63)	(-24.20)	(-26.40)	(-25.49)	(-27.82)
Observations	1,370,869	1,370,869	1,370,869	1,370,869	1,370,869
R-squared	0.022	0.030	0.136	0.043	0.155
Date FE	No	Yes	No	No	Yes
Account FE	No	No	Yes	No	Yes
Stock FE	No	No	No	Yes	Yes

Table 2.4: Extreme versus Non-Extreme Stocks in the Investor's Portfolio

I report the same regression as column 5 of Tables 2.2 and 2.3 with various sample restrictions. I identify a stock as "Extreme" if its cumulative return since it was purchased is the best or worst in the given investor's portfolio. Columns 1-3 report regressions on the unconditional sample, whereas columns 4-6 report regressions on the sale conditioned sample. Columns 1 and 4 are the base case reported in column 5 of Tables 2.2 and 2.3. Columns 2 and 5 report regression coefficients when the sample is restricted to the extreme stocks in the investor's portfolio, while columns 3 and 6 restrict attention to the non-extreme stocks in the investor's portfolio. I report cluster-robust t-stats in parentheses. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

	Unconditional Sample			Sale Conditioned Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Sale	Base	Extreme	Non-Extreme	Base	Extreme	Non-Extreme
Gain	0.00336***	0.00389***	0.00289***	0.155***	0.181***	0.094***
	(22.73)	(24.64)	(15.57)	(32.23)	(27.50)	(19.42)
Portfolio_Gain	0.00170***	0.00203***	0.00131***	0.011***	-0.036***	0.020***
	(18.86)	(19.16)	(13.92)	(3.70)	(-7.91)	(7.74)
Gain * Portfolio_Gain	-0.00247***	-0.00269***	-0.00220***	-0.118***	-0.089***	-0.067***
	(-20.50)	(-21.93)	(-13.05)	(-27.82)	(-17.74)	(-14.17)
Observations	102,275,125	54,743,365	47,531,618	1,370,869	448,902	916,834
R-squared	0.010	0.012	0.010	0.155	0.184	0.110
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.5: Regressions using Complete Liquidations

I report the same analysis as Tables 2.2 and 2.3 except with *Full_Sale* as the dependent variable instead of *Sale*. I define *Full_Sale* to be equal to one if an entire position is sold and zero otherwise. This definition does not identify partial sales like the original *Sale* variable. Thus, *Full_Sale* only identifies complete liquidations. I report cluster-robust t-stats in parentheses. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Panel A: Unconditional

Dependent Variable: Full_Sale	(1)	(2)	(3)	(4)	(5)
Gain	0.00173***	0.00168***	0.00278***	0.00206***	0.00292***
	(16.13)	(16.09)	(21.85)	(19.81)	(22.84)
Portfolio_Gain	0.00021***	0.00016***	0.00166***	0.00026***	0.00156***
	(5.01)	(3.90)	(19.42)	(6.82)	(18.64)
Gain * Portfolio_Gain	-0.00170***	-0.00168***	-0.00230***	-0.00168***	-0.00224***
	(-20.96)	(-20.85)	(-20.72)	(-21.59)	(-20.65)
Observations	102,275,125	102,275,125	102,275,125	102,275,125	102,275,125
R-squared	0.000	0.001	0.008	0.001	0.008
Date FE	No	Yes	No	No	Yes
Account FE	No	No	Yes	No	Yes
Stock FE	No	No	No	Yes	Yes

Panel B: Sale Conditioned

Dependent Variable: Full_Sale	(1)	(2)	(3)	(4)	(5)
Gain	0.139***	0.138***	0.128***	0.148***	0.136***
	(24.78)	(25.43)	(27.95)	(28.21)	(31.73)
Portfolio_Gain	-0.001	0.002	0.016***	0.001	0.017***
	(-0.26)	(0.44)	(5.67)	(0.30)	(6.13)
Gain * Portfolio_Gain	-0.128***	-0.126***	-0.110***	-0.124***	-0.109***
	(-24.23)	(-24.89)	(-27.30)	(-26.06)	(-28.58)
Observations	1,370,869	1,370,869	1,370,869	1,370,869	1,370,869
R-squared	0.018	0.028	0.140	0.037	0.158
Date FE	No	Yes	No	No	Yes
Account FE	No	No	Yes	No	Yes
Stock FE	No	No	No	Yes	Yes

Table 2.6: Two Sell Residuals – A Test of Simultaneous Selling Independence

I report the average residuals according to the simple model described in equations (7)-(9). The sample is restricted to account-days in which the investor owns at least three stocks and sells exactly two stocks. I calculate predicted probabilities that an investor sells two gains, one gain and one loss, or two losses based on equations (7)-(9). Next, I define residuals as the dummy for whether the investor actually sold two gains (or 1 gain and 1 loss, or 2 losses) minus the predicted probability of that event. Finally, I average those residuals across all observations. If an investor's choice is truly independent, then all residuals should be insignificant from zero. I report cluster-robust t-stats. All standard errors are clustered by account, following the procedure of Rogers (1993).

	Residual Statistics		
Two Sell Scenarios	Mean	t-stat	Observations
Sell 2 Gains	0.102***	18.92	18,862
Sell 1 Gain and 1 Loss	-0.129***	-32.89	18,862
Sell 2 Losses	0.027***	5.73	18,862

Table 2.7: Non-Simultaneous Sales

I report the same regression as column 5 of Tables 2.2 and 2.3 with various sample restrictions. Columns 1 and 2 report regressions on the unconditional sample, while columns 3 and 4 report regressions on the sale conditioned sample. Columns 1 and 3 restrict attention to account-days in which the investor makes at most one sale transaction on the given date, while columns 2 and 4 restrict attention to account-days in which the investor makes at most one sale transaction in the five-day window centered on the given date. I report cluster-robust t-stats in parentheses. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

	Unconditional Sample		Sale Conditioned Sample	
	(1)	(2)	(3)	(4)
Dependent Variable: Sale	≤1 Trans	≤1Trans, 5 day window	≤1 Trans	≤1Trans, 5 day window
Gain	0.00292***	0.00242***	0.177***	0.188***
	(25.75)	(26.93)	(38.36)	(40.34)
Portfolio_Gain	0.00133***	0.00108***	0.008***	0.006**
	(20.84)	(20.77)	(2.69)	(2.14)
Gain * Portfolio_Gain	-0.00212***	-0.00175***	-0.130***	-0.137***
	(-24.48)	(-25.97)	(-33.90)	(-36.34)
Observations	101,890,663	101,514,366	1,036,755	835,049
R-squared	0.007	0.005	0.148	0.150
Date FE	Yes	Yes	Yes	Yes
Account FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes

Table 2.8: Sophisticated/Skilled Investors (Unconditional Sample)

Panel A reports tests split by non-professional, professional, low-income and high-income investors. I define these sub-samples consistent with Dhar and Zhu (2006). Professional investors include those classified as "professional/technical" or "administrative/managerial". Non-professional investors include those classified as "white collar/clerical," "blue collar/craftsman," or "service/sale". High-income investors have an annual income of at least $100,000. Low-income investors have an annual income no greater than $40,000. Panel B, columns 1 and 2 report the disposition effect when *Portfolio_Gain* equals 1 on all observations with a DGTW identifier. Columns 3 and 4 report the same test as column 2 for the sub-samples in which *Alpha* equals 1 and 0, respectively. *Alpha* is defined as 1 if portfolio gain is generated only due to positive DGTW alpha (alpha > 0, beta ≤ 0), 0 if *Portfolio_Gain* is generated only due to positive DGTW beta (alpha ≤ 0, beta > 0), and missing otherwise. In column 5, the interaction term tests the difference between the coefficients reported in columns 3 and 4. I report cluster-robust t-stats in parentheses. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Panel A: Sophistication Proxies (Employment and Income)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Sale		Non-Professional			Professional	
Gain	0.00094***	0.00223***	0.00349***	0.00082***	0.00198***	0.00355***
	(7.05)	(11.02)	(11.35)	(7.93)	(15.14)	(16.20)
Portfolio_Gain			0.00145***			0.00173***
			(7.84)			(13.26)
Gain * Portfolio_Gain			-0.00208***			-0.00250***
			(-7.67)			(-13.64)
Observations	3,808,775	3,808,766	3,808,766	20,106,059	20,106,054	20,106,054
R-squared	0.000	0.012	0.012	0.000	0.009	0.009
Stock FE	No	Yes	Yes	No	Yes	Yes
Date FE	No	Yes	Yes	No	Yes	Yes
Account FE	No	Yes	Yes	No	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Sale		Low Income			High Income	
Gain	0.00091***	0.00206***	0.00364***	0.00073***	0.00191***	0.00350***
	(8.49)	(16.29)	(17.62)	(7.02)	(14.44)	(14.58)
Portfolio_Gain			0.00176***			0.00169***
			(14.91)			(11.34)
Gain * Portfolio_Gain			-0.00254***			-0.00250***
			(-14.53)			(-11.63)
Observations	27,276,108	27,276,100	27,276,100	15,635,783	15,635,778	15,635,778
R-squared	0.000	0.010	0.010	0.000	0.010	0.010
Stock FE	No	Yes	Yes	No	Yes	Yes
Date FE	No	Yes	Yes	No	Yes	Yes
Account FE	No	Yes	Yes	No	Yes	Yes

Panel B: DGTW Breakout

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Sale	Portfolio_Gain = 1		Alpha=1	Alpha=0	
Gain	0.00008	0.00091***	0.00130***	0.00120***	0.00119***
	(0.92)	(12.81)	(7.09)	(14.53)	(14.56)
Alpha					-0.00060***
					(-5.72)
Gain*Alpha					0.00025*
					(1.89)
Observations	57,224,454	57,224,187	1,867,281	29,473,110	31,340,391
R-squared	0.000	0.010	0.051	0.012	0.012
Stock FE	No	Yes	Yes	Yes	Yes
Date FE	No	Yes	Yes	Yes	Yes
Account FE	No	Yes	Yes	Yes	Yes

Table 2.9: Sophisticated/Skilled Investors (Sale Conditioned Sample)

This table reports the same tests as Table 2.8 for the sale conditioned sample. Panel A reports tests split by non-professional, professional, low-income and high-income investors. I define these sub-samples consistent with Dhar and Zhu (2006). Professional investors include those classified as "professional/technical" or "administrative/managerial". Non-professional investors include those classified as "white collar/clerical," "blue collar/craftsman," or "service/sale". High-income investors have an annual income of at least $100,000. Low-income investors have an annual income no greater than $40,000. Panel B, columns 1 and 2 report the disposition effect when *Portfolio_Gain* equals 1 on all observations with a DGTW identifier. Columns 3 and 4 report the same test as column 2 for the sub-samples in which *Alpha* equals 1 and 0, respectively. *Alpha* is defined as 1 if *Portfolio_Gain* is generated only due to positive DGTW alpha (alpha > 0, beta ≤ 0), 0 if *Portfolio_Gain* is generated only due to positive DGTW beta (alpha ≤ 0, beta > 0), and missing otherwise. In column 5, the interaction term tests the difference between the coefficients reported in columns 3 and 4. I report cluster-robust t-stats in parentheses. All standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Panel A: Sophistication Proxies (Employment and Income)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Sale		Non-Professional			Professional	
Gain	0.094***	0.134***	0.198***	0.068***	0.094***	0.167***
	(7.03)	(9.83)	(12.00)	(7.75)	(13.13)	(16.36)
Portfolio_Gain			-0.016*			0.007
			(-1.65)			(1.29)
Gain * Portfolio_Gain			-0.115***			-0.117***
			(-8.04)			(-12.15)
Observations	39,275	38,987	38,987	253,112	252,904	252,904
R-squared	0.012	0.215	0.222	0.007	0.166	0.172
Stock FE	No	Yes	Yes	No	Yes	Yes
Date FE	No	Yes	Yes	No	Yes	Yes
Account FE	No	Yes	Yes	No	Yes	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Sale		Low Income			High Income	
Gain	0.070***	0.090***	0.161***	0.068***	0.096***	0.165***
	(9.01)	(14.57)	(21.19)	(8.29)	(13.98)	(15.15)
Portfolio_Gain			0.004			0.009
			(0.93)			(1.47)
Gain * Portfolio_Gain			-0.114***			-0.115***
			(-16.53)			(-10.99)
Observations	378,143	377,954	377,954	190,629	190,409	190,409
R-squared	0.008	0.160	0.166	0.007	0.162	0.168
Stock FE	No	Yes	Yes	No	Yes	Yes
Date FE	No	Yes	Yes	No	Yes	Yes
Account FE	No	Yes	Yes	No	Yes	Yes

Panel B: DGTW Breakout

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Sale	Portfolio_Gain = 1		Alpha=1	Alpha=0	
Gain	0.019***	0.032***	0.042***	0.044***	0.044***
	(4.70)	(9.68)	(3.67)	(12.91)	(12.95)
Alpha					0.007
					(0.93)
Gain*Alpha					0.016
					(1.52)
Observations	778,218	772,643	17,257	418,297	435,554
R-squared	0.001	0.155	0.440	0.162	0.162
Stock FE	No	Yes	Yes	Yes	Yes
Date FE	No	Yes	Yes	Yes	Yes
Account FE	No	Yes	Yes	Yes	Yes

Table 2.10: Reinvestment Probabilities within Two Days of Sale

I report the difference in probabilities of reinvesting cash from a sale based on stock and portfolio performance. The dependent variable is *Reinvest Dummy* which takes the value of one if the investor makes a stock purchase different from the stock that was sold within two days of the original sale and zero otherwise. The variable *Loss_Gain* is one if the stock sold is at a loss and the remaining portfolio is at a gain. The same convention follows for the other independent variables. The variable *Gain_Loss* is omitted. Thus, the coefficients are interpreted as the difference in probability from the *Gain_Loss* scenario. I restrict attention to account-days in which exactly one sale occurs to avoid ambiguity. Standard errors are clustered by account, day, and stock, following the procedure of Cameron, Gelbach, and Miller (2011).

Dependent Variable: Reinvest Dummy	(1)	(2)	(3)	(4)	(5)
Loss_Gain	0.108***	0.106***	0.067***	0.105***	0.061***
	(14.40)	(14.79)	(15.04)	(15.14)	(13.78)
Loss_Loss	0.068***	0.075***	0.017***	0.069***	0.025***
	(11.85)	(13.68)	(3.96)	(12.81)	(5.96)
Gain_Gain	0.030***	0.021***	0.032***	0.028***	0.019***
	(5.27)	(3.59)	(8.92)	(5.12)	(5.48)
Observations	183,084	183,084	183,084	183,084	183,084
R-squared	0.006	0.025	0.297	0.031	0.326
Date FE	No	Yes	No	No	Yes
Account FE	No	No	Yes	No	Yes
Stock FE	No	No	No	Yes	Yes

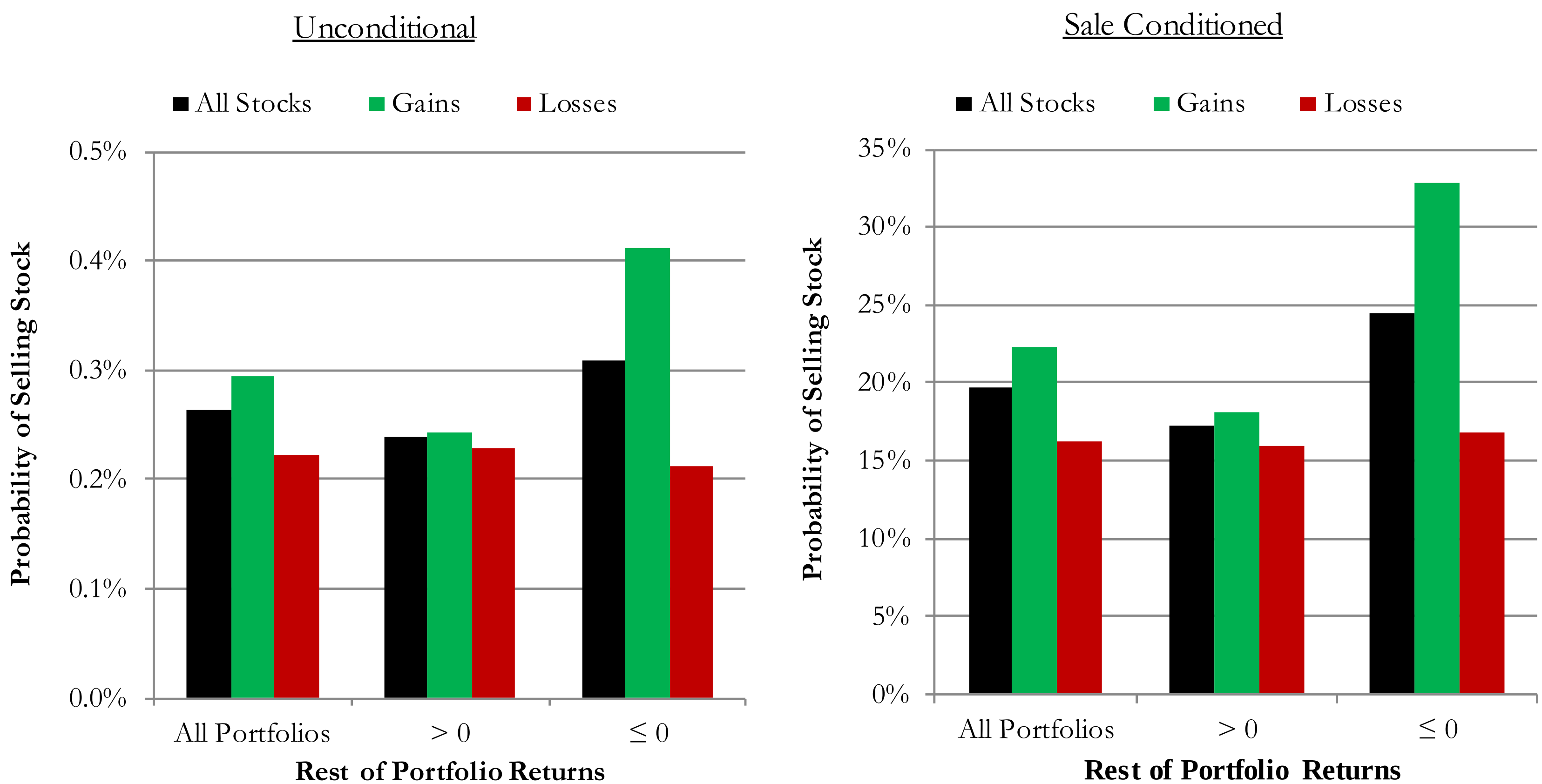
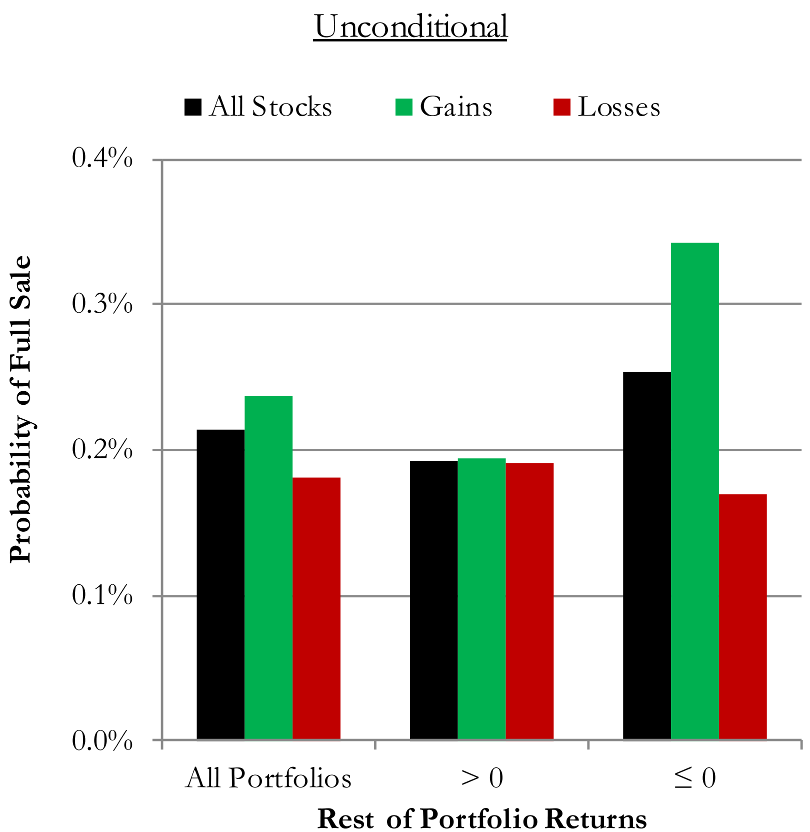


Figure 2.1: The Disposition Effect and Portfolio Performance

I report the probability of selling a stock (including partial sales) based on the stock's performance (gain versus loss) from the date the investor purchased the stock and the performance of the rest of the investor's portfolio (excluding the stock under consideration). I report the unconditional probabilities (left) and conditioning on a sale taking place (right). The unconditional results have 102,275,146 observations (57% stock gains, 43% stock losses; 64% portfolio gains, 36% portfolio losses). The conditional results have 1,371,064 observations (57% stock gains, 43% stock losses; 66% portfolio gains, 34% portfolio losses). I define gains as strictly greater than zero while losses include zeros.



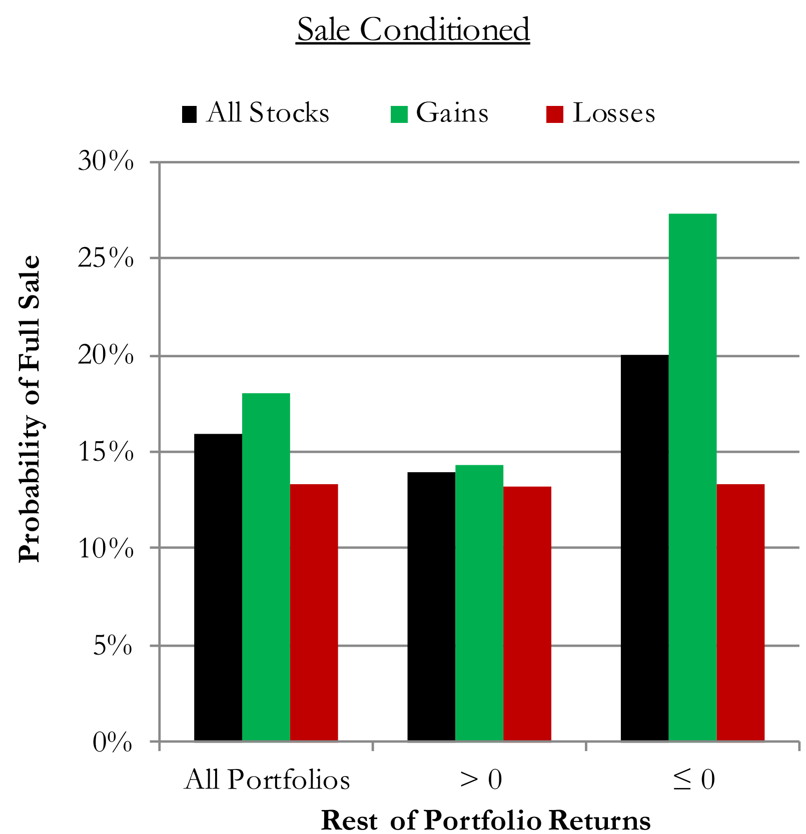


Figure 2.2: Complete Liquidations

I report the same graphs as in Figure 2.1 except that here I analyze only full sales, or complete liquidations, instead of including partial sales. These graphs show the probability of a complete liquidation based on the stock's performance (gain versus loss) from the date the investor purchased the stock and the performance of the rest of the investor's portfolio (excluding the stock under consideration). I report the unconditional probabilities (left) and conditioning on a sale taking place (right). The unconditional results have 102,275,146 observations (57% stock gains, 43% stock losses; 64% portfolio gains, 36% portfolio losses). The conditional results have 1,371,064 observations (57% stock gains, 43% stock losses; 66% portfolio gains, 34% portfolio losses). I define gains as strictly greater than zero while losses include zeros.

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APPENDIX

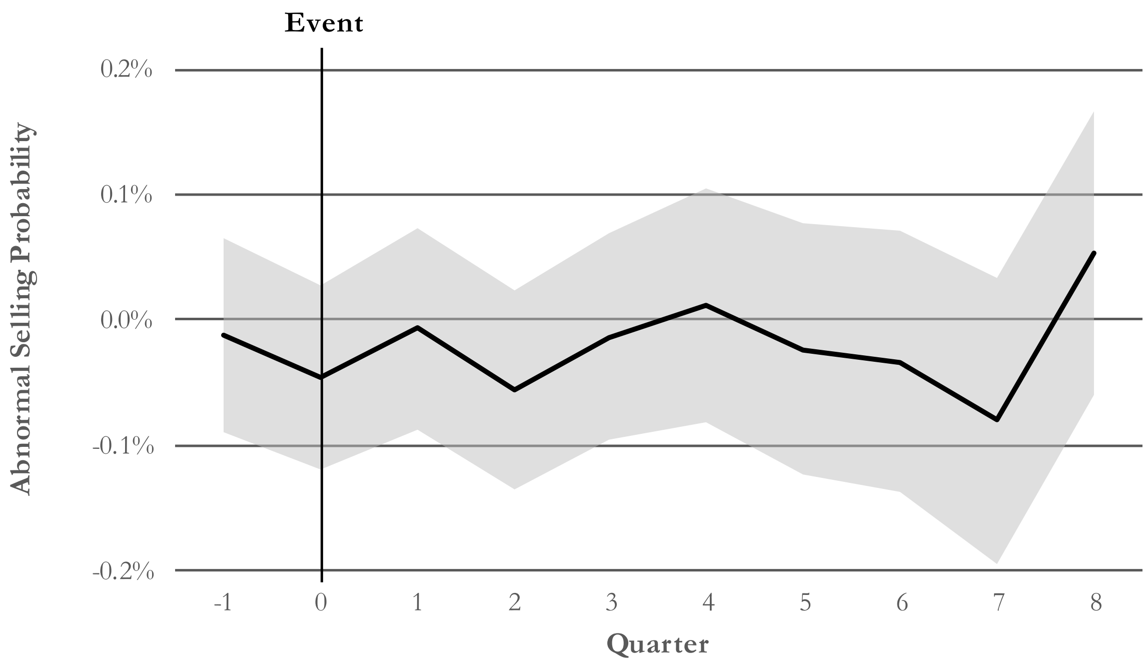

Figure A.1: Unconditional Abnormal Selling around Natural Disasters Robustness

This figure displays the unconditional abnormal selling probabilities at the county-quarter level. The black line represents the β coefficients from the estimate of equation (3) with 95% confidence intervals shaded in grey. Natural disasters events are those that inflict *any* level of damage in a given county (also described as the moderate, severe, and extreme cohorts in Essay 1, Section 4.2).

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 disaster severity. The dependent variable, *Sale*, is defined to be one if a sale (including partial sales) occurs for the 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, 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: 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.4: 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 1.3 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.5: 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.6: 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